A History of Psycholinguistics
The Pre-Chomskyan Era

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Dedication

To Claartje, dear daughter
and psycholinguist

Preface

I have developed the habit of collecting statements about the age of psycholinguistics. They abound in textbooks, their promotion materials, reviews, and anthologies. Here are some: "With psycholinguistics in its fifth decade of existence," "Psycholinguistics began with attempts to test the empirical validity of various formal linguistic concepts," "Psycholinguistics is a young and fast changing science," "The young science of psycholinguistics," "For about four decades following its emergence around 1950, psycholinguistics . . .," and so on.

It is, indeed, the default view among us psycholinguists and our students that our science took off during the 1950s. It all began with the 1951 Summer Seminar in Psychology and Linguistics at Cornell University, held under the auspices of the Social Science Research Council, and the subsequent 1953 seminar at Indiana University. They led to *Psycholinguistics. A survey of theory and research problems*, so ably edited by Charles Osgood and Thomas Sebeok. It became the "charter" of the new discipline. Then, by the end of the 1950s, there was the "cognitive revolution." The cooperation between George Miller and Noam Chomsky transformed the young science into a mentalistic "generative" psycholinguistics, initially testing the "psychological reality" of transformational grammar, then by and by unfolding into our present science.

Every people cherishes its own myth of origin and so do we psycholinguists. But evolution theory has taught us otherwise. There has been history before history. This book reviews that "prehistory." Psycholinguistics used to be called the psychology of language. Its empirical roots go back to the late eighteenth century. It was an established discipline a century later and it was a booming science during the first half of the twentieth century. Its roots are tripartite. Psychology (both experimental and developmental) and linguistics are inseparably unified in our modern designation. There is, in addition, the study of language in the brain, which has always figured in the psychology of language. We will trace their mutual interactions and sketch how they came to travel together.

The book opens in the year 1951, our historical common ground. From that starting point we dive into the real origins of our empirical science. Chapters 2 to 6 move us from pioneers such as Tiedemann, Gall, Humboldt, and Steinthal, to the year 1900, when Wundt published his monumental two-volume *Psychologie der Sprache.* Chapters 7 to 13 review the twentieth century developments, both theoretical and empirical, up to the "cognitive revolution." Chapter 14 is explicitly devoted to the tragic repercussions during the Third Reich, which drastically reshaped the world map of psycholinguistics. Chapter 15, finally, returns to the 1950s, sketching an optimistic interdisciplinary ambience, which would, by the end of the decade, become the cradle of the Chomskyan revolution in psycholinguistics. But there we halt. The book does not deal with that revolution, and

I have drawn a sharp line. Skinner's *Verbal behavior* of 1957 is covered in this book, Chomsky's (1959) review of it is not. That review was the declaration of war which sparked the "revolution." The new paradigm and its state after some 15 years I have already treated in great detail in my *Formal grammars in linguistics and psycholinguistics*, which reappeared in 2008.

In short, this book tells the history of "pre-Chomskyan" psycholinguistics, "psycholinguistics BC" for short. It is a history both of ideas and of theory, and one of empirical exploration and methodology. Last but not least, it is a personal history of the men and women whose intelligence, brilliant insights, fads, fallacies, cooperations, and rivalries have created our discipline. The book is entirely based on consulting the original sources. Many of them had hardly ever left their libraries; some could only be consulted in darkish rooms, laid out on soft cushions (the books I mean). I was, of course, often inspired and informed by secondary sources, to which I make due reference. But afterwards, I always went *ad fontes.*

The "roots" approach makes it possible for different categories of readers to select the chapters relevant to them. Child language students will find a comprehensive, self-contained history of their field by reading Chapters 4 and 10. Aphasiologists and neuroscientists of language are similarly served by reading Chapters 3 and 11. Experimental psycholinguists get the full historical picture from Chapters 5 and 12. The linguistic historical perspective, finally, is covered by Chapters 2, 7, 9, and 13.

Acknowledgments

The solo venture of writing a monograph turned out, after all, to be team work in many respects. Finding the sources became a huge exercise. On average, two books and many more papers went through my hands every week over the five-year writing period. This flood was professionally channeled by the helpful librarians of my Max Planck Institute in Nijmegen, Karin Kastens (head of the library), Meggie Uijen, and Annemieke Sweere. My dedicated secretary of 15 years, Evelyn Giering, also managed to retrieve the most unlikely documents and historical information. Another excellent source of historical books has been the Amsterdam University Library, just five minutes from my home in the center of Amsterdam. I also gratefully acknowledge access to the Boas and the Köhler archives in the library of the American Philosophical Society, and access to the information on Kainz in the archives of the Austrian Academy of Sciences and of the University of Vienna.

Many friends and colleagues provided me with solicited and unsolicited advice during writing and some did not even know they were sources of inspiration for me. Clemens Knobloch, for instance, has in many respects been ahead of me in his thorough monographs on the German history of language sciences. And so was Arthur Blumenthal on Wilhelm Wundt's psycholinguistics. In fact, he was already working on the early history of psycholinguistics when we were both postdocs at Harvard's Center for Cognitive Studies, long ago. Magdalena Smoczynska put me on the trail of her brilliant countryman Baudouin de Courtenay. She also helped me on Gvozdev's work in phonologogical development. My Max Planck colleague and friend Wolfgang Klein time and again brought me, even gave me, wonderful first editions of historical sources from his own gigantic library. Without Achim Eschbach, I could not have written my texts on Karl and Charlotte Bühler. Achim gave me access to all relevant documents in the Bühler archive, which is under his trust. Even more important was his personal tutoring of me on the Bühlers' work and life. His colleague at Essen University, Walter Schmitz, has been my main source of information on the Dutch Significa movement.

During the end phase of writing, five dear colleagues helped me by reviewing the whole draft manuscript or parts thereof. Wolfgang Klein from my own institute worked through the whole text and offered me detailed comments. Dan Slobin did a thorough job on the developmental chapters and in addition opened many windows for me on Russian historical sources. He also reviewed the chapter on linguistic relativity, as did my dear Max Planck colleague Stephen Levinson. Simon Garrod reviewed two chapters during his busy sabbatical year in Perth, Australia. But the main contribution during this phase came from Asifa Majid, also from my own institute. It would be a violation of Grice's maxim of quantity to say that she read the whole manuscript. Rather she read every chapter, every

paragraph, sentence, word, letter, and punctuation mark, correcting my less than idiomatic English, spotting errors, and making suggestions for improvements all over the place. The dedication of all these critical minds has pleased and touched me.

The final production of the book was again real team work. I am grateful to the competent and dedicated OUP staff with whom I had the pleasure to cooperate. I want to thank in particular Abigail Stanley, who edited the production of this complicated book with foresight, flexibility, and precision. I am also much obliged to tenacious, multilingual expert Miranda Bethell, who worked wonders on the page proofs.

My best team worker, however, has as always been Elisabeth. Thank you, Els.

Further acknowledgement

Several readers have been so kind as to send me comments and to attend me to factual errors, omission, misprints and typo's in the hardcover edition of this book. I have gratefully adapted and corrected the text wherever possible. I acknowledge in particular helpful comments by Zachay Brooks, Marc Brysbaert, Noam Chomsky, Samuel Fillenbaum, Evelyn Giering, John Goldsmith, Sieb Nooteboom, Phaeda Royle, Gunter Senft and Pieter Seuren.

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Part 1

Orientation

The term "psycholinguistic" was introduced in 1936 by Jacob Kantor, but it was rarely used until 1946, when his student Nicholas Pronko published his article *Language and psycholinguistics: A review*. The review covers a broad and diverse set of approaches to language phenomena, which roughly share the fact that they focus on "the essential psychological features of linguistic happenings." These approaches include experimental, statistical, and phonetic ones. They also include studies of language acquisition, language abilities, gestural language, aphasia, and more. Pronko was well aware of the history of such approaches to language, but also of their theoretical diversity. His aim was to provide a unifying theoretical framework and he offered Kantor's "interbehavioral language theory" as the optimal solution. That behavioristic framework is discussed in Chapter 8. Of relevance here is that the term "psycholinguistics" was here, for the first time, used to denote an interdisciplinary field of study that could be theoretically coherent. The term soon became widely accepted, not least because of its programmatic challenge: to create a new, unified approach to human linguistic communication. Chapter 1 sketches how this challenge was realized in the early 1950s.

Psycholinguistics was established as a discipline, coherent in both method and theory. However, psycholinguistics was nothing other than what was called "the psychology of language" from the end of the nineteenth century. It then also enjoyed a certain theoretical coherence, as a mentalistic science. That science was the outcome of a full century of empirical and theoretical research in comparative linguistics, developmental and experimental psychology, brain anatomy, and pathology. Theoretical frameworks came and went, and this was to continue till the present day. It is a characteristic of any viable science.

The framework of the early 1950s introduced a booming decade in psycholinguistics, to which we return in the very last chapter of this book. The initial and final chapters sandwich almost two centuries of historical development in the study of language use in mind, brain, and behavior. The theoretical coherence of the early 1950s was, in its turn, soon to dissolve in a new paradigm shift, to a "generative" psycholinguistics. That, however, is beyond the scope of this book.

Chapter 1

1951

It is a widely shared opinion that the new discipline of psycholinguistics emerged during the 1950s. Nothing could be further from the truth. The purpose of this book is to sketch a history of psycholinguistics, the psychology of language, going back to the end of the eighteenth century, when empirical research began in earnest. Still, it makes sense to begin where psycholinguistics is generally believed to have started, in the early 1950s and more precisely in 1951. That is, to some extent, familiar ground. We will then time travel, over almost two centuries, from the early Romantic period back to this era. In the book's last chapter, we will return safely to the 1950s, but now recognizing that those years formed no less, but also no more, than a hinge in an old, well-established, and respectable science.

The year 1951 will indeed stand out as a hinge in the history of psycholinguistics, although not by design. It was sheer coincidence that three landmark events were packed in that one year halfway through the twentieth century.

The first event was the Interdisciplinary Summer Seminar in Psychology and Linguistics, which was held at Cornell University from June 18 to August 10. It set out to "explore the relationships which might exist between the fields of psychology and linguistics" and to make recommendations for the development of a field of overlap coined "psycholinguistics." It is widely considered to be the birth of modern psycholinguistics.

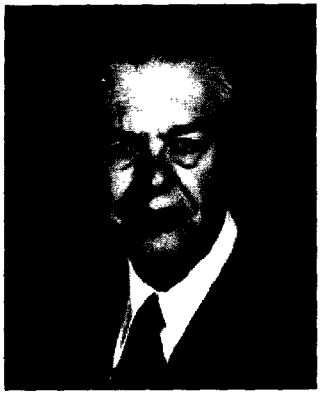
The second event was the publication of George Miller's *Language and communication.* This textbook reviewed the state of the art in the psychology of language and communication. Its systematic treatment of the empirical literature covered more than five decades of research, occasionally reaching back into the nineteenth-century French and German roots of the field. The new discipline of psycholinguistics that was about to be launched would actually continue a century-old research tradition in the psychology of language.

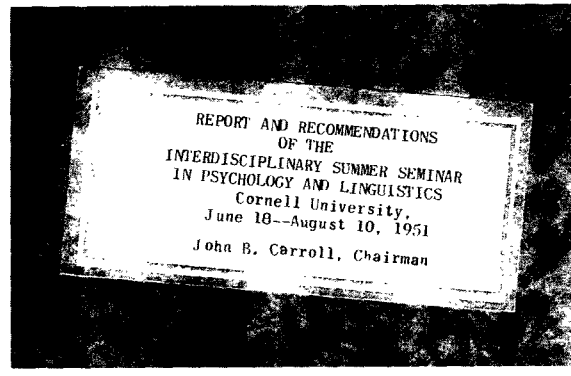
The third landmark was Karl Lashley's paper "The problem of serial order in behavior." This was the first frontal attack on the traditional behaviorist associative-chain theories of serial behavior, such as speech and language. In that paper, Lashley pleaded for a new *syntactic* approach to the treatment of all skilled hierarchical behavior. This was to become a core issue in the imminent "cognitive revolution."

The Interdisciplinary Summer Seminar in Psychology and Linguistics and its follow-up

Cornell 1951

The Summer Seminar would not have happened without John Bissell Carroll (1916–2003) (Fig. 1.1). John (or Jack) Carroll grew up in Hartford, Connecticut. When he was 14, he





(a) (b)

Fig. 1.1 (a) John Bissell Carroll. © SLTI. Reproduced here with permission. (b) The cover title of his 1951 report.

happened to attend a talk at the local children's museum by a certain Mr. Benjamin Whorf, on Aztec and Maya Indians. At the time, Whorf, a fire insurance agent, was also a graduate student of the great linguist Edward Sapir at Yale, and was spending much time on translating an old Nahuatl text. After the talk, John became Whorf's youthful assistant, helping him looking up words in a Spanish Nahuatl dictionary. Whorf greatly inspired the young Carroll over the three years of their cooperation and they continued to keep in touch until Whorf's death in 1941. Whorf also introduced Jack to Edward Sapir, who, mostly through his writings, had aroused Carroll's interest in linguistic typology. But, Sapir convinced Carroll that linguistics would not provide him with a viable career. Something like the psychology of language would offer better opportunities.

After his undergraduate studies at Wesleyan University, where he was introduced to Weiss's behavioristic psychology of language, Carroll became B.F. Skinner's very first graduate student at the University of Minnesota. It was not a perfect match. Skinner, whose work we will discuss in Chapter 8, taught a course in the psychology of language, but Carroll could not recognize or discuss the linguistics he had become well acquainted with through his early training. Skinner, however, was kind enough to introduce him to George Zipf at Harvard University, who, in turn, kindled Jack's interest in the study of word frequencies. Skinner also had him work with Louis Thurstone at Chicago on a factor analytic approach to the testing of verbal abilities. At the age of 25 Carroll spent a year at Indiana with J.R. Kantor, the most behaviorist of all psychologists of language (see Chapter 8). There Carroll published a lucid *Psychological Review* paper which laid out a strictly Skinnerian program for the study of verbal behavior. Only a few years later, then a Harvard professor in the department of education, he accepted an invitation from the Carnegie Corporation to review the state of the art in linguistics and its possible applications. During 1950 he traveled all over the country to interview the leading American linguists. The report he submitted was very well received.[1] It recommended the

[1] Carroll (1953) is an extended, revised version of the report.

development of "language psychology" as an interdisciplinary field. As a man of action, Carroll's next step was to apply to the Social Science Research Council (SSRC) for the funding of a workshop on that topic. It was to become the Summer Seminar in Psychology and Linguistics at Cornell University. Carroll continued to play a central role on the psycholinguistic scene, as will be apparent from Chapters 10–13 and 15. In 1974 he moved to the University of North Carolina, where he became emeritus in 1982.

The Cornell seminar was both a mutual teaching and a joint planning exercise among three senior psychologists and three senior linguists. Aside from Carroll himself, the psychologists were Charles Osgood of the University of Illinois and Richard Solomon of Harvard University. Osgood had made a name in the psychology of language with his work on the semantic differential (see Chapters 8 and 15). Solomon had worked on tachistoscopic word recognition.[2] The linguists were Fred Agard, a specialist in second-language acquisition at Cornell University; Stanley Newman, a former Sapir student and linguistic anthropologist from the University of New Mexico; and Thomas Sebeok, linguist and a leading scholar in semiotics (the study of signs and symbols) from Indiana University.[3]

At the end of eight intensive weeks they had produced a 57-page report clarifying the roles of psychology, linguistics, psycholinguistics, and information theory in analyzing the process of communication. It also discussed some of the core research issues for psycholinguistics, including potential applications, and it concluded with a list of practical recommendations.

Figure 1.2 represents the report's diagram of the communication process. We are as much speakers as we are hearers; we are both transmitters and receivers of messages, and we normally alternate in reciprocity between these roles. The diagram depicts five components in the process and indicates the disciplines that (should) deal with them. Psychology deals with the intentional speaker and the interpretative hearer. This is the domain of perceptions, needs, attitudes, intents, meanings, overt acts, etc. Descriptive linguistics is primarily concerned with the structure and content of messages, irrespective of their antecedents or subsequent effects. And then there is psycholinguistics, neatly assigned with the two tasks of studying the speaker's encoding and the listener's decoding behavior. When we speak, how do events at the semantic level, the level of ideas, determine our "selection of articulatory motor skill sequences"? And how, when we listen, do "auditory or visual receptor skill sequences" determine our selection of semantic events? The report appeared two years after Shannon and Weaver's *The mathematical theory of communication*, which had deeply impressed students of communication. It had suddenly become possible to quantify the amount of information transmitted between sender and receiver, its redundancy, transmission rate and noise in the channel, and so on. George Miller was quick to thoroughly integrate information theory in his textbook. The report dutifully stressed the importance of information theory, but did not do

[2] Together with David Howes, he had just discovered the word frequency effect in visual word recognition, see Chapter 12.

[3] There were also two graduate students involved, Don Dulany and Leonard Newmark.

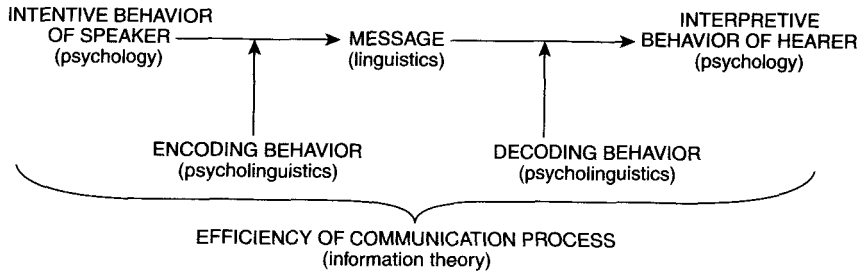


Fig. 1.2 The communicative process. Data from *Report and recommendations of the Interdisciplinary Summer Seminar in Psychology and Linguistics*. Cornell University, June 18–August 10.

much more. Diagram and text assigned to information theory the task of studying the "efficiency" of the communicative process as a whole and of the encoding and decoding processes in particular, but without specifying much further detail.

Although the assignments of linguistics versus psychology/psycholinguistics in Figure 1.2 seem natural enough, the text signals deeper issues. After remarking that "psychological theory has little relevance for descriptive linguistics," the report adds that "when interest shifts to the explanation of linguistic structure it is necessary to appeal to psycholinguistics and perhaps to general psychological theory" (p. 31). Here the authors are (unknowingly) in the good company of Heymann Steinthal, from a century earlier, who we will soon meet in Chapter 2. The issue of explanation in linguistics was in for a rather tumultuous come-back during the "cognitive revolution."

One research issue that received a lot of attention in the report was "the problem of units." The speaker's units of production need not coincide with linguistic units. A speaker may say *it is a case of . . . (pause) homicide.* Here, *it is a case of* is not a linguistic unit, but the pause marks a boundary between the speaker's units of encoding. Inevitably, given the preponderance of information theory, the discussion leads into considering transitional probabilities. The probability that a speaker will say *of* after *It is a case . . .* is much higher than the probability of saying *homicide* after *It is a case of . . .*. This indicates a difference in transitional habit strength. These associative habits are the underlying determinants of the speaker's encoding or selection of units. On this matter, the report follows the standard behaviorist approach to sequential behavior. The stream of speech is governed by the habit strengths of associations. The preferred mathematical analysis of transitional probabilities is in terms of Markov chains. This is not different from Miller's approach in his textbook and, like Miller, the report discusses higher-order transitional probabilities. The probability of saying *do* after *you* is not particularly high, but *do* is certainly a quite probable continuation of *how do you . . .*. We will do a better job in predicting the speaker's next move when we take more antecedent words into account. First-order transitional probabilities form a one-word context only (the frequency of the target word itself). Second-order probabilities take stretches of two subsequent words into account (the target word and the one preceding it), and so on. It was the general belief that there would be some, hopefully not too high, order of transitional probabilities that would

give a satisfactory account of the speaker's transitional habits. "Not too high," because the relevant number of transitional probabilities would become intractable. It is to the credit of the report that this story is told with an inkling of doubt. In particular, the hierarchical nature of language units (words as parts of phrases, phrases as parts of larger phrases or sentences) is not transparently dealt with in this statistical approach, or in terms of the report: "the characteristics of messages containing a hierarchy of units have not been mathematically formalized in any satisfactory way" (p. 17).

The seminar wrestled with the elusive "problem of meaning." The report stresses that much confusion can be prevented by making clear terminological distinctions. Lexical meaning (the meaning of words) is different from grammatical meaning (the meaning of features such as tense or aspect). Denotative meaning is different from connotative meaning. Still, the report is optimistic about the possibilities of a learning theory approach to meaning. Meanings are, in essence, habitual responses to stimuli. In Osgood's so-called "mediation theory" of meaning, the association between an overt stimulus (S), such as a spoken word, and an overt response (R), such as pushing a button, would normally be mediated by an entirely internal chain of "little" s–r associations. But Carroll was practical enough to use the term "idea" when such a mediational process was referred to. Optimism also derived from advances in making meaning measurable. Osgood's recent invention of the semantic differential technique made connotative meaning measurable and new techniques of content analysis would get to the semantic organization underlying whole texts.

With John Carroll, Benjamin Whorf's former assistant, in the driver's seat, the report not surprisingly also discusses the "linguistic *Weltanschauung* problem." Does a language predispose its speakers to attend to particular distinctions when they perceive or think? Suggestions are made for cross-linguistic and cross-cultural experiments, together with ethnologists (i.e., cultural anthropologists).

It had been Carroll's goal to be practical. The report ends with a number of recommendations to promote cooperation and exchange of information between linguists and psychologists. It proposed, in particular, the establishment of a standing committee on psycholinguistics in the SSRC and the organization of further interdisciplinary conferences and symposia on selected topics.

Both recommendations received diligent follow-up. In October 1952 the SSRC established a Committee on Linguistics and Psychology, with Charles Osgood as its first chairman and among its members John Carroll and Thomas Sebeok. One of the committee's first decisions was to organize a follow-up to the summer seminar at Cornell. It took place at Indiana University in the summer of 1953.

Indiana 1953

The Summer Seminar on Psycholinguistics was, like the Cornell seminar, an intensive eight-week mutual tutoring and research planning endeavor. The linguists Thomas Sebeok and Joseph Greenberg were involved in this project full-time. The latter was a linguistic anthropologist and typologist at Columbia University. The full-time psychologists were Charles Osgood and James Jenkins of the University of Minnesota, the latter a

former student of Skinner. Also present was Floyd G. Lounsbury, an ethnologist at Yale with a strong background in behavioral psychology. Involved for a two-week period were John Carroll, MIT linguist Eric Lenneberg, and anthropologist Joseph Casagrande of the SSRC.

The seminar produced a 200-page joint report, *Psycholinguistics: A survey of theory and research problems*, which was to lead a life of its own. It was produced at top speed by a collection of 11 authors, the five full-time staff members, John Carroll, and five graduate students: Susan Ervin, the sole woman on the seminar team and a main contributor to the report, Leonard Newmark, Sol Saporta, Donald Walker, and Kellog Wilson. It was published as a book in 1954, edited by Osgood and Sebeok.

It is clearly apparent from the publication that it was Charlie Osgood who took the lead in this joint author effort. He alone produced one-third of the text and his mediation-theoretical perspective is evident in the remainder of the volume. Co-editor Sebeok's contribution to the writing was, in fact, minimal. Osgood was becoming a primary moving force behind the new psycholinguistics, both scientifically and organizationally. Here, he was on a par with George Miller. There was mutual respect, but little cooperation between these two.

Charles Egerton Osgood (1916–1991)[4] was born and grew up in the Boston area (Fig. 1.3). He went to Dartmouth College and then, for his graduate training in psychology, to Yale University. There, Clark Hull became his idol, but only for being the powerful thinker he was, not as a person to work with. It was Donald Marquis who supervised Osgood's thesis work, *Meaningful similarity of interference in learning.* Meaning would always stay his central research interest. Osgood was convinced that, in the landscape of American behaviorism, Hull's mediation theory provided the best treatment of signs and their meanings. Over his lifetime Osgood continued to refine and qualify this mediational neobehaviorism[5] to handle meaning in language behavior. In 1949 Osgood moved to the University of Illinois at Urbana, where he would stay till the end of his academic career in 1982. Before Urbana, he had been teaching introductory courses in psychology at Yale and the University of Connecticut. It was during this time that he conceived of and wrote his astonishing *Method and theory in experimental psychology*, an 800-page comprehensive review of the experimental literature and its theoretical interpretation. It appeared in 1953. Producing *Psycholinguistics* during the fall of that same year must have been his next major challenge. The Cornell seminar in 1951 had been an eye-opener for Osgood. It was his first professional meeting with linguists; linguists turned out to be serious scientists: "not only were the linguists there neither polyglots nor lexicographers, but they were robust, rigorous and objective."[6] Osgood learned to appreciate their structural descriptions of "the message," their cross-linguistic

[4] William F. Brewer published, in 1994, an excellent biography of Charles Osgood in the *American Journal of Psychology.*

[5] To be discussed in Chapter 8.

[6] From Osgood (1975, p. 17).

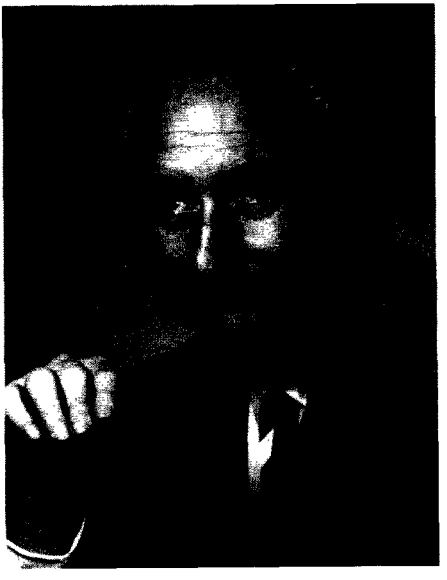


Fig. 1.3 Charles Egerton Osgood.

work, and their work on second-language learning and bilingualism. But he did not see enough "semantics in the foreground." His own work on the semantic differential had, so far, been linguistically uninformed. That was not much different, a few years later, in his best-selling monograph *The measurement of meaning* (1957). It was not until Katz and Fodor's featural semantics of 1963 that he incorporated linguistic ideas into his work on word meaning. During the decade following 1951, Osgood was, with Miller, the dominant personality in psycholinguistics. He was an excellent organizer and an internationally esteemed neo-behaviorist psychologist.

The Indiana report continued to expand on the issues of the 1951 Cornell report. It distinguished three approaches to language behavior. The *linguistic approach* is largely descriptive, determining the phonological, morphological, and syntactic units for a particular language and searching for universals across languages (including any proto-language that might have been the origin of known languages). Issues of meaning, such as homonymy and paraphrase, are the purview of lexicographers.

The *learning theory approach* is concerned with the explanation of language behavior in terms of learned associations between stimuli and responses, i.e., verbal habits. A whole gamut of behaviorist theories is reviewed in the book, and the issue of units is a central one, as already pointed out in the Cornell report. What are the units of encoding and decoding? How do they relate to linguistic units and how can they be empirically studied?

The *information theory approach* quantifies the transmission of information in the channel from speaker to listener. As in the Cornell report, the associative habits from learning theory are mapped onto transitional probabilities in the communication channel. Associations between higher-order behavioral units are ideally mapped onto higher-order transitional probabilities. The mathematics applied is entirely in terms of higher-order Markov processes.

The report treats various applications of these approaches, in particular to first and second-language learning and bilingualism. It also proposes a learning theory account of language change. For instance, in a redundant message one element or unit is strongly predictive of a subsequent unit. Such a strong association increases the probability that one of the elements becomes modified (such as a word unit turning into a suffix). For all these cases of learning and change, mediation theory provides the psychological explanation in *Psycholinguistics.*

The report also gives considerable attention to the Sapir–Whorf hypothesis, the idea that a culture's language affects its users' cognition (see Chapter 13). It discusses stronger and weaker forms of the hypothesis and discusses ways of testing them experimentally. Eric Lenneberg had just started a new paradigm of having participants name color patches, to which we will return in Chapter 13.

Psycholinguistics: A survey of theory and research problems, the second stage of the rocket launched in 1951, became a landmark. It signaled the emergence of a new interdisciplinary effort in the study of human verbal behavior. As Diebold (1965) remarked, it became the "charter of psycholinguistics." It had the (very American) charm of beaming "here we are and we'll do it." It was a barely concealed invitation to jump on the bandwagon. That is what happened and on a remarkable scale, a matter to which we will return in Chapter 15.

Why was the launching of this rocket so successful? There were multiple causes. The appearance of Shannon and Weaver's *A mathematical theory of communication* in 1949 had deeply impressed American psychologists and, to some extent, linguists. The study of (human) communication is a *real science*, objective, quantitative, general, with technological applications in the offing. Both the 1951 and 1954 reports explicitly pronounce this new theory of communication to be their starting point. Of crucial importance was the "pulling in" of linguists. At the time, American linguistics was a minor and quite isolated academic discipline. Edward Sapir had good reason for discouraging John Carroll to go for a career in linguistics. But the psychologist Carroll knew this linguistic world and its concerns in great detail. He had, quite correctly, sensed that both disciplines would profit by crossing their borders. Psycholinguistics had the potential of sparking innovative research, as had happened in so many other interdisciplinary endeavors (such as physical chemistry, biophysics, and biochemistry). Still, this would not have worked without a coalition of strong, highly motivated leaders. Carroll, Sebeok, and Osgood forged the alliance of forces in the new SSRC Committee on Linguistics and Psychology. These leaders were keen to involve talented graduate students from the very beginning. Many of them were co-authors of the reports and became leaders themselves in the new enterprise.

It also would not have worked without the fruitful multidisciplinary ambience and young talent concentrated in the MIT–Harvard region. Here, George Miller exerted effective intellectual leadership.

A crucial role was certainly played by the substantial financial impetus. For the SSRC Committee that impetus came initially from the Carnegie Corporation, where visionary psychologist John W. Gardner, "the ultimate builder of ideas and unifier of people and

causes,"[7] played an instrumental role. More generally, as will be discussed in Chapter 15, communication engineering and science had been receiving major funding, often defense funding, since World War II. Psycholinguistics was getting its fair share.

A final critical factor was that there was sufficient theoretical agreement within the leading forces. Learning theory was common ground in American psychology, in spite of large differences in detail. Most linguists were amenable here, in part because it did not matter much to their real theoretical concerns and in part because Bloomfield's structural linguistics had, since his *Language* (1933), payed lip service to an objective (i.e., behaviorist) foundation of the discipline.

At the same time, the new psycholinguistics was very much a US internal affair. Only there could the behaviorist theory consensus exist. European psychologists and linguists were by tradition mentalists. There was hardly any move to involve any of them. No Jean Piaget, no Frederic Bartlett, no Alexander Luria. Even Roman Jakobson had not been involved in either of the two seminars, although he had been in the US since 1941 and had occupied the chair for Slavic Languages at Harvard University since 1949. His talent was spotted by George Miller, who would soon introduce Jakobson's phonology into his theory of speech sound perception. Most dramatic of all was the fact that the greatest pre-war European psychologist of language, Karl Bühler, was in 1951 spending his time, in almost total academic isolation, as a practicing clinician at the Cedars of Lebanon Hospital in Los Angeles. Back in 1930 Bühler had been offered William McDougall's former chair at Harvard, but by the 1950s his influence was negligble.

George Miller's *Language and communication*

George Miller was not involved with the Summer Seminars. He was in fact running another SSRC seminar himself at Tufts during the same 1951 summer period, on mathematical models for behavior theory. Miller was appointed to the new SSRC Committee on Psychology and Linguistics, but did not show up to meetings[8] and resigned by the end of the first year. He was deep into other issues, in particular mathematical psychology. His time in the new psycholinguistics was soon to come. Nevertheless, *Language and communication*, which nowhere uses the term "psycholinguistics," addressed many of the same issues as the Seminars. What a team produced in the *Psycholinguistics* survey, Miller composed all himself before the seminars had even taken place. The text is a blueprint for a science of communication that is both possible and worth pursuing. It both reviews and unifies various existing approaches. Shannon and Weaver's communication theory provides the background framework, and the tools of information theory are skillfully applied throughout the text. Miller also explicitly opts for a behavioristic approach to provide the psychological basis for a theory of communication. Psychology is a public affair. It has to explain publicly observable behavior. "The bias is behavioristic – not fanatically behavioristic, but certainly tainted

[7] From *Independent Sector* obituary, 2002.

[8] John Carroll (personal communication).

Fig. 1.4 George Armitage Miller. © Courtesy of Harvard University Department of Psychology.

by a preference. There does not seem to be a more scientific kind of bias, or, if there is, it turns out to be behaviorism after all." (p. v). The other explicit choice is for facts, as opposed to speculations. "Rejecting opinions in favor of facts helps to reduce this vast topic to manageable proportions" (p. 1). Indeed, the book rather comprehensively reviews half a century of empirical literature in the psychology of language, but eschews speculation. The latter holds as much for the introspective Wundtian tradition in the psychology of language as for objective speculation. There is, for instance, no mention of *An objective psychology of grammar*, Kantor's (1936) behavioristic diatribe against a subjective psychology of language (see Chapter 8).

George Armitage Miller (1920–2012, Fig. 1.4), born in Charleston, West Virginia, received a Master of Arts degree from the University of Alabama in 1941. He had wanted to become a writer, but decided to pursue a degree in psychology instead and was admitted to Harvard University, where he received his PhD in 1946, under the supervision of psychophysicist Smith (Smitty) Stevens. In 1951 Miller left Stevens' laboratory for an associate professorship at MIT, but then returned to Harvard in 1955 where he would, with interruptions, stay till 1968. It was during this period that Miller and his co-workers applied notions from Chomsky's generative transformational grammar to psycholinguistics. While at Stanford's Center for Advanced Study (1958–1959), Miller, Galanter, and Pribram wrote their influential book *Plans and the structure of behavior*, which replaced the basic S–R reflex units of learning theory with TOTE units, hierarchically organized cybernetic feedback units.[9] In 1960, Miller, together with Jerome Bruner, founded the Center for Cognitive Studies at Harvard, which became a breeding place for the

[9] TOTE stands for test–operate–test–exit.

American cognitive revolution. Miller left Harvard for Rockefeller University in 1968, but spent the years 1972 to 1976 as a visitor at the Institute for Advanced Study in Princeton. There, together with Johnson-Laird, he wrote *Language and perception.* In 1979 Miller moved to Princeton University, the final post in his rich scientific career. There he published *The science of words* (1991) and kept working on his *Wordnet*, a semantic network for the English language.

During the war years Stevens ran a defense-financed, and well-equipped psychoacoustics laboratory in the basement of Memorial Hall. Miller worked on acoustic issues in speech communication. The intelligibility of speech under adverse conditions, such as noise or interruption, was a main topic in his experimental research. And there were potential applications. What is the best masking noise for speech? Miller's answer: it is speech. Some of this fine, but classified, work was published right after the war.[10] Miller continued in the laboratory as an assistant professor till he moved to MIT in 1951. These were extremely productive years. Not only did he write *Language and communication*, but he took the lead in introducing Shannon's new information theory to psychology. Shannon's original two-part paper was published in *The Bell Systems Technical Journal* of 1947. It was only shortly later, in 1949, that Miller and Frick's paper *Statistical behavioristics and sequences of responses* appeared in *Psychological Review.* It was the first application of information theory to psychology, and it set the scene for a small revolution. Seminars and symposia on the Shannon and Weaver theory followed in rapid succession and its applicability in psychology seemed to have no limits. In the Miller and Frick paper the application had been to sequential behavior. Its treatment had always been a controversial issue in behavioristic psychology. Here were the mathematical tools to be applied. The issue of sequential behavior in speech and language would be Miller's focal interest for several years to come. His first go at this problem is fully worked out in *Language and communication*, but the notions of channel capacity, sequential constraints, redundancy, etc. turned out to be of much wider applicability and Miller was, again, a leader in the experimental manipulation of these quantities in studies of auditory and visual perception. His invited paper *What is information measurement?* (1953) made all this accessible to the wide audience of the *American Psychologist.* Ultimately it led Miller to his most cited paper, *The magical number seven plus or minus two* of 1957. As Bruner (1983, p. 97) put it, referring to this publication: "I think if there were a retrospective Nobel Prize in Psychology for the mid-1950s, George Miller would win it hands down – and on the basis of one article." Miller's occupation with information theory did not close his eyes to the latest developments in psycholinguistics. He used the term "psycholinguistics" as the title of his overview chapter in Lindzey's *Handbook of social psychology*, which appeared in the same year (1954a) as the Osgood and Sebeok survey.

Language and communication leads the reader "from the molecular to the molar." It first reviews the articulation and perception of speech sounds. It demonstrates in particular the strong relationship between the intelligibility of a speech sound (be it a

[10] For a complete listing of these studies, see W. Hirst (1988).

phoneme, a nonsense syllable, a digit, or a word in a sentence) and the item's information value (its redundancy, or the number of alternatives). It then turns to language statistics. There is Zipf's law rf = k, which claims that the frequency of occurrence (f) of a word in a text (for instance the word *the*), times its rank order (r) in the frequency hierarchy (usually "1" for the word *the*) is a constant, k.[11] The statistics become more interesting when context is taken into account. A speech sound or word, of whatever frequency, can be more or less redundant dependent on its preceding context. It was Miller's idea to relate transitional habit strengths to transitional probabilities. The talker "has learned a complex set of intraverbal associations . . . The occurrence of a particular symbol or sequence of symbols limits the range of alternatives from which the talker can choose succeeding symbols because he must proceed in accordance with the verbal associations he has learned." (p. 192). The higher the statistical order of approximation to normal text, the more natural it sounds. It fits verbal habits better and it should, therefore, also be easier to memorize. The classical Miller and Selfridge experiment of 1950 demonstrates this indeed to be the case. They used word strings of increasing approximation to English, such as

(first-order): *abilities with that beside I for waltz you the sewing*

(third-order): *tall and thin boy is a biped is the beat*

(fifth-order): *they saw the play Saturday and sat down beside him*

(text): *the history of California is largely that of a railroad*

The ease of recalling a string of words rapidly increases to a fifth-order approximation, but there is no difference in recall between higher-order approximations and real text. This led to Miller's optimistic view that "The process of forging sentences is not inexplicable" (p. 192). Of course, Miller was aware that associative habits could not do all of the work. Personal motivations and the needs of the audience may affect which associations are going to be relevant. However, the "*striking thing*" is "*the great uniformity in the verbal habits of all normal members of the language community*" (pp. 192–193, Miller's emphasis).

Miller's text addresses many, if not most, of the issues in the Osgood and Sebeok survey, and often in a similar vein: verbal learning in children, their acquisition of the phoneme and word repositories of their language, individual differences in style, content analysis, etc. It is more elaborate on issues of readability of texts, but less so on Whorfian issues of linguistic relativity. It has, in addition, a chapter on thinking and problem solving, including syllogistic reasoning, and another one on the spreading of verbal information through social networks. Here, Miller is one of the first to apply graph theory to the modeling of communication in small groups. The applications are plentiful: the effectiveness of mass media, the spreading of (increasingly distorted) rumors, etc.

On one topic both surveys are weak: the domain of language disorders, in particular aphasia. *Psycholinguistics* hardly mentions the issue. *Language and communication* does

[11] Zipf's law will be taken up in Chapter 12.

have a paragraph on aphasia, but a very inconclusive one: "Among the modern investigators one can find almost every point of view, ranging from belief in strict, detailed localization at one extreme to a belief in a generalized loss of abstract intelligence at the other" (p. 240). Miller confines himself to presenting cases of expressive and amnesic aphasia. Indeed, it was a moment of theoretical abeyance in aphasiology, but there is no indication that Miller was much aware of the rich European tradition, more than century old, in aphasia research.

In all other respects, Miller's review of the empirical facts in the study of human verbal behavior demonstrates his thorough knowledge of over half a century of empirical research. The text goes back to Galton's (1879) work on word associations, McKeen Cattell's (1889) pioneering work on letter reading, and Merkel's (1885) work on the effect of numbers of alternatives on the reaction time in disjunctive reactions. Admittedly, many of these old references came from secondary sources, in particular Woodworth (1938), but Miller's text does create a sense of history, flowing without major interruption from the European sources to the present (i.e., 1951) moment.

In spite of this, Miller did not feel that history had endowed us with a coherent science. In his paper (1954a) he writes: "Psycholinguistics is a very young child of two reasonably mature behavioral sciences and it is not yet entirely clear what it includes or how it will develop . . . What is not clear is just how far each science overlaps the other or how the separate conceptual schemes used in the two can be formulated in a common vernacular for purposes of comparison and mutual supplementation." (p. 693). Both Miller and Osgood/Sebeok embrace the term "psycholinguistics" to denote a program for integrating two sciences that have largely developed independently. Miller was aware of the fact that such integration had been the normal case in the European tradition. It had been "more inclined to admit that psychological factors are indispensable in the solution of many linguistic problems."[12] As we will see in the next chapter, it was Heymann Steinthal who wrote in 1855: "Speaking is a mental activity and consequently linguistics belongs to the circle of psychological sciences."[13]

Karl Lashley's *The problem of serial order in behavior*

While the brain was practically ignored in the previous two landmark works discussed, it figured centrally in Lashley's paper *The problem of serial order in behavior*. He presented it at the Hixon symposium Cerebral Mechanisms in Behavior, which was held at the California Institute of Technology in 1948. It was intended to restore communication between psychologists, psychiatrists, and neurophysiologists. The proceedings of the meeting appeared in 1951.

Karl Lashley (1890–1958) was a zoologist, physiological psychologist, and brain surgeon (Fig. 1.5). He received his PhD at Johns Hopkins University, where he worked with

[12] Miller (1954a, p. 695).

[13] "Sprechen ist eine Seelenthätigkeit und folglich gehört die Sprachwissenschaft in den Kreis psychologischer Wissenschaften" (Steinthal 1855, pp. 141–142).

Fig. 1.5 Karl Lashley.

James Watson, the father of behaviorism. In 1935 he moved to Harvard University, where from 1937 he occupied the chair of neuropsychology. He also directed the Yerkes Laboratory in Orange Park, Florida, from 1942. He retired from both posts in 1955.

In his 1951 paper Lashley challenged both mainstream neurophysiology and mainstream psychology to reconsider their prevailing handling of temporal integration. He took the unusual approach of demonstrating the complexities of sequential ordering from linguistics, citing (on p. 181)[14] Fournié (1877): "Speech is the only window through which the physiologist can view cerebral life."

Physiologists as much as psychologists tacitly or overtly adhere to associative-chain theories in explaining serial behavior, Lashley argues, and that is a dead-end track. Physiologists essentially consider the brain as static, only becoming active upon receiving some sensory stimulus. This is then followed by a chain of reflexes, eventually causing some motor response. Psychologists, introspectionists, and behaviorists alike are equally chain theorists. Titchener, for instance, takes the meaning of a word as the chain of associations among the mental images it arouses. Thinking is running such chains, which introspectively appear as inner speech. When Titchener wants to make a point "I hear my own voice speaking just ahead of me." Lashley: "He need not think but only listen to his inner voice: to the chain of auditory images," "lazy man"! (p. 184). The behaviorists, from Watson on, are if possible even more addicted to chain-reflex theory. Washburn (1916), for instance, "describes speech as a succession of vocal acts in which the kinesthetic impulses from each movement serve as a unique stimulus for the next in the series." (p. 182).

This cannot be correct, says Lashley. The elements of speech and language have no fixed orders. The motor actions in pronouncing *right* and *tire* involves the same motor elements, but in reverse order. There is no fixed forward association among them. Positioning the (spoken) word *right* in a sentence, its temporal valence depends on its role as a noun, an

[14] Citations from reprint in Saporta (1961).

adjective, an adverb, or verb, and on its homonymic meaning: "The mill-wright on my right thinks it right that some conventional rite should symbolize the right of every man to write as he pleases." (p. 183). Arranging these words is not due to any direct associations of the word *right* with other words.

What then determines the order? Here, Lashley draws on European aphasiology and consciousness psychology. Aphasiologist Arnold Pick argued in 1913 that, whatever mental schema is active in preparing the content of speech, it must energize a linguistic schema, in which words are inserted. Schemata of different languages need not correspond. German and English, for instance, express the same thought by way of quite different syntactic orderings. Their syntactic schemata are different. Such a schema is a case of the "determining tendency" or "set" proposed by the Würzburg school.[15] Lashley then discusses some instances where such sets are, apparently, active. Errors of typing may be caused that way. For instance, the set for doubling a character, activated by the typing schema for the word *look*, may lead to typing *l-o-k-k* instead of *l-o-o-k*. Similar derailing effects of schemata show up in spoonerisms and tip-of-the-tongue effects, discussed by William James. Sequential verbal behavior is governed by "a series of hierarchies of organization." They range from those that order the vocal movements in pronouncing the word, to the ones that determine the order of words in a sentence, the arrangement of sentences in a paragraph, and of paragraphs in a discourse. Lashley then generalizes: "the problems raised by the organization of language seem to me to be characteristic of almost all cerebral activity." The essential problem of serial order is "the existence of generalized schemata of action," a "syntax of act." (p. 188).

Seriality is a property of any complex behavior, whether it is speech or gait or playing music. Lashley was defining the problem that would dominate discussions in linguistics and psycholinguistics for the years to come. Miller's textbook still viewed serial verbal behavior, such as producing sentences, as the outcome of verbal habits. The sequencing of words emerges from a complex set of intraverbal associations. The report of the Summer Seminar does wonder how such verbal habits can account for the hierarchical nature of language. A speaker always produces a hierarchy of units—text, sentences, morphemes, phonemes—not just a linear string of units. It recognizes that there must be a system of habit hierarchies for which there is, however, no adequate formal treatment available. Lashley goes for a frontal attack: Associative-chain theories, dominant in behaviorism, are deeply inadequate for explaining serial behavior. What is needed is a new *syntax* of hierarchical serial order. Explaining serial order in speech and language will then also set the scene for work in other domains of serial behavior.

Lashley's paper was to become one of the crowbars in the so-called "cognitive revolution." Since Chomsky's (1959) citation it was much referred to in the effort to undermine the dominant learning theory account of language behavior, the behaviorist consensus on which both Miller's text and the reports of the 1951 and 1953 Summer Seminars were based.

[15] To be covered in Chapter 7.

Other 1951 milestones

The 1951 harvest involved more than just these three landmark events. Howes and Solomon, for instance, discovered the word frequency effect. Words that are frequent in language use are more easily recognized in a tachistoscope than less frequent ones, a finding that reverberates in all modern models of word recognition. The Haskins Laboratories published their invention of a pattern playback device (see Chapter 15). Till the advent of the laboratory computer it became the chosen device for studying the perception of vowels, consonants, and short syllables because it could play back any potentially relevant formant transitions. And last, but not least, there was a linguistic milestone in 1951, Zellig Harris's *Methods in structural linguistics.* This was the ultimate statement of distributional analysis, a mechanical, corpus-based discovery procedure for the units of language, phonemes, morphemes, and syntactic constituents. Relevant for psycholinguistics was Harris's claim that these procedures would ultimately allow for a generative grammar, composed of rules (which he called statements): "The work of analysis leads right up to statements which enable anyone to synthesize or predict utterances in the language. These statements form a deductive system with axiomatically defined initial elements and with theorems concerning the relations among them. The final theorems would indicate the structure of the utterances of the language in terms of the preceding parts of the system." (pp. 372–373).

Both Zellig Harris and his student Noam Chomsky worked on developing such a generative, transformational grammar during the 1950s. It was through the cooperation of George Miller and Noam Chomsky that by the end of the 1950s a crucial further landmark was erected in the history of psycholinguistics. In contrast to Zellig Harris, they proclaimed that a generative grammar is "psychologically real," i.e., it is mental machinery whose study is a "chapter of psychology." That story, however, will not be told in this book. Rather, these pages will demonstrate that this Chomsky-and-Miller revolution turned psycholinguistics back to its historical, mentalistic roots.

Part 2

Establishing the discipline: 1770–1900

During most of the nineteenth century "psychology of language" was not a coherent, shared notion. The term did not appear at all till the late 1890s.[1] Heymann Steinthal, who had set out to formulate such a discipline as an auxiliary science for linguistics, usually spoke of "philosophy of language," which indeed was common practice among psychologizing linguists. Still, most branches of psycholinguistics as an empirical science find their roots in the nineteenth century, especially its second half.

Chapter 2 reviews the first, linguistic root of psycholinguistics, which was based on the quest for the origins of language. It was triggered by the late eighteenth-century discovery of the Indo-European language family. The second root, also originating by the end of the eighteenth century, was the empirical study of language in the brain; it became an advanced science during the second half of the nineteenth century, especially after Broca's and Wernicke's discoveries, and is the topic of Chapter 3. The third root is formed by the empirical study of the child's acquisition of language. It had received some attention on and off since Rousseau first published his *Émile* in 1762, but became the subject of systematic empirical study, a fad almost, after Taine and Darwin published their biographical developmental notes in 1877. That story will be told in Chapter 4. The fourth and youngest root of the newly arising discipline consisted of the budding experimental and speech error approaches to the language production and perception of normal adults. Franciscus Donders' elegant experimental discovery and manipulation of "mental processing speed" in 1865 provided the foundation for a new research paradigm in experimental psycholinguistics, "mental chronometry," the measurement of reaction

[1] The oldest reference I was able to find is de la Grasserie's *De la psychologie du langage* of 1889. Regrettably, the book is hardly on that topic; it is barely more than a practical grammar. The next references in my files are Sommer's paper *Zur Psychologie der Sprache* (1891), the chapter entitled *Die Psychologie der Sprache. Einleitung und Art der Sprachfehler* in Gutzmann (1894) and Friedeberger's (1896) *Zur Psychologie der Sprache*, which is largely about sign and spoken language in deaf people. Bagley's (1900) was probably the first journal paper with *psychology of language* in the title.

times. It was widely applied in the world's first psychological laboratory in Leipzig, which Wilhelm Wundt established in 1879. Rudolf Meringer, finally, pioneered the modern analysis of spontaneously produced speech errors by the late 1890s. These developments are reviewed in Chapter 5.

There was no big brother supervising and coordinating these different empirical approaches. They did occasionally interact, but they were also often taken in splendid isolation. The first one to sketch the grand picture was Wilhelm Wundt, at the very end of the century. He could draw on well-developed historical and comparative linguistics, on Steinthal's "philosophy of language," on major advances in the study of language in the brain, language acquisition, and sign language studies, as well as on experimental and speech error research. This grand synthesis will be the topic of Chapter 6, which concludes our review of the eighteenth- and nineteenth-century roots of psycholinguistics.

Chapter 2

Inventing a psychology of language

The origins of language

If any theme obsessed the minds of nineteenth century linguists, it was the natural origins of language and speech. The obsession was fed from two not unrelated sources. One was the transition from the Enlightenment to Romanticism during the latter decades of the eighteenth century. The other was the emergence of historical-comparative linguistics during the same period.

Where the period of the Enlightenment, in its continental tradition, had promoted a rationalist, analytical and ultimately mechanical approach to the study of both nature and mind, Romanticism rather took a naturalistic and holistic stance. It stressed the unity of man and nature, at least in their origins. Its preferred explanations were ecological, organic, genetic, and, increasingly, evolutionary. Language was considered to emerge from natural causes instead of being a divine gift to recently created mankind or some form of deliberate social contract. Logic and formalism were on the decline in the analysis of language and languages. The fascination was increasingly with the comparative, especially in the context of newly discovered "exotic" languages: Asian, African and, in particular, American-Indian.

From the Enlightenment to Romanticism

In 1770, Johann Gottfried Herder submitted his *Essay on the origin of language*[1] to the (Prussian) Royal Academy of Sciences. It was published in 1772. It sets the tone for all Romantic speculation on the origins of language. And it would not be budding Romanticism without a good dose of emotion and exaltation.

Herder's essay is a fierce attack on the then still-prevalent notion of language as a divine creation, which he deems "philosophical nonsense."[2] Rather, language is a necessary product of human nature. "Even as an animal, a human being has language."[3] All strong emotions in the animal are expressed in cries and sounds, and as such communicated. But human language is more than instinctual expression. Characteristic of human nature

[1] Johann Gottfried Herder (1744–1803) submitted his *Abhandlung über den Ursprung der Sprache* to the (Prussian) Royal Academy of Sciences and won the academy's prize for an essay on the origin of language.

[2] "Philosophischer Unsinn" (p. 42; page references are to the 1901 edition of the first, 1772 printing of Herder's 1770 essay).

[3] "Schon als Tier hat der Mensch Sprache" (ibid. p. 21).

is precisely the *lack* of instincts. "As an naked, instinctless animal, a human being is the most wretched of all creatures."[4] However, unlike other animals, man has, by nature, "reflective consciousness."[5] This makes it possible for him to freely attend to anything striking in the environment. When he observes a sheep, he may see or touch the white woolly animal, but the thing that will really catch his attention is the sudden bleating. Next time that conspicuous noise resounds, he will think "Ha! You are the bleating one."[6] The bleating sound has become the sign for the whole creature. And he might imitate it to communicate the sheep's presence to others in the family or tribe. More generally, the sounds of nature provided savage man with his first vocabulary. "From every sounding entity rings out its name."[7] The first language is poetic rather than prosaic, "because what else was the first language than a set of poetic elements? Imitation of sounding, acting, moving nature!"[8] It is a singing, sounding language rather than a talking one. And this feature of language was never fully lost. As language became more structured, "it always remained a kind of song."[9] Audition is primary here. Feeling/touch is too undifferentiated, vision too rich in properties to be useful in the initial state.

This emergence of language is inevitable in the presence of these conspicuous, sound properties of natural entities. "because . . . the experience of a first clear property, hits audition, the sense between eye and touch, *the genesis of language is an inner pressure much like an embryo's pressure for birth at the moment of gestation.* All of nature charges human beings to develop their forces, their senses, until they are human beings. And as language begins from this state, the whole chain of states in the human soul is such, that each of them adds to the construction of language."[10] Human reason and language are intrinsically bound: "Without language, man has no reason and without reason no language."[11] At the same time Herder stresses the social necessity of language: "Man's destination is to be a creature of herd, of community; the genesis of language becomes for him an essential necessity."[12]

[4] "Als nacktes, instinktloses Tier betrachtet, ist der Mensch das elendeste der Wesen" (Herder 1901, p. 97).

[5] For Herder's flood of terms such as "Verstand, Vernunft, Besinnung, Besonnenheit, Reflexion." It is this reflexion which forms the basis of language.

[6] "Ha! du bist das Blökende!" (ibid. p. 48).

[7] "Von jedem tönenden Wesen klang sein Name" (ibid. p. 62).

[8] "Denn was war *diese erste Sprache als eine Sammlung von Elementen der Poesie?* Nachahmung der tönenden, handelnden, sich regenden Natur!" (ibid. p. 65).

[9] "Selbst da die Sprache später mehr regelmäßig, eintönig und gereihet wurde, blieb sie noch immer eine Gattung Gesang" (ibid. p. 67).

[10] "so ist die Genesis der Sprache ein so inneres Dringnis wie der Drang des Embryons zur Geburt bei dem Moment seiner Reife. Die ganze Natur stürmt auf den Menschen, um seine Kräfte, um seine Sinne zu entwickeln, bis er Mensch sei. Und wie von diesem Zustande die Sprache anfängt, so ist die ganze Kette von Zuständen in der menschlichen Seele von der Art, daß jeder die Sprache fortbildet" (ibid. p. 99).

[11] "Ohne Sprache hat der Mensch keine Vernunft und ohne Vernunft keine Sprache" (ibid. p. 52).

[12] "Der Mensch ist in seiner Bestimmung ein Geschöpf der Heerde, der Gesellschaft: die Fortbildung einer Sprache wird ihm also natürlich, wesentlich, notwendig" (ibid. p. 111).

The essay discusses how, from this auditory base, other senses start providing properties to be named, how signs and "words" become more and more arbitrary, and how languages develop over the generations. Herder stresses the remarkable commonality among languages: "among all peoples on earth, grammar is built in almost the same way."[13] Now, "grammar" in this context does not mean much more than the repository of phonemes,[14] the alphabet. Herder's explanation is *not* that universality is dictated by human nature. His explanation is rather that all present languages[15] go back to one original tribe's language, whose features have been inherited generation upon generation, surviving the diversification of "national" languages.

In the same year of 1772 Dietrich Tiedemann (1748–1803) published a book on the origin of language. These works do not reference each other. Tiedemann also rejects the proposition that God invented language for human beings, but his argument differs from Herder's; his is a *reductio ad absurdum* argument. Languages are very different and cannot have a single common source, according to Tiedemann. Hence, divine creation must have been the creation of many different perfect languages. But then, savage peoples must have lost the richness of the languages God gave them, because these languages are now rough and poor. That, however, is counterintuitive. Unlike Herder, Tiedemann postulated gestural communication as the real origin of human languages. Still Tiedemann agreed with Herder that the origin of spoken language is onomatopoeic. People began imitating the natural sounds of the entities around them.

Herder and Tiedemann were not in any way the only philosophers of language origins during the final decades of the eighteenth century. There was, for instance, James Burnett, Lord Monboddo (1714–1799), a Scottish Enlightenment lawyer and philosopher, who published a six-volume treatise *The origin and progress of man and language* (1773–1792). He was probably the first to comparatively study languages of Eskimo, Huron, Algonquian, Tahitian, and Peruvian peoples. These so-called "primitive" languages are vowel-rich and words are strongly polysyllabic, which provides the clarity and redundancy needed by language users when facing the dangers of their primitive society. And there was, of course, Jean-Jacques Rousseau, whose essay on the origin of language and music appeared in 1781. According to Rousseau, language emerged slowly in primordial human societies. Initially, human beings lived almost totally in isolated families. Procreation took place between brothers and sisters. There was only instinctual need (*besoin*), no passion. Communication was largely by gestures and inarticulate sounds of voice. Other people evoked anxiety and aggression. Only after some modicum of existential security had been

[13] "denn unter allen Völkern der Erde ist die Grammatik beinahe *auf einerlei Art gebaut*" (Herder 1901, p. 132).

[14] "Buchstaben."

[15] Chinese may be an exception here, according to Herder. In his later and much riper work, *Ideen zur Philosophie der Geschichte der Menschheit* (1784), he stresses both the unity and diversity of mankind. There are no races. The diversity of body, language, and culture emerges from the interaction between human beings and their environment. Herder goes all out to describe the world's large variety of environments and the variations in human beings they have created.

achieved did contacts begin extending beyond the family. Now boys and girls would meet and real passion would arise. It is from passion, love, pity, fury, that spoken language emerged.[16] Nothing in Rousseau's essay suggests that all languages had a common origin. Rather the type of language arising was much dependent on the climactic conditions in which nations (i.e., tribes, peoples) developed. That explains, according to Rousseau, why languages in northern, cold climates are rude, monotonic, articulated, and shrieking, whereas southern languages are lively, sonorous, and eloquent.

More relevant for a history of psycholinguistics is the work of Viennese scientist/engineer Wolfgang von Kempelen (1734–1804). He was the first to construct a speaking machine, which we will discuss in Chapter 5. This invention was still very much in the Enlightenment tradition of a mechanical approach to human nature. His 1791 book *Mechanism of human speech*[17] is, however, of much wider importance. Not only does it provide the first detailed theory of speech sound formation in the human vocal tract, but it also takes issue with the ongoing discussions on the origin of language. Kempelen takes sides with Herder on the untenability of divine creation, but adds further interesting arguments for language being a human invention. As other thinkers before and after him, Kempelen mentions the universal usability of gestures and sounds for making oneself understood when there is no shared language. We know that this universal means of communication is easily conventionalized, Kempelen argues. He had been visiting the Abbé de l'Épée, the founder of the Institute for the Deaf in Paris. There he had observed that a gestural language can be "brought to the same high level of completeness as our normal spoken language."[18] It has turned into "a conventional language of signs."[19] The Abbé's students "speak in their sign language with admirable skill about the most abstract entities."[20] This proves that a language need not be endowed by the Creator, but can be stepwise invented by us human beings. And what is true for sign language should also hold for spoken language. Kempelen disagrees with Herder (and others) on the common origin of all languages. Here he argues from his native language, Hungarian. Just about all of Hungarian's root words have no equivalent in German, French, or other European languages. Kempelen was fluent not only in Hungarian and German, but also in Italian, English, and Latin; he had good knowledge of Slavic languages as well. And indeed the book is full of tables comparing vocabularies in various languages—for instance number vocabularies in 12 exotic languages—all showing the lack of common origin. We will come back to Kempelen in Chapters 4 and 5.

Let us return to Herder. His romantic speculations were much more influential than Tiedemann's and Kempelen's more enlightened reasoning. Herder's conjectures on the

[16] Somewhat foreshadowing Darwin's theory of sexual selection.

[17] *Mechanismus der menschlichen Sprache nebst der Beschreibung einer sprechenden Machine* (Kempelen, 1791).

[18] "zu einer eben so hohen Vollkommenheit gebracht werden, als unsere gewöhnliche Wortsprache" (ibid. p. 17).

[19] "sie geht in eine konventionelle Zeichensprache über" (ibid.).

[20] "sprechen in ihrer Zeichensprache von den abstraktesten Gegenständen mit bewunderenswerter Fertigkeit" (ibid.).

singing origins of language would still resound, exactly 150 years later, in Jespersen's *Language* of 1922: "Men sang out their feelings long before they were able to speak their thoughts. . . . They little suspected that in singing as nature prompted them they were paving the way for a language capable of rendering minute shades of thought" (pp. 436–437).[21] As we will see, romantic evolutionary speculation never subsided in the meantime,[22] but it became both enriched and restricted by important developments in empirical, evolutionary linguistics. Herder was one of the very first evolution theorists.[23]

Historical and comparative philology

Serious, empirical evidence was emerging from historical and comparative linguistics, which enlivened discussions of language origins. Herder's essay had shown no awareness of the lexical or morphological correspondences between languages. But such comparisons were increasingly being made. Missionaries had long noticed correspondences between Sanskrit and Latin, but it is customary to attribute the birth of historical and comparative philology to a 1786 communication to the Asiatic Society of Bengal. The communication was by Sir William Jones, a British court lawyer in Calcutta and founder of the Society. Jones noticed lexical and morphological correspondences between Sanskrit, Latin, and Greek; "no philologer could examine them all three without believing them to have sprung from a common source, which, perhaps, no longer exists." He also speculated about similarities between these languages and Gothic, Celtic, and Persian, suggesting a common origin.

Within a few decades, historical linguistics became established as a highly respected, rigorous empirical discipline; for many,[24] a natural science. Studying the evolution of the Indo-Germanic[25] language family was the core enterprise. It was not only historical

[21] Jespersen (1922), pp. 436–437. The first time Jespersen proposed this theory was, however, much earlier, in Jespersen (1894).

[22] Except maybe in the Parisian Société de Linguistique, which was founded in 1864. It stated in its bylaws that no communications would be accepted on the origin of language.

[23] This is what he wrote in his *Ideen* (1784): "Several compounds of water, air and light must have preceded, before the germ of the first plant organization, somewhat like moss, could emerge. Many plants must have preceded and died before an organization of animals came about; also here, insects, birds, water and night animals preceded the more advanced surface and day animals, until finally, after all, the crown of our earth's organization emerged, the human being." ("Mancherlei Verbindungen des Wassers, der Luft, des Lichts mußten vorhergegangen sein, ehe der Same der ersten Pflanzenorganisation, etwa das Moos, hervorgehen konnte. Vielen Pflanzen mußten hervorgegangen und gestorben sein, ehe eine Tierorganisation ward; auch bei dieser gingen Insekten, Vögel, Wasser- und Nachttiere den gebildetern Tiere der Erde und des Tages vor, bis endlich nach allen die Krone der Organisation unsrer Erde, der Mensch, auftrat" (Herder 1965 edition, Vol. 1, p. 27)).

[24] Schleicher (1863): "Die Wissenschaft der Sprache ist demnach eine Naturwissenschaft." (p. 7); Chapter 1 of Max Müller (1860) is entitled "The science of language a physical science."

[25] This language family is historically labeled in different ways. Bopp, in the title of his book simply lists all the languages he considered. He preferred to use the term Indo-European, which is still the dominant use (with the acronym PIE for proto-Indo-European). Humboldt, and most German

because of its aim to reconstruct the history of this language family, but also because it could use text records of extinct languages, in particular Sanskrit, as old as two to three millennia. In fact, the pioneers of historical linguistics, Rasmus Rask (1787–1832), Franz Bopp (1791–1876), Jacob Grimm (1785–1863), and many others largely based their analyses on records of extinct languages. It was August Friedrich Pott (1802–1887) who published, between 1859 and 1876, in a monumental six-volume work, the then definitive, comprehensive etymological treatment of the Indo-European languages.[26]

At the same time, the systematic study of languages from other families became organized. In 1820, for instance, Wilhelm von Humboldt proposed to the Berlin Academy of Sciences the establishment of a new discipline: comparative linguistics. Here the focus was less on historical reconstruction than on the comparison of languages, be they related or unrelated. Comparative linguists studied phonological and grammatical commonalities among languages, i.e., language types. Languages became, for instance, classified in terms of how they expressed grammatical relations. Some languages, such as French or German, dominantly *inflect*, adding affixes to words as in tense marking (*work-worked*) or plurality marking (*horse-horses*). Others, such as Finnish, *agglutinate* (the term was presumably introduced by Wilhelm's brother, the naturalist Alexander). Here, long words are formed as a chain of morphemes, eacł. expressing a particular grammatical meaning (such as voice, tense, number, or mood). Still others, for instance the occasional American-Indian language Humboldt studied, *incorporate*. They both inflect and agglutinate. And then there are languages, such as Chinese, that *isolate*; they altogether lack prefixes, suffixes, or infixes. In addition, comparative linguistics studied how a nation's language relates to and affects the community's way of thinking and experiencing, its world view.[27]

Wilhelm von Humboldt (1767–1835) was a towering figure in German and European intellectual life. Being a high-born *Freiherr*, he fulfilled many high-level administrative and diplomatic functions in Prussian society, such as drastically reforming the entire educational system and founding the University in Berlin (now Humboldt University). But increasingly, he dedicated his life to the humanities, among them comparative philology, and stayed in touch with the leading scholars of his time. Among these were renowned orientalists, such as August Wilhelm Schlegel and Franz Bopp. His own main interest, however, was in non-Indo-European languages, such as Basque, Mexican-Indian languages and Austronesian languages. In fact, he established the first comparative language database, of some 200 exotic languages, a good reason for calling him the father of comparative linguistics.

linguists, rather used "Indo-Germanic." Others, such as Müller, spoke of Aryan languages. This term, however, came in disrepute during the Nazi period. I will generally use the term Indo-European.

[26] Pott was a professor at Halle when he produced this work. He had been a student of Franz Bopp (1791–1876), a Sanskritist in Berlin, who was himself a recognized authority in Indo-European philology, author of a six-volume *Vergleichende Grammatik* (1833–1852).

[27] In most of nineteenth-century linguistic literature, the term "nation" roughly stands for "language community."

During the history of linguistics, Humboldt's authority was time and again called upon to serve the rhetoric of the moment. His writing was often fluid enough to allow for that. As Jespersen[28] put it: "Humboldt, as it were, lifts us to a higher plane, where the air may be purer, but where it is also thinner and not seldom cloudier as well." Less deferentially, William Whitney[29] talks about "the dark sayings, of that ingenious and profound, but unclear and wholly unpractical thinker."

Humboldt's place in the history of psycholinguistics relates to three of his notions. The first and most important one is his definition of language. Language is an *activity*. What kind of activity? "Articulation is the essence of language." Language "is namely the ever repeated labor of the mind, to enable the articulated sound to express the thought. Therefore, its true definition can only be a genetic one." And Humboldt adds immediately "Directly and strictly speaking, this is the definition of ever occurring speaking; but in a true and essential respect one can, as it were, see the totality of speaking as language."[30] Language is what the speaker does. It was certainly innovative to define language as an activity rather than as a written or spoken product. As we will see, the psychology of language was invented in the first place to account for this *microgenesis* of language. I will use the term *microgenesis* to distinguish the speaker's generation of language in running speech from the child's *ontogenesis* of language and from *phylogenesis*: language evolution. All three genetic perspectives were actively pursued during the nineteenth-century. Together they form the genetic stance, which became so characteristic of nineteenth-century romantic linguistics and psycholinguistics. Both microgenesis and ontogenesis were time and again invoked to provide explanations for phylogenesis—for the origins of language.

Another Humboldtian notion, that of inner or internal speech form (*innere Sprachform*), was also invoked by the pioneers, all the way into the twentieth century. Heymann Steinthal, as we will discuss, even waxed lyrical about this Humboldtian notion. This is quite surprising, because the term only plays a minor role in Humboldt's work. It surfaces only briefly in Humboldt's treatise and never returns. It is, moreover, completely undefined. We will have to swallow romantic statements such as the one that introduces his paragraph on *innere Sprachform*. It says that even the most artistic and euphonious articulation will not provide the mind with a dignified language, "if the radiant clarity of the ideas that relate to speech does not penetrate it with its light and warmth. This, its entirely internal and purely intellectual part, in the end constitutes language; it is the use

[28] In his *Language* (1922, p. 57).

[29] Whitney (1872, p. 273).

[30] "Die Articulation ist das eigentliche Wesen der Sprache, der Hebel, durch welchen sie und der Gedanke zu Stande kommt, der Schlussstein ihrer beiderseitigen innigen Verbindung." In: *Über die Verschiedenheit des menschlichen Sprachbaues* (1827–1829) (Humboldt, 1963 edition, p. 192). "Sie selbst ist kein Werk (Ergon), sondern eine Tätigkeit (Energeia). Ihre wahre Definition kann daher nur eine genetische seyn. Sie ist nämlich die ewig wiederholende Arbeit des Geistes, den articulierten Laut zum Ausdruck des Gedanken fähig zu machen" (p. 418). "Aber im wahren und wesentlichem Sinne kann man auch nur gleichsam die Totalität des Sprechens als die Sprache ansehen" (p. 418).

for which the generation of language employs the sound form."[31] The best we can say is that Humboldt wants to distinguish the internal, intellectual activity underlying speech from the running, articulated sound form, but the notion lacks "radiant clarity." The notion had been used more clearly before by Herder,[32] to whom Humboldt does not refer.

Finally, Humboldt has always been recognized for promoting the idea of linguistic relativity. As mentioned, his comparative approach to language was as much a comparison of the worldviews that come with the languages. Like almost all nineteenth-century students of language, Humboldt believed that there is a hierarchy of languages, from the most primitive languages of the savage to the most highly developed, with Greek at the zenith. There is relativity here, because "sharpness of thought gains when the logical relations also precisely reflect the grammatical ones."[33] But his study of American-Indian languages had convinced him that even languages of the savage could have their idiosyncratic complexities. And these make possible the expression of a host of ideas. For Humboldt the relation between a nation's language and its worldview did not implicate the strong Whorfian claim that the language determines what can be thought. It is possible to express any thought in any language. Linguistic relativity is rather a matter of affordance: "It is not what can be expressed in a language which determines its advantages or deficiencies, but rather that which it incites and inspires."[34]

The root barrier and beyond

The great challenge for historical linguists was to construct the etymology of speech sounds, inflections and words in the language family they were considering. It was, in particular, the search for roots. Roots were conceived to be the original building materials out of which the vocabularies of the languages had evolved. Roots are not themselves words, but rather the hypothetical origins of a language's words. Words are formed by combining roots: some roots become affixes to others or reduplicate, and roots proliferate by an ever-continuing process of sound change. Rasmus Rask had discovered some

[31] "Alle Vorzüge noch so kunstvoller und tonreicher Lautformen, auch verbunden mit dem regesten Articulationssinn, bleiben aber unvermögend, dem Geiste würdigzusagenen Sprachen hervorzubringen, wenn nicht die strahlende Klarheit der auf die Sprache bezug habenden Ideen sie mit ihrem Lichte und ihrer Wärme durchdringt. Dieser ihr ganz innerer und rein intellectueller Theil macht eigentlich die Sprache aus: er ist der Gebrauch, zu welchem die Spracherzeugung sich der Lautform bedient." (Humboldt 1963, p. 463).

[32] Herder (1901, p. 71) distinguishes "innere Sprache" as "kinds of ideas of the mind" ("Vorstellungsarten der Seele") which has "immediately its sound" ("unmittelbar seinen Laut"), which is "äussere Sprache."

[33] "Die Schärfe des Denkens gewinnt, wenn den logischen Verhältnissen auch die grammatischen genau entsprechen." In: *Über den Einfluss des verschiedenen Charakters der Sprachen auf Literatur und Geistesbildung* (ibid. p. 34).

[34] "Nicht, was in einer Sprache ausgedrückt zu werden vermag, sondern das, wozu sie aus eigner, innerer Kraft anfeuert und begeistert, entscheidet über ihre Vorzüge, oder Mängel." (ibid.).

regularities in these sound changes over time, in particular the shifts among *p*, *f*, and *b*, among *d*, *t*, and *th*, and among *g*, *k*, and *h*, in developments from old Indic Sanskrit to early medieval Gothic and further to modern High German. Jacob Grimm (1822) adopted and extended these notions from Rask and formulated them as regularities of sound shift, which describe the historical transitions. Max Müller introduced the eponym "Grimm's laws"[35] for these sound-shift regularities. If they were to live on as laws, they would be ones with numerous exceptions, but as laws they certainly invited linguists and psychologists alike to come up with explanations for them. And they did. Grimm himself saw in them the work of a *Sprachgeist*[36] that would move a language to perfection. The early medieval sound shifts in German were connected with the people's "violent progress and craving for freedom."[37] Much later in the century, Max Müller (1891),[38] to whom we will presently turn, summarized what to him seemed to be two causes of phonetic change: (i) "I believe I was the first to suggest the prosaic reason that all phonetic change was due to laziness, to an economy of muscular effort required in pronouncing vowels and consonants" and (ii) false analogy. Only the latter is at work in Grimm's laws, according to Müller. Wilhelm Wundt, in his monumental *Die Sprache*[39] of 1900, reviewed many of the psychological explanations put forward during the course of the nineteenth century, such as changes due to ease of articulation, by (false) analogy, through sound symbolism, etc. (see Chapter 6). But in 1821 the Dane Jakob Bredsdorff[40] (1790–1841) had already formulated an astonishingly complete listing of potential psychological causes of sound change. According to him, sound change can result from mishearing or misunderstanding, from misrecollection of sounds, from imperfection of speech organs, from (false) analogy, from the desire to be distinct, from a need to express new ideas, but most importantly from indolence, i.e., laziness. However, Bredsdorff's work did not reach the linguistic audience of most of the nineteenth century.

Roots, in short, were generally conceived as the original building blocks of languages, which became subject to eras of sound change. August Pott (1802–1887) estimated that a language normally has some thousand roots, from which all the words in the language are derivatives. Reconstructing the roots of a language *family*, the *primary* roots (*Urwurzeln*) was an important empirical endeavor in search of the origins of language. They are the most ancient roots that can be empirically reconstructed. It was the hope and conviction of many a linguist that the roots of the Indo-European proto-language would reveal something about sound and meaning in a primitive stage of language evolution.

[35] According to Jespersen (1922, p. 43). See Müller's treatment of the laws in (1860, Vol. 2, Ch. 5).

[36] "ein unbewußt waltender sprachgeist" (Grimm 1851/1958, p. 40).

[37] Grimm (1822), cited by Jespersen (1922, p. 45).

[38] Müller (1860/1891, Preface to the 1891 edition, pp. xvii–xx).

[39] Wundt (1900a, Vol. II, Chapter 4).

[40] See Jespersen (1922, p. 70). He had discovered the original Danish pamphlet, which led to its republication in 1886.

Grimm speculated (without much evidence) that the Indo-European roots only contained three vowels—*a*, *i*, and *u*—and were monosyllabic. Because vowels were feminine and consonants masculine according to Grimm, he doubted whether any primary root could have consisted of a single vowel. To engender (*erzeugen*) a root, you would need both genders as parents. Franz Bopp (1791–1867) and many others accepted the monosyllabicity of primary roots, not Grimm's mystic gender story. Roots could be as short as a single vowel, but would normally also contain consonants. Later in the century, August Fick (1833–1916) summarized the evidence that these roots could consist of a single vowel, or a vowel followed by a consonant, or a single or double consonant followed by a vowel.[41] Hence, *a*, *ap*, *pa*, and *spa* are possible primitive roots. In contrast, Bopp claimed, roots of Semitic languages, such as Hebrew, are characterized by triples of consonants, which means that they tend to be disyllabic. Hence different language families could have had different starting points for their word forms.

Historical linguists were obviously intrigued by the meanings of roots and in particular of primary roots. What is it that "savage man" would express in the early stages of language use? For Herder the first sound imitations would refer to entities such as objects, animals, and plants in the environment. But root analyses made it more likely that primitive roots primarily denoted actions or events. They were more verb-like than noun-like: building, weaving, cooking, plowing, milking, walking, rowing, and cleaning. In addition, a small class of roots had deictic functions, such as indicating "me" and "you."

Reconstructing the original meanings required as much insight into the vicissitudes of meaning—"meaning shift" (*Bedeutungswandel*)—as Grimm's laws provided for sound shift. There is hardly an historical linguist who did not consider the character of initial root meanings and the ways in which they changed and shifted over time, and there was hardly any agreement among them. Grimm, Diefenbach, Heyse, Pott, and others claimed that a given root must have had a specific and idiosyncratic meaning; there were no homonyms, nor synonymies in the first radical stage of language.[42] Ambiguity would defeat the whole purpose of creating linguistic means of communication; it would not be a healthy language. Others, such as Steinthal, Geiger, and Müller, rather argued instead for the polysemy of roots. According to Steinthal it is clearly the case that almost all roots that were reconstructed are ambiguous. The Indo-European stem *dha*, for instance, could mean to do, to stand, and to give.[43] If this was empirically the case for reconstructed roots, why would it have been any different for the very first, primary roots? The answer, in the other camp, was that languages have *decayed* over time, increasingly allowing for

[41] Fick (1870, p. 939).

[42] Heyse (1856, p. 209) posited an "organic unity of meaning and sound" for the original roots. Following this principle, "Each root can, for one and the same language-creating human tribe, consequently have only *one* meaning; polysemous roots are unthinkable." ("Nach diesem Prinzip der organischen Einheit von Laut und Begriff kann mithin auch jede Wurzel für einen und denselben sprachschaffenden Menschenstamm nur *einen* Sinn haben."). Grimm (1851, p. 45) explicitly rejected homonyms ("was unabsehbar verwirren müßte").

[43] Geiger (1869, p. 53).

ambiguities. And indeed, that was a respectable view more generally in the high-romantic era.[44] Grimm, Heyse, Schleicher, and others defended that somewhat misanthropic position. Heyse (1856) wrote: "Thus we see the languages, during the whole time course of their historical life, in a state of slow dissolution and disruption."[45] Schleicher gave this a biological foundation by considering languages as living organisms, with a lifecycle from birth to adulthood, decay, and death. During decay, "the spirit (*Geist*) withdraws from the language, from the sound in which it used to live."[46]

Geiger, however, argued that polysemy of roots gave them the potential to engender a rich array of more specific meaningful roots. Also, the existence of quasi-synonymous roots would stimulate a further specification of meaning. Indeed, constriction of meaning was a much observed phenomenon in historical meaning change. And there was a psychological explanation as well: the drive, even instinct, to establish clarity. At the same time, widening a word's meaning was observed no less often, i.e., specific terms getting a more general meaning. Here there was the opposite drive at work: to maximize ease of use. But Geiger argued for linguistic discipline. Reconstructing the meaning and form of primary roots is a matter of meticulous comparative language work. Nothing is a priori obvious here. Both the sound form of a root and its meaning can unrecognizably change over time and, importantly, there is hardly ever an intrinsic relation between the two of them. What happens is that the sound form keeps fluctuating and the concepts keep shifting and differentiating. The basic mechanism then is for a related concept to become associated with a related sound form. Here majority use decides. Hardly ever is an entirely new root or an entirely new concept introduced into the evolving language.

The linguist Max Müller (1823–1900) exerted considerable efforts to reconstruct the meanings of Sanskrit roots. Born in Dessau, Müller studied Sanskrit and historical linguistics with Franz Bopp in Berlin and Sanskrit with Eugène Burnouf in Paris. He set out to translate the Rig Vedas and went in 1846 to England to study the Sanskrit texts in the possession of the East India Company in Oxford. He was to stay there for the rest of his productive life, occupying chairs of modern European languages, comparative philology, and comparative theology. Müller, son of a poet,[47] was an elegant writer, whose books reached a wide intellectual audience.

[44] But the view that languages decay was older and widely shared. Samuel Johnson, for instance, wrote: "Tongues, like governments, have a natural tendency to degeneration." (Johnson 1775, p. 5).

[45] Grimm (1851) is ambivalent here. He laments the loss of inflexion, going from Sanskrit to the modern Indo-European languages ("ist die innere kraft und gelenkigkeit der flexion meistens aufgegeben und gestört" (p. 38)), but then argues that this was replaced by a system in which the use of particles became a powerful tool for thought, with English presently at the very top. English displays "eine wesentliche gewalt des ausdrucks . . ., wie sie vielleicht noch nie einer andern menschlichen zunge zu gebot stand." (p. 53). Heyse (1856, p. 211): "So sehen wir durch den ganzen Zeitraum ihres geschichtlichen Lebens die Sprachen in einem Zustande allmählicher Auflösung und Zerrütung."

[46] "desto mehr zieht sich der Geist aus der Sprache, aus dem Laute heraus, in dem er einst allein lebte" (Schleicher 1860, p. 64; see also n. 71).

[47] In fact, the author of both *Die Winterreise* and *Die schöne Müllerin*.

Being one of the world's leading Sanskritists, it was obvious for Müller that he was sitting on a gold mine. After all, it was the Hindu grammarians themselves who, in Panini's time,[48] had invented the notion of roots and a listing of some 1100 Sanskrit roots from Panini was still available after more then two millennia. Starting from there, Müller drastically reduced this large number in several steps, considering duplications, sound relations among them, and so on. He ended up with 121 core roots. He then attempted an extensive analysis of their meanings, which led to his final list of 121 original concepts. "These 121 concepts," Müller states, "constitute the stock-in-trade with which I maintain that every thought that has passed through the mind of India, so far as it is known to us in its literature, has been expressed."[49] Practically all of the 121 concepts are actions, such as sharpen, scratch, clean, push, dress, attack, burn, run, and speak. Some are mental states, such as hate, fear, know, think, and desire. In addition there are be and do.

Müller's highly readable book was entitled *The science of thought.* "Language," he said, "is the true autobiography of mind." (p. 515). In "the innumerable languages of the human race the students of mental science may find materials as rich and as real as any that nature supplies to students of physical science." (p. 292). In various versions this view had been around since Herder and Humboldt. As we will see in Chapter 6, Wundt shared this view, insisting that psychologists exploit this still fully accessible data base to discover the evolutionary laws of human cognition.

However, the methods of historical linguistics inevitably led to a root barrier: how certain can we be about reconstructed primary roots and would it at all be possible to look beyond them? "The roots stand like barriers between the chaos and the cosmos of human speech" (Müller 1887, p. 174). How had they come into existence to start with? In his fine review of the literature on language origins, Lazarus Geiger[50] was even less optimistic than Müller. The farther you go back in history, the less you are able to reconstruct the roots, their sound forms, and meanings. You will never see the beginnings of language. The comparative methodology of historical linguistics is unable to tell us how the roots came about. Still, at this point linguists made different choices. Some, such as Franz Bopp, August Pott, and the American orientalist Dwight Whitney[51] kept to their handiwork, the meticulous analysis of available text materials. And so did Schleicher. For him

[48] The great Indian linguist Panini is generally assumed to have lived around the fifth century BC. His team of phonologists/grammarians operated in an oral tradition. Their texts were painstakingly orally transmitted from generation to generation, even into the twentieth century (see, for instance, Staal 1986).

[49] Ibid. p. 406, directly following the list of concepts.

[50] Geiger (1869), which is the popular edition of his two-volume (1868–1872) work on the origins of language.

[51] Whitney hated psychologizing. Steinthal was one of his targets: "may Heaven defend the science of language from psychology!" (Whitney 1872, p. 287). "Psychology may be a valuable handmaid to linguistic science, but it must be a harmful mistress; it may follow alongside of historical investigation, guarding and checking every conclusion, but it has no right to claim to go in advance and lead the way." (p. 307). With Max Müller he entertained a life-long feud, whether it concerned linguistics or the psychology thereof.

it was not the task of linguistics to go beyond the root barrier: "to fathom the way in which human beings came to create this most simple, still reconstructable oldest language, is not its business, it rather falls within that of anthropology."[52]

But others, including Müller and Geiger, would cross the root barrier and speculate on how the first speech sounds might have emerged in primordial society. Here we enter the realm of speculation, and the speculation was often psychological in nature. Linguists came up with their own folk psychologies. They could hardly do otherwise since there was a distinct lack of an established psychological science. If we find some of these nineteenth-century conjectures raving or amusing, we should also entertain the humbling thought that now, in the twenty-first century, we still lack an empirically founded evolutionary theory of language origins.

According to Herder's sound *imitation theory*, primitive man would imitate the natural sounds of entities in the environment and this theory was often invoked by later writers. There are, after all, still obvious sound imitation words in our own languages, the onomatopoeias of the *bow wow* and *cuckoo* type. Humboldt, for instance, accepted this as a possible origin, but one of limited significance. Sound imitations are infrequent in a language and, as later linguists, such as Max Müller, would argue, they are by far less productive than other roots. Humboldt added another possible source of roots, what we now call *phonetic symbolism*. Some speech sounds invite particular meanings, according to Humboldt. *St* suggests fixedness. We use it in words such as stand (*stehen*), stiff (*starr*) and stately (*stätig*). The sound *w* suggests fluctuation and we use it in words such as wind (*Wind*), whirling, wavering (*Wirren*) and wish (*Wunsch*).[53] This observation has stood the test of time. Our guesses of word meanings in an unknown language are not entirely random (see Chapter 12). How do these sounds acquire their connotations? Geiger does not hesitate to suggest a scenario. The original meaning of a vocal sound was the gesture that produced it; visually perceived gesture and produced sound are the same. Pressing the lips together and then releasing them produces an *m*-sound. The Greek stem *myô* still means closing the mouth, and by analogy closing the eyes and, via closing the hand, *myôn* means palm of the hand, and so on. In this way, many *m*-words still have related connotations, according to Geiger.[54] In spite of such heroic efforts, sound symbolism remained of limited significance in linguistic etymology. We will probably never know whether savage man was a phonetic symbolizer. Finally, Humboldt mentioned as a mechanism that denoting similar meanings was preferentially done by using sound-similar words. A small change in a root's sound form would often go with a small change in meaning. In this way roots can engender families of both meaning and sound related words. This is still a valid mechanism in historical linguistics.

[52] "aber wie der Mensch dazu gekommen ist, diese einfachste, erschließbare älteste Sprache zu schaffen, das zu ergründen ist nicht ihre Sache. Die Lehre von der Entstehung der Sprache liegt jenseits ihres Gebietes, sie fällt viel mehr in das der Anthropologie." (Schleicher 1860, p. 37).

[53] In *Über die Kawisprache* (1836, p. XCIV *ff*). The German examples are Humboldt's own.

[54] Geiger (1869, pp. 167–169).

Some theories of language origins are more psychological than others. Rousseau (1781), about a decade after Herder's essay, proposed that language originates as a "cry of nature." Initially the rather solitary primeval man or woman would emit sounds, cries, as we still do when we hurt ourselves. These were natural expressions of affect and emotion. Only later, when people, in particular men and women, occasionally met, would they slowly agree on particular vocal articulations to express certain ideas. This type of theory lived on as the *interjection* theory (or the *pooh-pooh* theory, as Jespersen[55] coined it). Such interjections still exist in every living language. They are also the first vocal expressions of infants. Darwin (1809–1882) sympathized with this theory in his *The expression of emotions* (1872). Steinthal, as we will see, explicitly turned to psychology for an explanation of these vocal expressive movements, in particular to Lotze's theory of reflex movement. When the nervous system gets excited, the nervous energy flows over into the motor system, causing muscles to contract, including the respiratory and vocal muscles. This is a purely mechanical reflex. Still, the interjection theory never took much hold in linguistics.[56] Max Müller and others noted the great variability of the "same" interjection in different languages. What is *Oh!* in English, is *Ih!* in German and *Ah!* in French.[57] What is the real natural cry? Such interjections are, rather, arbitrary conventional signs just as any other word, according to Müller.

Müller himself advocated a different theory, the *synergastic* theory proposed by the philosopher of language, Ludwig Noiré (1829–1889).[58] Its starting point is similar to the interjection theory. When you are excited or exert muscular effort, uttering sounds such as shouts, hems, songs, laughs, or cries may give a sense of relief. According to Noiré, this is particularly striking when people work together, such as rowing sailors or spinning women. There is a consciousness of their joint activity together with their jointly produced sounds. These sounds, then, display two noticeable properties. They become, first, signs of repeated acts, which we can remember as such. "The basic content of all primary roots is nothing other than human activity."[59] And, second, they are not the solitary expressive sounds produced by Rousseau's or Steinthal's primeval man, but they are the sounds produced by people working together with a common purpose, and understood as such by all. "The speech sound originated in the solidarity of activity" and

[55] Jespersen (1922, p. 414).

[56] Psychologists were more sympathetic. William James (1890, vol. 2), for instance, saw the emotional interjection as the starting point. Imitating another's interjection would not have the same emotional cause. But then, it would remind the imitator of his own previous use of that natural cry, which would as yet cause the emotion to arise. This coupling and decoupling of sign and emotion would "*fix* this cry on *that* emotion" (p. 358).

[57] Müller (1887, p. 190).

[58] Noiré (1877). Müller discusses the theory in Müller (1887, pp. 300 *ff*), a book dedicated to Ludwig Noiré.

[59] "daß der Inhalt der Grundanschauung aller ursprünglichen Wurzeln nichts anders ist als menschliche Thätigkeit" (Noiré 1877, p. 251).

(citing Feuerbach) "Only by communication, only from the conversation of people with people ideas originate."[60] According to Noiré, we need not speculate about these original activities. There is a solid archeological record of the tools used by primeval tribes in their cave dwellings or lacustrian huts for their communal activities, such as cutting, weaving, digging, and building. And there is a record of the artifacts they jointly produced.

If the first vocal sounds were recognized as signs of joint, repeated acts, then the primitive roots should all denote such actions. That is a strong claim, Müller realized, and he set out to check for roots that might denote objects or attributes. Would they exist, or would they in fact also denote actions?[61] Müller produced various cases, among them the Sanskrit word for color: *varna*. Indeed, it is clearly derived from the root *var*, which means to cover, i.e., an activity, in this case meaning to cover with paint. So, for Noiré, the characteristic feature of the vocal sounds that became primary roots is that they involve consciousness of repeated social acts. Certainly, it was a major innovation for Noiré to postulate social, communicative causation of roots. After all, the dominant psychology of that era was an *individual* psychology. Correspondingly, the dominant explanations of language origins were individualistic. The first speech sounds were, in some way or another, expressions of the individual soul. This was as much true for Steinthal in 1855 as it was for Wundt in 1900. But for Müller, thanks to Noiré: "One of the oldest riddles of the world seemed to me solved, and solved without a residue."[62]

Inevitably, the discussion of language origins beyond the roots increasingly interacted with Darwin's theory, to which we turn next.

Darwinian evolution and language origins

The idea of evolution was prominent after the beginning of the Romantic era and most students of language took some position in the ongoing discourse in biology, (physical) anthropology, anatomy and, of course, historical and comparative philology itself. Herder extensively discussed human evolution in his anthropological work.[63] He was very aware of the fact that species evolve in interaction with their specific environments and he provided an almost comprehensive treatment of human physical and cultural appearance as it developed in the different geographical and climactic environments of our world. Still, the notion of common ancestors of man and animal did not seem credible to him. In order to be human, he argued, you had to be upright. Even wolf children who had grown up among quadruped animals were thought to be unable to acquire language. If man had been a four-footed animal from the very beginning, he would never have become upright to acquire language. That required a new act of creation. Although all forms

[60] "Der Sprachlaut ist aus der Gemeinsamkeit der Thätigkeit hervorgegangen" (Noiré 1877, p. 324).

[61] He does not cite Grimm (1851) here, who stated that "All nouns presuppose verbs" (ibid. p. 45), *wind* meaning *the blowing one*, etc. (ibid. pp. 45–46).

[62] Müller (1891, Vol. 1, p. X).

[63] See here, p. 25, n. 23.

of animal formation seem to converge on the human "Gestalt," it was the "creating Mother" who "coalesced all these animal Gestalts to form the main Gestalt, human beauty."[64] Still, Herder was not a creationist. His "creating Mother" was not a personal God, but nature itself.

Jacob Grimm, three quarters of a century after Herder, returned to the biblical story. According to him it could not be the case that language was revealed to mankind after the creation. In that case the first people would have been seriously handicapped before that moment of revelation, which is incompatible with divine omnipotence. The ability to develop language must have been endowed in the very first people. Grimm only qualified the biblical story by suggesting that human beings were not created as a single couple, but as a few couples. After all, a single first mother "could have begotten only sons or only daughters, by which all procreation would have been hindered."[65]

But others showed much more awareness of new insights in evolution. In particular, August Schleicher (1821–1868) promoted the idea of natural evolution to the linguistic community. A respected Indo-Europeanist, Schleicher was also a botanist. He was accustomed to the biological notions of genus/species/variety and set out to apply them in his comparative linguistic theory. He was the first to propose (and draw) language pedigrees (*Stammbäume*) and considered languages as living organisms. Languages originate, reach maturity, and decline. These ideas are amply evident in his 1860 book *Deutsche Sprache*, written whilst he was unaware of Darwin's *On the origin of species* (1859).[66] The 1860 German translation of *On the origin of species* was sent to Schleicher by his friend, the famous anatomist/zoologist Ernst Haeckel (1834–1919). In response, Schleicher wrote him a long letter, which was eventually published in 1863. That essay drew the attention of his colleagues, especially occasioned by his sudden death in 1868. Both Müller and Farrer published reviews of the essay in *Nature* (1870).

Darwin's own reference to language in *On the origin of species* is this:

> It may be worth while to illustrate this view of classification, by taking the case of languages. If we possessed a perfect pedigree of mankind, a genealogical arrangement of the races of man would afford the best classification of the various languages now spoken throughout the world; and if all extinct languages, and all intermediate and slowly changing dialects, had to be included, such an arrangement would, I think, be the only possible one. Yet it might be that some very ancient language had altered little, and had given rise to few new languages, whilst others (owing to the spreading and subsequent isolation and states of civilisation of the several races, descended from a common race) had altered much, and had given rise to many new languages and dialects. The various degrees of difference in the languages from the same stock, would have to be expressed by groups subordinate to groups; but the proper or even only possible arrangement would still be

[64] Herder (1784, pp. 96–97): "Als die bildende Mutter ihre Werke vollbracht und alle Formen geschöpft hatte" . . . "da ging sie mit sich zu Rat, drängte die Gestalten zusammen und formte aus allen ihr Hauptgebilde, die menschliche Schönheit."

[65] "ist es sogar wahrscheinlich, dass mehr als ein paar geschaffen würde, schon aus dem natürlichen grunde, weil die erste mutter möglicherweise lauter söhne oder lauter töchter hätte gebären können, wodurch alle fortzeugung gehinderd worden wäre" (Grimm 1851, p. 36).

[66] According to Schleicher's (1873) own account. We have no reason to doubt this.

> genealogical; and this would be strictly natural, as it would connect together all languages, extinct and modern, by the closest affinities, and would give the filiation and origin of each tongue.[67]

Darwin was, as we will see, well aware of endeavors in historical linguistics,[68] and he himself proposed the construction of pedigrees—genealogical trees of languages. That was exactly what Schleicher had already introduced into historical linguistics, as early as 1853,[69] something Darwin was (then) apparently unaware of. Understandably, Schleicher was quite excited by his first reading of Darwin. His essay, and another follow-up,[70] elaborated the notion of pedigrees (which his friend Haeckel was then to introduce on a large scale in evolutionary biology), arguing that linguistics was in a superior position on this point. Whereas ancestral species are quite hard to reconstruct in biology, ancestral languages have left written records going back more than 2000 years. What figures as a *genus* in an evolutionary pedigree corresponds to a linguistic *stem*. *Classes* correspond to language *families*; *subspecies* to *dialects*, and *varieties* to *subdialects*. Sharp linguistic boundaries cannot be drawn here, while in biology they can, according to Darwin. The (graphic) roots of the family trees correspond to original languages (*Ursprachen*) and there have been several such original languages. These have independent origins but are still remarkably similar in form, according to Schleicher. In some languages, such as Chinese, that form is still preserved. The roots or words are just meaning sounds (*Bedeutungslaute*), simple sound images for percepts and concepts, which can have any grammatical role. They are neither nouns nor verbs, neither conjugations nor declensions. In such languages grammatical functions are not expressed in sound forms.

But then language families diverged during the second stage of their development. Indo-European languages, for instance, prefer suffixes over prefixes to mark grammatical relations, whereas the Semitic language family prefers prefixes over suffixes. Schleicher did not hesitate to apply Darwin's notion of struggle for life as a main cause of language death. Some languages, like some species, can survive for a long time in a protected niche. Basque is an example. In general, languages decline and are inevitably doomed; only a few will survive. Doomed, for instance, are the American-Indian languages. We can already observe that "the Indian tribes of North America, if only because of their infinitely complicated and in forms truly rampantly growing languages, are unfit for historical life, and therefore henceforth doomed to atrophy, even ruin."[71] On one point Schleicher chose to differ from Darwin: the latter apparently shares "the well-known narrow-mindedness of his compatriots in matters of religion," where he makes the illogical concession of

[67] Darwin (1859, pp. 422–423).

[68] As he explicitly states in *The descent of man*, p. 56.

[69] Schleicher, A. (1853).

[70] Schleicher, A. (1865).

[71] "So wie wir nun wahrnehmen können, dass gewisse Völker, so die Indianenstämme Nordamerikas, schon ihrer unendlich complicierten und in Formen wahrhaft wucherenden Sprachen wegen für das geschichtliche Leben ungeeignet sind, und deshalb nunmehr einer Rückbildung, ja dem Untergange verfallen" (ibid. p. 28).

reconciling his views on the evolution of organisms with the notion of Creation. That is, of course, a "contradiction."[72]

Schleicher did not live to see *The descent of man* (1871) or *The expression of emotions in man and animal* (1872). They might have moved him to ponder that initial stage of language, where, according to him, imitations and expressive sounds dominate. He would certainly have enjoyed seeing himself cited by Darwin on page 56 of *The descent of man.* Darwin had seen the review of Schleicher's *Darwinism* paper by Farrer in *Nature* 1870 and read Bikker's English translation of the essay. He had also read Max Müller's celebrated lectures,[73] as well as his cousin Wedgwood's work on the origin of language,[74] and he expressed his agreement with their views: "I cannot doubt that language owes its origin to the imitation and modification, aided by signs and gestures, of various natural sounds, the voices of other animals, and man's own instinctive cries."[75] For instance, "some unusually wise ape-like animal should have thought of imitating the growl of a beast of prey, so as to indicate to his fellow monkeys the nature of the expected danger."[76] Darwin added the entirely new suggestion that sexual selection might have contributed to the evolution of language as well: "primeval man, or rather some early progenitor of man, probably used his voice largely, as one of the gibbon-apes at the present day, in producing true musical cadences, that is in singing; . . . this power would have been especially exerted during the courtship of sexes, serving to express various emotions."[77] Curiously, Jespersen (1922) did not refer to Darwin when he wrote "Language was born in the courting days of mankind," and then espousing his theory of primitive singing.[78]

Darwin further discussed evolutionary relations between the development of the brain, of mental powers, and of language, being careful not to identify thought and language. A remarkable claim is that "hand-writing is certainly inherited,"[79] a case, as it were, of self-domestication. And why did we opt for the vocal organs as the instrument of language, not for the hands and fingers? After all, "a person with practice can report to a deaf man every word of a speech rapidly delivered at a public meeting." It is because "the loss of our hands, whilst thus employed, would have been a serious inconvenience."[80]

[72] "wo der Verfasser der bekannten Beschränktheit seiner Landsleute in Glaubenssachen das wenig folgerichtige Zugeständniss macht, dass sich mit seiner Ansicht dennoch der Begriff der Schöpfung vereinigen lasse" (Schleicher 1863, p. 8).

[73] Müller (1861–1864).

[74] Wedgwood (1866) was a supporter of the imitation theory.

[75] Darwin (1871, Vol. I, p. 56). Müller's adoption of Noiré's theory was not yet there in Müller (1860).

[76] Ibid. p. 57.

[77] Ibid. p. 56.

[78] We know he had read *The expression of emotions*, extensively citing, on p. 414, Darwin's "physiological reasons for some interjections." He also did not refer to Donovan (1891).

[79] Darwin 1871, Vol. I, p. 58, under reference to his *The variation of animals and plants under domestication*, Vol. 2, p. 6.

[80] Ibid.

Finally, he discussed the parallels between the formation of languages and the formation of species. Here he referred to Lyell (1863) rather than to Schleicher. He also cited Max Müller, precisely where the latter in his review of Schleicher's paper replaced the struggle for life among languages by the struggle for life among words (after all, it is not languages that struggle, but the people who speak them): "A struggle for life is constantly going on amongst the words and grammatical forms in each language. The better, the shorter, the easier forms are constantly gaining the upper hand, and they owe their success to their own inherent virtue."[81]

However, Müller was not in total agreement with Darwin. He refused to accept that the progenitors of human beings were animals.[82] "By no effort of the understanding, by no stretch of the imagination, can I explain to myself how language could have grown out of anything which animals possess, even if we granted them millions of years for that purpose" (p. 163). "Language is our Rubicon, and no brute will dare to cross it" (p. 177). More specifically, only man could have developed the first, radical stage of language from the earlier stage of imitations and natural cries. In this radical stage only primary roots were used. Muller then tries to reconcile this position with Darwin's own abstract framework. After all, Darwin himself did not reduce all organic beings to one ancestor. He allowed for four different progenitors; "but why he should have thought it impossible that in addition there was a fifth progenitor for man, I cannot understand" (pp. 157–158). So, no disagreement with Darwin about man as a product of evolution, but we were blessed with our own progenitor, which enabled the evolution of language.

What was Darwin's response? Here is Müller's own account: "I had the privilege of corresponding and conversing with Darwin, after I had delivered my Lectures to the Royal Institution in 1873, and though he was unable to discuss the whole question with me, because, as he said, his knowledge on the subject of languages was only derived from a few personal friends, he said to me in the kindest, half-humorous, half-serious way, 'You are a dangerous man'" (p. 153). Müller was vain enough[83] not to spot the irony in Darwin's remark: "Far be it from me to see in these words more than a good-natured compliment; but they showed at all events that Darwin's mind remained accessible to argument to the very end of his life" (p. 153). Darwin's young friend George Romanes would, though still politely, give short shrift to Müller's notions of language evolution.[84]

Wilhelm Wundt, in his 1920 autobiography, reflected on the nineteenth-century efforts by linguists to explain the origins and evolution of language, as follows: "Closer testing

[81] p. 60, from Müller (1870, p. 257). Zipf (1949) developed the same notions as his *principle of least effort*, see Chapter 12.

[82] Müller (1887), dedicating all of Chapter IV to this issue.

[83] His Preface to *The science of mind* ends with the complete listing of all his national and international knighthoods, academy memberships and honorary degrees, an impressive list indeed.

[84] Romanes (1889) ridicules Müller's misconceptions, such as this one: "Once a sheep, always a sheep, once an ape always an ape, once a man, always a man . . . What seems to me simply irrational is to look for a fossil ape as the father of a fossil man." (p. 432). See also Chapter 5.

the explanations by linguists from a psychological perspective, it turned out, irrefutably, that they generally arose completely haphazardly."[85] The linguists were not to blame, because there was no contemporary psychology advanced enough to handle the genesis of language at any level, microgenetically, ontogenetically, or phylogenetically. It was Heymann Steinthal who would take the first steps in inventing such a psychology.

Heymann Steinthal

Steinthal wrote in 1855: "Felicitous advances in linguistics presuppose an advanced psychology."[86] One of his life-long projects was to develop such a psychology. In a generous mood, we might say that the psychology of language was his invention, although he never used the term himself.

Heymann Steinthal (1823–1889, Fig. 2.1) was born in Gröbzig. He studied philology and philosophy with Bopp, Trendelenburg, and Heyse in Berlin, where the ghosts of Humboldt and Hegel were still very much evident. His 1849 *Habilitations schrift* was about their linguistics and philosophy. After a three-year sojourn in Paris, where he studied Chinese and African languages, he returned permanently to Berlin, where he held professorships in general linguistics and in biblical exegesis.[87]

For Steinthal "the essence of language is identical to its origin," its genesis. Language is an emanation of the human mind. "In just the same way as any embryo builds this or that organ during a particular phase of its development, at some definite point the mind necessarily builds language, today, as in the primeval era."[88] Steinthal embraces Humboldt's genetic view on language. The essence of language is as much visible in the speaker's microgenesis of language, as it is in phylogenesis and in ontogenesis. In fact, to approach the classical problem of origins, the linguist is in an advantaged position because "the laws that are still today operative in the child's acquisition of language, were also the driving forces in the invention" of language.[89]

[85] "Es ergab sich, wenn man die Erklärungen der Sprachforscher vom psychologischen Standpunkte aus näher prüfte, unabweislich, daß sie im ganzen völlig planlos entstanden waren." (Wundt 1920, p. 262).

[86] "Glückliche Fortschritte in der Sprachwissenschaft setzen eine entwickelte Psychologie voraus." (Steinthal 1855, p. 234).

[87] The latter one in the Hochschule für die Wissenschaft des Judentums.

[88] "Und die Folge also ist die: dass Wesen und Ursprung der Sprache identisch sind; ihr Wesen liegt in ihrem Ursprunge, und ihr Entspringen ist ihr Sein und Wesen." "Die Sprache ist eine Emanation, eine Entwicklung der Seele." "Wie jedes Embryo in einer bestimmten Epoche seiner Entwicklung dieses oder jenes Organ bildet, so bildet die Seele auf einem gewissen Punkte nothwendig Sprache, heute, wie in der Urzeit." (Steinthal, 1855, p. 232).

[89] "so bemerken wir, dass der Sprachforscher glücklicher gestellt ist, als wer die Geschichte einer sonstigen Erfindung erkundet, insofern die Gesetze, die heute noch beim Erlernen der Sprache sich in jedem Kinde wirksam zeigen, auch die treibenden Kräfte bei der Erfindung waren" (ibid. p. 233).

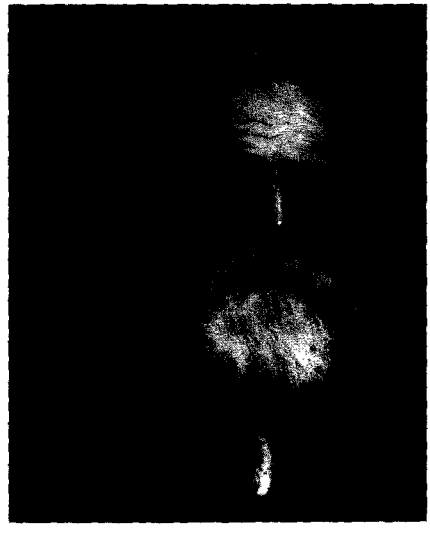


Fig. 2.1 Heymann Steinthal. UB der Humboldt Universität zu Berlin; Porträtsammlung; Steinhal, Heymann.

This is a forceful starting point. Language is genesis, and its genesis is psychological in nature. The development of these notions, however, became a heavy enterprise for Steinthal. His first book in 1855 was in fact a non-starter. It was unbalanced, often cryptic, and sometimes even unreadable. The monograph's first half is an ongoing diatribe against a somewhat prominent, but now forgotten philologist by the name of Carl Becker, who had performed the sin of confusing grammar and logic. Becker's work is "the emptiest nothing ever published."[90] Next, Steinthal turns to the core issue of (phylogenetic) language origins, developing his notions about the psychology of percept, image, concept, and spoken word, to which we will turn shortly, and idolizingly adopts Humboldt's vague notion of inner speech (*innere Sprache*). Finally, the book goes on to become a rather standard introduction to linguistics.

If anyone agreed that this book was a non-starter, it was Steinthal himself: "It is an occasional book, as all of my earlier publications, and really just a long-winded essay."[91] It is only fair to ignore it and to consider Steinthal's completely reworked presentation of his psychological approach to linguistics in his 1871 *Introduction to psychology and linguistics*. Another source of his ever developing ideas is *The origin of language*, which appeared in four editions from 1851 to 1888. Admittedly, neither of them is a pleasure to read—his style was "clumsy and colorless"[92]—but his later work is at least reasonably

[90] "dass ein Werk, wie Becker's Organism, . . . das leerste Nichts sein soll, das je veröffentlicht wurde" (Steinthal 1855, p. vi).

[91] "Es ist ein Gelegenheits-Buch, wie alle meine früheren Schriften, und eigentlich nur eine weitläufige Abhandlung" (Steinthal 1871, p. xii).

[92] Delbrück (1901, pp. 1–2) remarks: "Seine Ausdrucksweise ist schwerfällig und farblos; er hat die leidige Gewohnheit, einen Gegenstand, ehe er auf ihn stößt, in weiteren Kreisen zu umziehen, so dass die Geduld des Lesers, der gerne zur Sache kommen möchte, oft auf eine harte Probe gestellt wird."

well-organized. Let us first turn to Steinthal's psychology, then to his views on the genesis of language, and finally to his treatment of language dysfunctions.

Psychology

Steinthal's psychology is curiously ambivalent. Early in his career he was a staunch promoter of what was called *Völkerpsychologie*, which we will, hesitantly, translate as *ethnic psychology*.[93] Together with his brother-in-law and life-long dear friend Moritz Lazarus, Steinthal had in 1859 founded the new and enormously successful *Journal of Ethnic Psychology*.[94] In 1851 Lazarus had published a programmatic pamphlet arguing for the establishment of this new psychological subdiscipline. At the core of this program was the romantic notion of community spirit (*Volksgeist*). Peoples differ in their psychological identity. There is a characteristic commonality among their members in their psychological functioning, which expresses itself in their ethics, mythology, art, and so on, but in particular in their language. The new science should explore the laws that govern the genesis and functioning of such a community spirit and it should, in particular, study the regular ways in which the community spirit reflects itself in the psychological functioning of the individual.

One would thus expect Steinthal's psychology to be a *social* psychology of sorts. And indeed in his *Introduction* we read: "Hence, psychology does not in the first place study the mind of the individual, to question, in a second round, how such constituted individuals then unite in communalities; rather it starts the other way round with the human community and then questions how individuals develop therein."[95] But Steinthal, in his actual psychology as well as in his treatment of the language origins issue, completely ignores this noble maxim. The psychology in the *Introduction* exclusively deals with the internal functioning of the individual mind. It is, moreover, not an original psychology. Rather, it is a reworked version of Herbart's idea psychology of 1816. Steinthal's approach to the psychology of language was to use whatever he could find in existing psychology and apply it to the genetic issues of language. The psychology of Johann Friedrich Herbart (1776–1841)[96] was, for the larger part of the nineteenth century, the one dominant

[93] Following Blumenthal (1970); Steinthal (1877, p. 135), uses the term "psychische Ethnologie" as equivalent. Lazarus argued for *Völkerpsychologie* as a necessary addition to anthropology, which was at the time what we now call "physical anthropology," and to ethnology and ethnography. All of these were largely *colonial* sciences during the nineteenth century. Werner (1940) and others also used the term "ethnopsychology." In present-day terminology "cognitive anthropology" is a useful equivalent of *Völkerpsychologie*.

[94] The *Zeitschrift für Völkerpsychologie* appeared in 20 volumes from 1859 to 1890.

[95] "Die Psychologie also erforscht nicht zuerst das seelische Individuum und fragt in einem zweiten Teile, wie solche so gewordenen Individuen sich darauf zu Gemeinsamkeiten zusammentun; sondern sie beginnt umgekehrt mit der menschlichen Gesellschaft und fragt dann, wie sich innerhalb derselben Individualitäten entwickeln" (Steinthal 1881, p.109).

[96] Herbart's *Lehrbuch zur Psychologie* of 1816 was followed by a much more technical and voluminous companion work *Psychologie als Wissenschaft* in 1824.

psychological theory. In 1809 Herbart had been appointed to Kant's former chair in Königsberg. Later he moved to Göttingen. He published widely in (idealistic) philosophy, is generally considered to be the father of scientific education and was probably the first, self-proclaimed, psychologist. Steinthal chose to be a Herbartian in his psychology.

Psychology is the science of inner life, which is in part *conscious* life. We can be conscious of ideas, of affects, and of desires. Ideas enter consciousness from sensory impressions or from memory. Sensory ideas have spatial and temporal form properties, visual ideas are mainly spatial, while auditory ideas are mainly temporal. More complex conceptual ideas have as dominant forms part/whole and thing/property relations. Ideas are always on the move and always interact. Psychology formulates the laws of this mental mechanics.

Steinthal, following Herbart, uses the term *Vorstellung* (which we translate by "idea") to denote the psychological entity to which these laws apply. But he is somewhat ambivalent here. His wider use of *Vorstellung* is indeed any content that can move in and out of consciousness, be it a sensory impression, a visual memory, concept, or whatever. But in his psychology of language he introduces a much narrower definition. *Vorstellung* is anything in mind that can be denoted by a word. This, however, makes no difference to mental mechanics.

An idea can be in one of two states, conscious or unconscious. Herbart had formalized the notion of unconsciousness.[97] Depending on their state of energy or inhibition, ideas can move from one mental compartment into another. Consciousness is like a stage on which actors appear briefly, and from which they disappear when their part is over. Consciousness is *narrow* for Herbart, only few players can be on stage simultaneously. Steinthal sharpens this notion drastically: there is at most one actor on stage at any time. You cannot entertain more than a single idea in consciousness.

Ideas interact in various ways. One mechanism is that of *fusion.* If you already posses a visual impression, an idea of a person, and you see that person again, the new impression will not create a new idea. Rather the visually evoked idea will fuse with the already existing one. So, A + A = A in Steinthal's mathematics. This is, essentially, the mechanism of recognition. Any two ideas that consciousness does not distinguish will fuse. Fusion is also the basis of abstraction, because it ignores all differences between ideas that do not reach consciousness. Of course, we can *learn* to distinguish and thus enrich our stock of ideas. This is of much practical use in education.

A second mechanism is *entanglement.* Two ideas that share properties may bind to one another, such as the idea of "animal" and the idea of "dog." Their shared properties (ideas in themselves) tend to fuse, but where they are different there is repulsion. This, clearly, is a kind of mental chemistry. Ideas can get entangled into ever larger configurations. The different kinds of binding entirely depend on the kind of properties that get fused. Fusion

[97] The notion had been around for some time in the psychiatric literature (cf. Ellenberger 1970).

and repulsion are always active in unconsciousness, creating ever larger complexes of ideas.

Association is the process by which one idea gains energy by being bound to other active ideas. Gaining energy is a condition for entering consciousness. An idea can reach consciousness while everything it is bound to stays unconscious. This is, as we will see, an important process in the use of language. This spreading of energy among associated ideas is usually asymmetric. It is easier to retrieve a foreign word's translation in your own native language, than the other way round.

As conglomerates of ideas become more complex, their mental mechanics also become more involved. Although all processes in a living organism obey chemical and physical laws, mental mechanics functions at some higher level of aggregation. The higher level of aggregation is called *apperception.*[98] By experience and education we have acquired our own characteristic conglomerates of ideas. New ideas are linked to and incorporated into these conglomerates in specific ways that are often characteristic of the individual or of a community of individuals. This is how we integrate new information. The absorbing mass of ideas is technically called the "mass of apperception."[99] It is, as it were, the individual's epistemology. The study of apperception is, next to the study of affect and desire, psychology's main assignment. It should be noticed that this mental mechanics was by no means generally shared in the psychology of these days. William James wrote, "I must confess that to my mind there is something almost hideous in the glib Herbartian jargon about *Vorstellungsmassen* and their *Hemmungen* and *Hemmungssummen*, and *sinken* and *erheben* and *schweben*, and *Verschmeltzungen* and *Complexionen*." And he added in a footnote: "The most burdensome and utterly gratuitous of them are perhaps Steinthal's, in his *Einleitung in die Psychologie*."[100]

There is still more to consider. According to Steinthal the psychological mechanics can control the movements of the body, but how? Here Steinthal turned to Hermann Lotze (William James's favorite psychologist), who in 1852 had published the first handbook of medical psychology. Its first volume dealt with physiological psychology (a term introduced by Lotze), which is the physiology of the soul. Lotze (1817–1881) succeeded Herbart in Göttingen, but he hated mental mechanics. His reflex arc psychology is hardly compatible with Herbart's, but this apparently did not bother Steinthal. What he needed for his work on language origins was a respectable theory of reflexive movements and Lotze could provide it.

Bodily movements are either vegetative, like breathing, or animal, like walking. Speaking is an animal movement, which is, however, closely connected to the breathing system. A reflexive movement is one in which some sensory innervation produces a sensation in the central organ, whose innervation, if strong enough, is then passed on to some motor

[98] Steinthal strictly follows Herbart's use of the term. It was Leibniz who had introduced it.

[99] "Apperzeptionsmasse."

[100] James (1890, Vol. I, p. 604).

nerve, releasing a movement. Also other innervations of the central organ, such as affects, desires and memories, often produce involuntary motor movements. Strictly speaking, any movement is a reflex movement, because it is the mechanical outcome of some central innervation. Speech movements originate from a specific center in the brain, "whose localization can be exactly and firmly determined."[101] However, it is better to reserve the term "reflex" for involuntary movements. Among them are also involuntary imitations such as yawning. Perceived or imagined movements often release the movement itself as if it were intentional.[102] Children's imitations of heard speech sounds are largely reflexive. Reflexive sympathy action is at the core of many social phenomena, as in dancing, parading, or crusading for that matter.

Voluntary movements are caused by a mental mechanics that affects the motor system. Here Steinthal had to link the unlinkable systems of Herbart and Lotze. Consciousness can never affect the interaction between body and natural environment: "no consciousness can, by itself, directly check or boost this purely natural process of interaction."[103] But then, what is the use of consciousness? Is it just an epiphenomenon? One thing it does, according to Steinthal, is to establish an intention (*Absicht*). "The mind dictates, the body executes." More specifically, intentions can affect the course of nervous innervation. Every sensation projects onto a specific localization in the central organ and every movement has its specific starting point in the central organ. Normally, sensory innervation runs some established pathway through the central organ and into the motor nerves. But if such a pathway does not yet exist in the central organ, an intention can establish it. The intention is the idea of a movement (*Bewegungsvorstellung*), which in itself possesses motor force, a "movement tendency."[104] In this way consciousness can connect any sensation to any motor action. By repeatedly doing so, the new pathway through the central organ becomes established and consciousness is no longer necessary for the execution of this newly created *associative movement*; it will run like any reflex movement. This is the foundation of skilled movements, such as the musician's moving his fingers in response to the visual pattern of notes.

These are the psychological tools on which Steinthal based his genetic theory of language.

Language genesis

Steinthal's views on the origins of language changed and developed. He was always willing to admit and then extensively discuss his earlier mistakes. A constant element,

[101] "dessen Lage sich sogar genau und bestimmt angeben lässt" (Steinthal 1881 edition, p. 271).

[102] Here Steinthal refers to the work of Johannes Müller (1833–1840). The historical hobbyist will find here an anticipation of Rizzolatti's "mirror neuron" notion.

[103] "Und kein Bewusstsein kann unmittelbar aus sich diesen Wechsel als reinen Natur-Process hemmen oder fördern" (ibid. p. 92).

[104] Steinthal refers again to Müller in his discussion of the movement tendencies caused by *Bewegungsvorstellungen* (ibid. p. 288).

however, is the way in which, according to him, first speech sounds emerged. Here the notion of inner speech form (*innere Sprachform*) has to do part of the work. As we saw, Humboldt hardly defined the notion; for him it is anything in the process of language that is not the speech sound event itself (the outer speech form): call it meaning, call it grammar, call it thought. Steinthal makes it a psychological factor. Here is primeval man who perceives something exciting that causes a sound reflex, just as Herder had described a century before. Any strong nervous innervation will "flow out," according to Lotze. This is a purely mechanical event. The sound reflex can be rather more interjectional, like a chuckle when we observe something funny, or something more like mimicry in spontaneous, reflexive sound imitations. In the end Steinthal calls them all onomatopoeic. An essential stage in this kind of sound reflex is the affect (*Gefühl*) that is aroused by the sensory impression in the central organ. It is this affect that mediates the motor response. Our ancestor now perceives the self-produced sound more or less simultaneously with the sensory event, which is still in consciousness. Since both share affect, they get associated. This momentarily conscious association between sound and perceptual event Steinthal calls the inner speech form of the sound (which itself is the outer speech form). It is a primary kind of apperception. Inner speech form is the consciousness of the link between self-produced sound and perceived event, a primary form of self-consciousness. The link is an affective one. The range of affects we can entertain, such as pleasure, pain, excitement, and fear, is rather limited, however. Hence, the set of primary onomatopoeic roots will be small as well.

As primeval man evolves, so does his apperception mass. Doing his work in interaction with others, he develops an ever richer network of ideas. Thanks to his upright posture, he is in an advantageous position here, compared to other animals (Herder again!). The next stage, then, in root formation is the derivation of roots from roots. New percepts are incorporated in conglomerates of already available ideas. If there exists a root for an available idea, then a new impression that associates to that idea will also associate to the already existing root. From this point on, the ethno-psychological history will do its work, deriving roots from roots and further differentiating meanings and their sound associations. In all cases the *primary* inner speech, i.e., the *affective* link between sound and percept, becomes lost.

Inner speech, however, is always there. It is the consciousness of the connection of a word to its apperceptive mass. The apperceptive mass is, psychologically, the word's meaning. It is almost never in consciousness because of its complexity. Consciousness is too narrow for it. What consciousness can contain is the internal speech form; it is the consciousness of the word's connection to the dark apperceptive mass in the mind's unconscious compartment. Steinthal now drastically narrows the notion of "idea" (*Vorstellung*). In the civilized language user practically any idea is a word-idea. Any idea in consciousness is just the abstract reference of a word to its unconscious meaning complex. This internal speech form has little or no content. For the speaker it is no more than a sign in consciousness, which got there through activation by the always active underlying, unconscious meaning complex. For the hearer it is also the conscious word

sign, which can activate the same underlying apperceptive mass, which is, psychologically, the word's meaning.

Unlike Max Müller, Steinthal wholeheartedly accepted the new Darwinian theory of human evolution. Linguistic mankind must have evolved from the animal world. In reaction, he revised his original (1855) genetic approach. Originally he had hoped and believed that we might reconstruct the origins of language from carefully observing how the child acquires language (ontogenesis) and from how the speaker generates language (microgenesis). This approach will not work any longer because modern human beings are biologically different from primeval man. We are today linguistic animals, but primeval man was not. What a modern child can acquire a primeval child could not. Our animal ancestors would not have been able to speak at all. The linguistic mind must have evolved slowly over thousands of years.

Why then did this linguistic ability germinate in the human species but not in closely related other ones? Here Steinthal mentions the upright position, which made us a better, more distinctive perceiver, and he makes the rather obligatory reference to social and family life.[105] But there is a noticeable innovation in Steinthal's thinking too. Roots alone, he argues, will never be language. For language to evolve, conditions must be met for the evolution of the *sentence,* the ability to relate a subject and a predicate in a judgment. Here again, it must have been the more developed apperceptive abilities in our species that made this possible. Both in the child and in primeval man perceptions are initially undivided, as they are in animals. You see a nursing woman (Steinthal's example) and that is it. You see another nursing woman and that is it. You see the first woman playing with the child and that is it. But gradually an apperception mass will be built up. All percepts of the same woman doing different things will get entangled through their common element, the particular woman. Similarly, all percepts of nursing by different women and by suckling animals will get entangled through their common element, nursing. This builds up two apperceptive conglomerates: the nursing one and the (particular) woman one. When you now perceive the particular woman nursing, both conglomerates are activated. In each of them that woman's nursing will be the most active element. All other incompatible elements will be inhibited. Hence, the percept of the nursing woman will simultaneously activate *two* apperceptions, "the particular woman" and "the nursing one." Here are the seeds of subject and predicate.[106] In other words, primeval man's remarkable apperceptive abilities made it possible, slowly, for him to perceive the world predicatively.

[105] "Nur in Gesellschaft mit einem Anderen erzeugt er Sprache"; "Zu wissen, was der Andre tut, wo der Andre ist; dem Dritten zu sagen, was jener tut: das ist der mächtigste Trieb der Sprache." (Steinthal 1881, p. 406).

[106] Steinthal (1881) develops this argument over many pages of mathematical formulas. He tried to be a real Herbartian. For Herbart, psychology had to be mathematical. An able mathematician himself, he developed a sophisticated calculus of mental mechanics. Steinthal clearly lacks that mathematical ability.

One of Steinthal's most original ideas concerns the microgenesis of language, the way in which language mediates thought. Steinthal departs from his notion that our conscious ideas are largely word ideas, which are practically without content. They are only lightweight references to the underlying, unconscious apperceptive structures: their "meanings."[107] Speech is our sixth sense, the sense by which we can observe, and have access to the contents of, our mind. For the speaker these contents present themselves as conscious word ideas. For the hearer perceived words activate the underlying contents. Any thinking, any creative mental process is unconscious, according to Steinthal. It is the never ending apperceptive interaction of association and dissociation among unconscious conglomerates. These highly complex events are consciously represented as words and sentences. Consciousness need not carry the weight of mental content, but it *represents* it. If we hear "hundred sheep," we do not get a detailed conscious visual image of a hundred sheep, but there will be underlying activation of our notions (conglomerates) of "hundred" and of "sheep" and they will apperceptively interact. In this way, lightweight, *narrow* consciousness can affect, drive heavyweight thinking. Similar ideas would recur time and again in psycholinguistics. There is, for instance, the discovery of imageless thought in the Würzburg school (see Chapter 7), and there is George Miller's theory of recoding (see Chapter 15): the way in which limited working memory can represent complex information. Indeed, Steinthal's theory of consciousness is very much a theory of working memory: *verbal* working memory.

Disorders of language and speech

When you read Steinthal you sometimes wonder whether he is developing an empirical science. He rarely discusses detailed linguistic etymology. He provides the occasional example of a child's utterance, but he never conducts or proposes experiments. By and large his work is theoretical. However, according to Steinthal, "when experiments are not allowed or impossible,"[108] language pathology can tell us something about the conditions of the normal speaking process. The last few sections of Steinthal's *Introduction* discuss various language and speech disorder syndromes. Here, Steinthal takes all his cited cases from three unnamed inaugural dissertations of the Berlin medical faculty, published between 1867 and 1869[109] and this is unchanged in the second, 1881 edition of his book. That explains why neither Broca nor Wernicke are referenced by Steinthal. Broca's 1861/1865 discovery of the left-hemisphere locus of articulated speech had, internationally, caught the imagination, but Steinthal missed it in both the 1871 and 1881 editions of his *Introduction*. Carl Wernicke had published his famous monograph in 1874 and moved to Berlin's *Charité* hospital in 1875, a few minutes' walk from Berlin University. But he was, apparently, not noticed by Steinthal, who did not mention him in his second edition. Or was Steinthal upset by young Wernicke's making fun of him in his monograph, in

[107] Steinthal (1881, p. 436).

[108] "in Fällen wo Experimente unerlaubt oder unmöglich sind" (ibid. pp. 453–454).

[109] According to his footnote, ibid. p. 480.

particular of his fallacy that language arises from conceptual development and its sensory origins, not from imitation? "If that were the case, then someone born blind would have much more reason to become mute than someone born deaf."[110]

Despite not mentioning them by name, Broca's and Wernicke's (unnamed) aphasic syndromes are exemplified and discussed by Steinthal. In the former syndrome, either the word image cannot be activated or the wrong one is, while the underlying thought is intact. In the latter syndrome there is an inability to build sentences, even if the underlying judgment is intact; *akataphasia* is the term Steinthal proposes for this agrammatism. Examples of this latter syndrome show, according to Steinthal, a disorder of intelligence. Apart from aphasic syndromes, Steinthal reviews stuttering, anarthria and apraxia. All these disorders must find their cause in disorders of relevant centers in the brain. There must be a center for intelligence. There may be another one for the motor production of speech sounds (but then its disorder must also cause problems with tongue, swallowing, and facial movements because the same muscles are involved, which is mostly not the case). And there must be a center for the psychic side of language. "Our psychology," Steinthal tells "the doctors," "is the necessary preliminary work for a physiology of the brain and enables a rational psychiatry."[111]

What is more interesting is Steinthal's discussion of speech disorders in healthy people, where similar phenomena can be observed. He mentions the tip-of-the-tongue phenomenon: names or other words do not want to come to mind, which William James was going to call "a gap that is intensively active." We mix up words, we make slips of the pen, and often we are unable to correctly complete a sentence. These observations lead Steinthal to distinguish three components in the mechanics of speaking. Speaking is somewhat like playing the organ. The composer provides the musical content, the performer the psychological mechanics, and the organ the physical mechanics. When, in the speaking process, the content is not picked up correctly by the psychological mechanics, the word idea is not reproduced or the appropriate syntax is not constructed. Here Steinthal makes the interesting observation that a sentence can never be conscious as a whole; consciousness can contain no more than one word idea. To compose a sentence, therefore, requires the underlying meaning conglomerates to keep "vibrating," so that their ordering and mutual relations can be established by the unconscious speaking mechanism.[112] If the psychological mechanism itself is deranged, words or sounds get exchanged or mixed up. If the motor mechanics do not precisely follow the psychological mechanics, stuttering or anarthria results, but also normal phonetic phenomena such as assimilation or reduction can occur. And if the motor mechanism itself fails, stammering results. There is a resemblance here to the three-way distinction that Adolf Kussmaul

[110] "Verhielte es sich so, so hätte der Blindgeborene viel mehr Ursache stumm zu werden als der taub Geborene" (Wernicke 1874, p. 20).

[111] "So ist auch unsre Psychologie die notwendige Vorarbeit für eine Physiologie des Gehirns und lässt auch eine rationale Psychiatrie zu" (ibid. p. 473).

[112] There is a similarity here to Haarmann and Kolk's (1991) theory of agrammatism.

introduced in 1876: preparation, diction and articulation. In fact, Kussmaul's excellent treatise (see Chapter 3) refers repeatedly to Steinthal's *Introduction*. In particular he picks up Steinthal's notion of akataphasia, i.e., agrammatism.

Steinthal's invention

Steinthal had a life-long allegiance to Humboldt's intellectual inheritance. Language is activity (*Tätigkeit*), a process rather than a product. Steinthal conceived of the idea of explaining this genesis of language, whether phylogenetic, ontogenetic or microgenetic, on psychological principles. Linguistics had to become a psychological science. This was his grand project. But for the historian, Steinthal is a moving target. In his writings we observe the process of his thinking, rather than any finished product. The psychology he invented for his purpose was an eclectic mix of Herbart's mental mechanics, Lotze's reflex theory and the ethnic psychology he had, together with Moritz Lazarus, put on the map. The latter, however, was a shallow addition. Nowhere is there any account of how social interaction generates or shapes language. His was a general psychology, a theory of the individual's mental processes.

A comparative linguist himself, Steinthal was an expert on linguistic matters. He was also a self-taught and self-made theoretical psychologist of considerable talent. His grand endeavor to provide the science of language with a psychological base evidently justifies calling Heymann Steinthal a pioneer of the psychology of language.

Why did he himself never speak of a "psychology of language"? The real reason was no doubt that for him all linguistic explanation was psychological, whether phylogenetic, ontogenetic, or microgenetic. It made no sense to single out the latter two only as psychology of language. For Steinthal the study of language was one or it was none.

By the end of the nineteenth century an explicit psychology of language had emerged. It was not only rooted in the psychological linguistics discussed in the present chapter, but also in language pathology, language ontogeny and in newly developed tools of measuring the mind. These will concern us in the following two chapters.

Retrospect

The need for a psychology of language emerged in linguistics. The discovery of the family relations among Indo-European languages strongly promoted the Romantic notion of evolution. To some extent language scientists took the lead in the development of evolution theory. The issue of the origin of languages became paramount. A thoroughly data-based empirical science developed to trace the Indo-European languages back to their roots: their primordial repository of meaningful sound elements. Some linguists would leave it at that, but others wanted to cross the root barrier and explain how these roots had arisen to start with. That, clearly, was a psychological issue. A colorful linguistic folk psychology arose, suggesting an array of causes for the emergence of meaningful sounds: imitation of natural sounds, affective interjections, phonetic symbolism, and more. Academic psychology could not provide much more than Herbart's mathematical theory of mental mechanics and associationist reflex theory, largely

developed in medical quarters. It was Hermann Steinthal who adopted these two disparate theories in constructing a psychology of language that could aim at an explanation of language origins. In retrospect, this must have been a major theoretical struggle. But it did substantially advance the discipline. Not only were many of Steinthal's notions adopted by leading scientists, such as Kussmaul and Monakow, but more importantly Wilhelm Wundt developed his unified psychology of language almost "in dialogue" with Steinthal. In *Die Sprache* (1900) Wundt rejects Steinthal's notion of "mechanistic" apperception and replaces it with his own "volitional" notion of apperception. We will return to this idea in Chapter 6; here it suffices to notice that there is a clear continuity from Steinthal's theory to Wundt's influential psychological theory of language.

Chapter 3

From bumps to diagrams: Tracing language in the brain

"Their knowledge with respect to the structure of the brain, and its inherent functions is equally very incomplete,"[1] was Franz Joseph Gall's verdict in 1818 about his predecessors in physiology. And indeed, at the end of the eighteenth century very little was known about the brain's anatomy and its relation to mental functions.

This was, *a fortiori*, true for the relations between brain and language. Cases of aphasia, both motor and sensory, had been described in the medical literature since the fifteenth century. In some cases, such as Johann Schmidt's (1676), the patient was described as right hemiplegic,[2] but relations between language and brain physiology had not been empirically studied. That would drastically change during the nineteenth century. A fascinating history of empirical discovery, theoretical imagination, dead ends, and priority fights was about to unfold, which by the end of the century had produced the foundations on which our present knowledge of language in the brain is based.

That history was as much an exercise in uncovering the neural anatomy involved in language functions as one of formulating the mental operations and functions themselves. The former aspect we will call "neural architecture," the latter "functional architecture."

The physiognomic beginnings

The facial expression of emotion has fascinated artists over the centuries. It became a focal topic in the work of da Vinci, Rembrandt, Hogarth, and others. Especially relevant in the present context are the "character heads" of rococo sculptor Franz Xavier Messerschmidt (1736–1783), who received his training at the Viennese Academy of Fine Arts. In a baroque way they express emotions and characters. The notion that human character is discernible by a person's physical appearance was not only around in art, but also in philosophy and science. Johann Kaspar Lavater (1741–1801), a Swiss theologian, published an influential, but also much criticized, four-volume work on this "natural language of faces," this "godly alphabet."[3] His cover term for this science was "physiognomy." It was Franz

[1] "Leurs connoissances relativement à la structure du cerveau, et aux fonctions qui lui sont propres, étoient également très-incomplètes" (Gall 1818, Vol. III, p. 40).

[2] See Tesak and Code (2008) for a review of these early studies.

[3] "Alphabeth zur Entzieferung der unwillkührlichen Natursprache im Antlitze," "Buchstaben dieses göttlichen Alphabeths" (Lavater 1784, p. 10).

Fig. 3.1 Franz Joseph Gall.

Joseph Gall who would provide a new twist to this science by relating the shape of a person's skull and brain to his prominent mental faculties, including language faculties.

Franz Joseph Gall (1758–1828, Fig. 3.1), from Pforzheim in Baden, received his MD in Vienna in 1785.[4] He would stay there till 1805, increasingly involved in autopsies and self-taught brain anatomy. He became recognized as an absolute leader in the anatomical dissection of the brain. Flourens, one of his major opponents, called Gall "a profound observer whose genius has opened for us the study of the anatomy and physiology of the brain. One can estimate his work only at its true value if one is acquainted with the depth of ignorance which prevailed when he appeared. I shall never forget the impression I received the first time I saw Gall dissect a brain. It seemed to me as if I had never seen that organ."[5] In Vienna Gall also famously lectured on brain functions. We can hardly imagine now what courage that required. Increasingly, Gall became denounced as a "materialist." In a handwritten letter of December 24, 1801 Emperor Franz prohibited Gall from teaching; his lectures endangered religion. But the lectures had already attracted young Johann Caspar Spurzheim (1776–1832) to Vienna in 1799. He became Gall's assistant in brain dissection and then co-author of the first two volumes of *Anatomy and physiology of the nervous system* (1810–1819), which Gall was later to complete by himself with an additional two volumes.[6] This four-volume work was an impressive accomplishment not only for its enormous size (over 1250 pages in folio edition, plus an atlas with 100 carefully engraved pictures), but also for its content. It presented the most detailed

[4] i.e., the same year Wolfgang Amadeus Mozart, two years older, published his Haydn quartets there.

[5] Cited by Marshall and Gurd (1994, p. 290).

[6] Gall and Spurzheim (1810, 1812), Gall (1818, 1819). Spurzheim's contribution was limited to the first volume and the first half of the second volume.

anatomy of the nervous system so far, including comparative analyses of mammalian and avian brains. The text reveals the hand of the expert anatomist, and the graphical plates are meticulous in their anatomical detail. In addition, its ambition was to provide a comprehensive account of our mental faculties and their relation to brain anatomy. Gall also did not eschew physiological, philosophical, and historical detail. Comprehensiveness was clearly his aim in writing this treatise.

After much wandering around through Europe, visiting asylums, seeing psychiatric patients, lecturing, and advising (see Eling *et al.* 2011), Gall settled in Paris in 1807, still accompanied by his assistant Spurzheim. Gall stayed there for the rest of his life. Spurzheim went his own way in 1813, going to London, Edinburgh, back to Paris, then London again, and ending up in Boston, where he died a few months after his arrival. Indeed, Spurzheim became somewhat of a traveling salesman for the new doctrine, which Gall had labeled "a system of physiognomy," but Spurzheim called, to Gall's irritation, "phrenology."[7]

Phrenology was as much a doctrine about neural architecture as about functional architecture. In fact, the approach to neural architecture was entirely guided by the functional doctrine. That was a matter of principle for Gall: "show me the basic forces of the soul, and I will find the organ and seat thereof."[8] The mind, according to Gall and Spurzheim, is a hierarchical organization of faculties. In Spurzheim's formulation,[9] they are of two orders: feelings and intellect. The feelings, in turn, consist of propensities and sentiments. Amativeness or physical love is a propensity. Veneration, the faculty of religion, is a sentiment. Intellectual faculties divide into knowing and reflecting faculties. The senses are knowing faculties, language and number are reflecting faculties. Gall distinguished 27 faculties of mind, but Spurzheim expanded the list, again to Gall's irritation, to 37.

Gall was not the inventor of faculty psychology. Christian von Wolff (1679–1754) in Halle and Thomas Reid (1710–1796) in Edinburgh were predecessors. Their faculties, however, were general ones, such as sensation, imagination, attention, abstraction, or memory. Gall, by contrast, construed his faculties in terms of specific content, such as faculties for language, music, mathematics, etc. In addition, Gall was the first to attempt to make this an empirical science. For Gall, faculties are innate, like instincts in animals. This is quite different for ideas. All ideas enter the mind through the senses: "there exists no internal source for our sensations and our ideas."[10] But although the faculties are

[7] Gall detested terms such as "craniology" and "phrenology"; Spurzheim claimed "phrenology" to be a term of his own invention, but, according to Hunt, it was introduced by Dr. Forster (see Hunt 1869, p. 202).

[8] ". . . indiquez-moi les forces fondamentales de l'âme, et je trouverai l'organe et le siége de chacune" (Gall 1818, Vol. III, p. 42).

[9] I follow Spurzheim (1815) here. Gall and Spurzheim (1812, p. 321) distinguishes two classes: Class I contains the sensory and voluntary movement faculties, Class II contains the moral qualities as well as the intellectual functions, tendencies, and sentiments.

[10] "Qu'il n'existe aucune source intérieure de nos sensations et de nos idées" (Gall and Spurzheim 1812, Vol. 2, p. 50).

innate, they are not heritable: "the properties of mind and soul are not transmitted . . . to children and grandchildren."[11] Faculties are also mutually independent: "The capabilities and propensities per se among themselves differ essentially and independently from each other."[12] This is particularly clear for the senses: "while the tired eye is resting, one can hear attentively."[13] "In aging, hearing usually deteriorates before vision, while taste often remains excellent."[14] But it holds for other faculties as well. "The memory of music has its organ in the organ of music; the memory of numbers in the organ of calculation; the memory of places in the organ of the locality sense"[15] Each faculty has its own resources. There is no general faculty of memory[16] or intellect.

The empirical approach was to show the independence of a faculty by spotting people who are exceptionally gifted in one faculty but mediocre in everything else. "It is much easier to distinguish the organ of music, of architecture, of poetry in people who are otherwise limited, than among those who posses a certain combination of talents."[17] Or, alternatively, one can go for people who have a markedly defective faculty, being otherwise normal, such as those who lack a sense of number. The empirical search is for such clear cases. But then, the eventual proof of the pudding is in the neural architecture. Faculties are situated in the brain. Each faculty has its own specific organ, in a distinct location. Hence the clear cases of a particular faculty should share a marked organ in some specific region of the brain. For instance, those who are very gifted in verbal memory share a strong and therefore *relatively large* word-memory organ. This was, in fact, one of the first organs discovered by Gall. Spurzheim describes how, as a 13-year-old schoolboy, Gall observed that school-fellows who could learn everything by heart had very prominent eyes. In Gall's own words: "I then noticed that all had big salient eyes."[18] Later he realized that the organ for words must be right behind them, in the anterior part of the brain, pushing out the eyes, as it were. The closely related philological faculty has an adjacent organ in the frontal cortex. If such a common bump is not found in similarly talented

[11] "Que les propriétés de l'esprit et de l'ame ne se transmettent pas, comme l'organisation, aux enfans et aux petit-enfans" (Thesis 12, Vol. 2, p. 49).

[12] Gall (1798); translation by Heeschen in Eling (1994, p. 20).

[13] Gall (ibid. p. 21).

[14] Gall (ibid. p. 23).

[15] "La mémoire de la musique a son organe dans l'organe de la musique; la mémoire des chiffres dans l'organe du calcul; la mémoire des lieux dans l'organe dus sens de localité" (Gall 1818, Vol. 4, p. 10).

[16] Gall's former student Bouillaud (1825, p. 42) castigated Gall for having placed a *general faculty* of memory in the frontal lobes. Here Gall is in "manifest contradiction with himself," but I definitely cannot read a general faculty in Gall's text.

[17] "Il est beaucoup plus facile de distinguer l'organe de la musique, de l'architecture, de la poésie, chez des hommes bornés du reste, que chez ceux qui possèdent une certaine réunion de talents" (Gall 1818, Vol. 3, p. 55). Double dissociation is in the offing here.

[18] Spurzheim (1815, p. 445). "Je remarquois alors que tous avoient de grands yeux sallans" (Gall and Spurzheim 1810, Vol. 1, p. I).

people, the faculty is in doubt. There was even stronger evidence to be obtained, according to Gall. Not only should those particularly gifted in language share a bulge, but those who clearly lack the same talent should show a depression in the same region. Spurzheim fused Gall's two language organs into one, but maintained the localization: in the anterior lobe, just behind the eyes.

Both Gall and Spurzheim assumed that the organs of mind would come in pairs, arranged perfectly symmetrically over the two hemispheres.[19] This assumption of symmetrical representation of functions they shared with the entire physiological community. This was known as Bichat's law of symmetry. He had observed that all parts of the brain "resemble each other on every side," adding that "two parts essentially alike in their structure, cannot be different in their mode of acting."[20] Hence, Gall arranged his 27 faculties symmetrically over 54 regions (Fig. 3.2a).

Gall did not deny the importance of relating brain damage (determined in autopsy) to the special disorder of a faculty a patient had suffered from. His treatise presents myriad pathological cases of this kind. In treating the faculty of memory for words, for instance, Gall described a number of patients who had partly or fully lost their memory for words. One of them is obviously an anomia patient, the first brain-localized description of motor aphasia, half a century before Broca. The patient was an officer, hit by a foil "in the midst of left canine tooth region, close to the nostril, in oblique direction from below up," penetrating some 3.5 inches "into the internal posterior of the left frontal lobe, in such a way as to reach the anterior part of the mesolobe." The patient was right hemiplegic and "the memory for nouns had been totally extinguished."[21] However, rarely would such damage have affected the region of a single faculty. There is, instead, a much more promising way of going about testing mental faculties and localizing their organs in the brain. Innate brain protrusions would show on the face of the skull. Select your clear cases, make a cast of their skull, and run your comparative measurements. No lack of data here: many people with special gifts spontaneously approached Gall, Spurzheim, or other phrenologists to have themselves diagnosed. When Spurzheim died in 1832, he owned at least 300 casts and 100 skulls. Phrenologist James DeVille of London owned no less than 5000 casts and skulls.[22]

[19] "All specific systems of the brain are double, like those of the spinal chord and of the senses" ("Tous les systèmes particuliérs du cerveau sont doubles, comme ceux de la colonne vertébrale et des sens.") (Gall and Spurzheim 1810, Vol. 1, p. 225).

[20] Cited by Harris (1999, p. 11).

[21] ". . . un coup à la partie moyenne de la région canine gauche, près de l'aile du nez, dans une direction oblique de bas en haut', dans la partie interne postérieure du lobe antérieure gauche du cerveau, de manière à se rapprocher de la partie antérieure du mésolobe." "un coup de fleuret qui lui fit perdre totalement la mémoire des noms." ". . . sens des mots, sens de noms, mémoire des mots, mémoire verbale" and "sens du langage de parole, talent de philologie." (Gall 1818, Vol. 4, p. 54).

[22] See Kaufman and Basden (1995). Gall's own skull went to the National History Museum in Paris. Gall's friend Dr. Fossati, provided the following cranioscopic diagnosis: ". . . locality, sense of persons, language, number, order, tune, colour, constructiveness, were all feebly developed; whilst comparison, causality, individuality, eventuality, and firmness were uncommonly large." (Hunt 1869, p. 204).

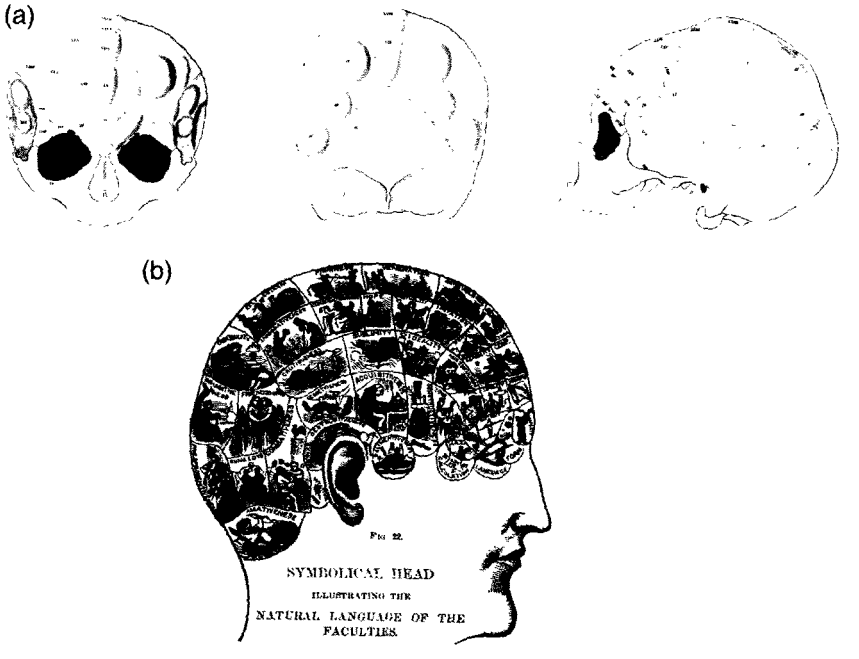


Fig. 3.2 (a) Gall's three plates depicting regions on the skull where underlying (numbered) cerebral convolutions could leave their mark, if the corresponding faculty was over- or underdeveloped. (b) Spurzheim's scheme in one of many popularized versions. Here Gall's two faculties "word sense" and "spoken language"[23] are replaced by a single language faculty, which can bulge below the eye.

This was a genuine research program. Gall could have been right about his protruding organs of mind and their visible marks on the skull. But alas, it was not to be. Phrenology was a short-lived fad. The Edinburgh Phrenological Society, one of the largest and most active among some 40 such societies around the world, was established in 1820 by the Combe brothers and closed in the 1850s.[24] Right from the beginning, and ever increasing over the years, there were accusations of quackery. As early as 1815, the *Edinburgh Review* wrote: "We look upon the whole doctrines taught by these modern peripatetics, (Drs Gall and Spurzheim), anatomical, physiological, and physiognomical, as a piece of thorough quackery from beginning to end."[25] Interestingly, Gall himself accused Spurzheim of scientific misconduct. Gall opened his single-authored Volume 3 with a full-on attack of his former co-author. First off, there is an accusation of plagiarism. Of the 361 pages of

[23] "... sens des mots, sens de noms, mémoire des mots, mémoire verbale" (Gall 1819, Vol. 4, p. 48) and "sens du langage de parole, talent de philologie" (ibid. p. 55).

[24] Hunt (1869), p. 204.

[25] From Marshall and Gurd (1994, p. 289).

Spurzheim's book *Observations on phrenology,* which had just appeared,[26] no less than 246 had been copied, without reference, from their jointly authored text: "Others have already accused him of plagiarism: it is at least very ingenious to make books by clips of the scissors."[27] But worse, according to Gall, Spurzheim had added faculties that had not been empirically established, such as benevolence, hopefulness, and supernaturality.[28] Another mistake Spurzheim made was to add the senses as faculties, something they are not: they *feed* the intellectual faculties, but they are independent organs, not faculties. Furthermore, Gall argued that Spurzheim's new nomenclature for most faculties is misleading. For instance, we share many faculties with other animals, and so the faculty's label should be equally applicable to man and animal. It is wrong, therefore, to replace the instinct of "propagation" by "amativeness." And the complaints and accusations continue for 16 folio pages. Indeed, Spurzheim was very keen to propagate and popularize his "phrenology." Practically all phrenological diagrams (and they were produced by the hundreds) followed Spurzheim's 37 faculty scheme and nomenclature. Ever more beautiful phrenological heads, such as the one in Figure 3.2b, did not fail to capture the popular imagination.

Still, when you read the original nineteenth-century papers and treatises on language and the brain, Gall is, as a rule, praised rather than ridiculed. He is praised for his superb brain anatomy, for his localizational approach to mental functions in the brain, for his efforts to relate neural and functional architectures, and for his localization of the language faculty in the anterior brain. The distinguished anatomist Samuel Solly wrote in 1847: "Every honest and erudite anatomist must acknowledge that we are indebted mainly to Gall and Spurzheim for the improvements which have been made in our mode of studying the brain."[29] Bunge (1905) wrote a convincing rehabilitation of Gall, as the fabulous brain anatomist and as the real pioneer of localizing the faculty of speech in the frontal lobes, which was at least partly based on post-mortem anatomical evidence. "The main credit for discovering the speech center goes to Gall and not to Bouillaud or Dax or even Broca."[30] Localizing the speech center would remain a hot theme for a large part of the century. The same holds for Gall's assumption of the functional symmetry of the brain.

Against localization

Pierre Flourens (1794–1867), who had studied with Gall in Paris and with the more academically established Georges Cuvier, eventually chose the latter as his patron and

[26] Spurzheim (1818).

[27] "Déjà d'autres l'ont accusé de plagiat; c'est au moins très-ingénieux de faire des livres à coups de ciseaux." (Gall 1818, p. x).

[28] "Bienveillance," "espérance," "surnaturalité."

[29] Cited from Hunt (1869, p. 205).

[30] "Das Hauptverdienst bei der Entdeckung des Sprachcentrums gebührt also Gall und nicht Bouillaud oder Dax oder gar Broca." (Bunge 1905, Vol. I, p. 248).

became one of the fiercest critics of Gall and the phrenologists. Flourens did not contribute anything on the topic of language and the brain, but what is relevant here is the position he took with respect to the localization of functions. That position would reverberate for decades in French academia and deeply affect the discussion of language functions. Flourens recognized four major partitions in the brain: cerebellum, medulla, thalamus, and hemispheres. According to his account, each brain partition performs a specific function, such as locomotion control in the cerebellum, respiration in the medulla, and vision in the thalamus. Only the hemispheres are involved with intellectual functions; they do not perform any other function. In particular, the hemispheres play no role in voluntary movement. This was already a deviation from phrenology, which also put functions other than intellectual ones in the hemispheres. But more importantly, Flourens completely rejected the idea that the hemispheres contained a number of small organs for the various intellectual faculties. He took a diametrically opposed position, anticipating Karl Lashley's *equipotentiality*: if one removes part of the hemispheres other parts will take over their function. Or in Flourens's own words: "one can dissect a rather extensive portion from the cerebral hemispheres, be it on the front, in the back, on top or on a side, without loss of intelligence. A fairly limited portion of those hemispheres thus suffices for the exercise of intelligence."[31]

Flourens became an influential and internationally recognized personality in French brain physiology. It was certainly quite respectable in mid-century Paris to align with Flourens and his equipotentiality. That is what Broca did initially, as we will see.

Phrenology without bumps

Jean-Baptiste Bouillaud (1796–1881) also studied with Gall in Paris, but unlike Flourens, he continued his career as a phrenologist. In 1831 he even established the Paris Society of Phrenology. But his was a respectable sort of phrenology without bumpy organs and skull diagnostics. In 1825, as a young man, he published a paper on the language organs in the brain,[32] testing the hypothesis that the anterior lobes are the organs of language. His approach was impeccable. He set out to show that (i) patients with a loss of articulate speech should, in autopsy, show damage to the anterior lobes, (ii) if an autopsy shows lesions in the anterior lobes, the patient should have had loss of articulate speech, and (iii) if an autopsy shows damage to another region of the brain, there should be no loss of articulate speech. The paper reported on six patients with loss of speech, whose autopsies Bouillaud had performed himself, and another 41 patients studied by his colleagues Lallemand (21 cases) and Rostan (20 cases), with whom he communicated by letter. Bouillaud concluded from his review of all these cases that there were no violations of

[31] "Mes expériences l'ont montré: on peut retrancher, soit par devant, soit par derrière, soit par en haut, soit par coté, une portion assez étendue de ces hémisphères cérébreux, sans que l'intelligence soit perdue. Une portion assez restreinte de ces hémisphères suffit donc à l'exercice de l'intelligence." (Flourens 1842, p.18).

[32] Based on his dissertation, the same year, on the control of muscular movements.

his three predictions. His inevitable conclusion was that the anterior lobes form the seat of the faculty of articulate language. The faculty involves two components: the memory for words and the control of articulatory movements. It is likely, according to his account, that the former resides in the gray matter and the latter in the white matter. The faculty is distinct and independent. In particular, it controls the *speech* movements of the articulatory muscles only. Other movements of tongue and throat are unaffected in loss of speech. This was not good news for Flourens, who had denied that motor control is located in the hemispheres. The only thing the two agreed on was the perfect symmetry of brain function.

In 1848 Bouillaud read another paper to the Academy in which he largely extended his database and arrived at the same conclusions. This time, he challenged his opponents to produce a single case of loss of speech without damage to the frontal lobes. He offered a prize of 500 francs for any such case submitted. The award was never claimed.

Bouillaud, like Flourens, was elected to the *Académie des Sciences*; he was no less respected. Although the bulk of his research was not on language disorders, his 1825 study remained on the agenda for 40 years.

The left hemisphere and Broca's area

Bichat's law of symmetry was practically an axiom during the first half of the nineteenth century. If there exists a brain region for articulated speech, it should be a bilateral region. It is likely that Marc Dax (1770–1837), a physician in Montpellier, wrote a short paper on this topic, arguing that the seat of spoken language was in the left hemisphere. Dax had intended to present the paper at the 1836 southern congress of the Medical Society in Montpellier,[33] but there is no independent evidence that it was actually delivered. The paper surfaced only as a publication in 1865 under curious circumstances, which we will discuss shortly. From 1800 Dax had kept statistics from his own practice as a physician of hemiplegia cases[34]—"more than 40" he said—in relation to loss of word memory. Loss of speech only occurred in right hemiplegia patients, never in cases of left hemiplegia. Clearly, this would not have been predicted by either Gall or Bouillaud, who believed in the functional symmetry of the brain. For Dax the really convincing case was as follows. He was urgently called in to one of his patients, who had suffered a small blackout on previous occasions. This time the attack was much more serious. She had a complete loss of speech. "I didn't have to think to know the nature, the site or the treatment of this illness. I immediately applied a large number of leeches to the left temple, and, in a few

[33] During this period, Jacques Lordat (1773–1870) was professor of physiology at Montpellier University. He taught a course on "alalie and paralalie" (Lordat 1843), in which he described cases of amnesia and both semantic and phonological paraphasia. The most famous case in this publication was Lordat himself, who, after an infectious disease had lost the ability to produce or understand words, though in full possession of inner speech. Loss of speech was an active issue in Montpellier medical circles.

[34] Hemiplegia is one-sided paralysis, resulting from brain disease.

minutes, as the blood ran, speech reestablished itself gradually."[35] She lived several more years "in good health."

Marc Dax's son Gustave (1815–1874), a physician in Sommières, had continued his father's collection of data, but never published them. It is not improbable that Broca's 1861 reports on two speechless patients, both with a left-sided lesion, prompted him in 1863 to submit his father's 1836 paper plus his own extended statistics to both the Medical Academy and the Academy of Sciences (on March 23 and 24, respectively).[36] His own statistics were as follows. He had collected 370 cases of hemiplegia, 16 from his own observations and the rest from other sources, in particular 273 cases from Lallemand (who had also contributed to Bouillaud's statistics). He had had to reject 225 cases that were insufficiently documented. The remaining clear cases consisted of 87 right hemiplegia patients with loss of speech and 53 left hemiplegia patients without loss of speech, both of which confirmed the theory. There were only 6 violating cases. Gustave's best guess was that the seat of articulated speech is in the "external anterior part of the medial left lobe," which Ogle (1867) translated as "that portion of the left hemisphere which borders on the fissure of Sylvius."[37] Dax victoriously concluded his paper thus: "Hence, the cerebral organ of speech has been found."[38]

The Academy of Science committee, with Flourens as a member, never responded to his submission. The committee of the Medical Academy, consisting of the members Bouillaud, Béclard, and chairman Lélut, did respond, but only almost two years later, or rather, Lélut did so on his own account.[39] There are certain issues, Lélut wrote, on which "my opinion can no longer be changed, nor modified"; "the relation one wanted to establish between some fact or faculty of mind, and some part of the central nervous system," is "no less and no more than phrenology," which is a "pseudo-science."[40] And then he gave examples from his own practice: an epileptic patient whose left hemisphere was "mush," but whose speech was intact; another one with damage to the cerebellum *with* loss of speech; and a third one with only meningitis at the very surface of the brain but *with* loss of speech. It is not a modern phenomenon that the process of peer review can occasionally fail. Only after this response did Gustave Dax decide to offer his joint paper to the *Gazette*, where it appeared on April 28, 1865, a crucial date as we will see.

[35] "... je n'eus pas besoin de réfléchir pour connaitre la nature, le siége, ni le traitement de cette maladie. Je fis promptement appliquer sur la tempe gauche un grand nombre de sangsues, et, dans quelque minutes, à mesure que le sang coulait, la parole s'établissait graduellement." (M. Dax 1865, p. 260).

[36] But he did not refer to Broca's 1861 papers in what he submitted in 1863 and published in 1865.

[37] Ogle (1867), p. 84.

[38] "Donc, l'organe cérébral de la parole est trouvé" (M. G. Dax 1865, p. 262).

[39] Bouillaud added a note that he intended to openly discuss the issue with Lélut in a session of the Academy.

[40] "... mon opinion ne saurait plus ni changer, ni se modifier" ... "la relation qu'on cherchait à établir entre tel fait ou telle faculté de l'esprit, et telle partie du système nerveux central" ... "Ceci n'est ni plus, ni moins que de la phrénologie ..." (Lélut 1865, p. 173).

Fig. 3.3 Paul Broca.

The old and never-dormant issue of the localization of mental faculties in distinct organs of the brain got a new surge when Gratiolet and Auburtin discussed it in early 1861 in a meeting of the Anthropological Society in Paris. That society had been established two years before by the brilliant young surgeon Paul Broca (1824–1880, Fig. 3.3), who was going to play a major role in follow-up discussions. He had been trained as an anatomist and surgeon by some of France's leading clinicians, such as Blandin and Lebert. His broad clinical skills were recognized early and his star quickly rose at the University of Paris Medical School, from his first appointment in 1848 to his early death in 1880 when he was a celebrated professor of surgery and as a widely respected member of French society.

At issue in the 1861 meeting was the cerebral localization of functions in general (Gratiolet's contribution) and the special test case of the localization of the faculty of speech. The latter was presented by Ernest Auburtin, who defended the frontal-lobe theory of his father-in-law, Jean-Baptiste Bouillaud. In a betting mood, not unlike his father-in-law in 1848, he declared he would renounce Bouillaud's theory of the localization of faculties if a case turned in which there was a loss of speech but no damage to the frontal lobes.

Only days after Auburtin's communication, Broca's attention was called to a patient who had not been able to speak for 21 years, but who otherwise seemed to be in the possession of his mental powers. Broca realized that this could be a test case for Auburtin's challenge. Broca invited him to come and diagnose the patient for himself. Auburtin did and agreed to accept the outcome of a future autopsy as conclusive; the patient's brain was predicted to show frontal lobe damage.[41] The patient, Mr. Leborgne, lovingly called "Tan" in the hospital, because "tan" or "tantan" was the only thing he could say, was in a

[41] These circumstances are related by Broca (1861b) himself.

serious state because he had an inflammation of the right leg, which was the reason for involving Broca. Tan died shortly afterwards, on April 17, and Broca performed an autopsy. Shortly after that Broca presented Tan's brain to the Anthropological Society. There is a short, technical report[42] describing Tan's brain, which states that the left frontal lobe, over most of its extension, was "softened" (*ramolli*). The concluding sentence is: "the lesion of the frontal lobe has been the cause of loss of speech."[43]

But Broca was too excellent a scientist to leave it at that. In a subsequent 27-page report to the Anthropological Society, also published during 1861, he gave a detailed account of the scientific reasons for analyzing Tan's brain, which was then followed by an equally detailed account of the autopsy. Broca was exceedingly clear about the scientific aim behind this autopsy. It was to test Bouillaud's claim that the faculty of articulated speech resides in the frontal lobes. Bouillaud had "saved from shipwreck"[44] the phrenological idea that the faculty of language has its seat somewhere in the frontal lobes, but he had been careful to limit the claim to a faculty of articulated speech. The general faculty of language was not at issue. The general language faculty manifests itself in diverse ways, such as writing, mime, and dactylography. It has, in fact three components: the faculty of relating signs to ideas, plus the faculties of emission and reception. It is rare that both the latter faculties are lost, but the specific loss of articulated speech is often observed in clinical practice. Such patients keep their general faculty of language and their receptive abilities. It is quite likely that the loss of articulated speech is not a kind of ataxia, a problem of muscle innervation. Instead, it is more likely it is some kind of "intellectual trouble."[45] It was much too early, according to Broca, to pinpoint the precise localization of functional organs in the brain: "That is an issue which seems to me unsolvable in the present state of science."[46] But we may be able to approach the underlying principle of localization itself, which is the starting point of the phrenological system. Here, Broca accepted Auburtin's approach, namely that the "localization of a single faculty would suffice to establish the veracity of that principle."[47] Loss of articulated speech can function as such a test case, because this pathology provides evidence that we are dealing with an independent faculty. So, the interest is in *whether* that test faculty is localized, not so much *where* it is localized.

With respect to the *where*, Broca still discussed three options: (i) There is no specific localization and any cerebral lobe could be involved, (ii) there is a specific gyrus involved, and (iii) it involves a specific lobe (for instance the anterior lobe), but the relevant gyrus

[42] Broca (1861a).

[43] "... la lésion du lobe frontal a été la cause de la perte de la parole." (Broca 1861a, p. 238).

[44] "si M. Bouillaud ne l'eut sauvée du naufrage" (Broca, 1861b, p. 330).

[45] "l'aphémie serait un trouble intellectuel" (Broca, 1861b, p. 335).

[46] "... c'est une question qui me paraît tout à fait insoluble dans l'état actuel de la science." (Broca 1861b, p. 338).

[47] "... que la localisation d'une *seule* faculté suffirait pour établir la vérité de ce principe" (ibid. p. 336). Auburtin's slim 1863 monograph, which discusses the Tan case, expresses the same opinion.

therein can be variable. This last option would mean that cerebral faculties are localized *by region*, not by *gyrus*. Broca clearly sympathizes with this region option: the scientific facts "allow us to recognize, as I have said elsewhere, that the grand regions of the mind correspond to the grand regions of the brain."[48] Flourens would have liked this statement, the grand regions being equipotential. But the frontal lobe as the site of articulated speech, a motor function, was anathema to him. Earlier phrenological diagnostics indeed show great variability in lesion localization, Broca remarked, even when limited to the frontal lobe. But here Broca pleaded for using a different geometry, not the usual one in centimeters from some anchor point, such as the anterior extremity of the brain, but one defined in terms of *gyri*, such as the first, second, etc gyri of the frontal lobe. In order to test option (iii), Bouillaud's theory, great precision in the autopsy is required.

It should be noticed, however, that Bouillaud's 1825 theory was not *exactly* option (iii). For Bouillaud, *any* lesion in the frontal lobe would cause loss of speech. Broca generously follows Auburtin on this point, the latter having claimed only that the faculty of speech should be *somewhere* in the frontal lobe.

After these theoretical considerations, Broca presented a detailed account of the autopsy he had conducted. Tan's brain showed extensive damage to most of the left frontal lobe and some also in adjacent areas, in particular the left insula. Broca's analysis focused on the lesion that was most likely the primary one, the initial cause of Tan's loss of speech, 21 years previously. The conclusion was that the original lesion was "most probably in the third frontal convolution," although the second convolution was not excluded. Finally returning to the theoretical issues, Broca concluded that the loss of speech had indeed been caused by a lesion in one of the frontal lobes, confirming Bouillaud's theory. But it was not then possible to decide whether faculties were sited in grand regions or in specific gyri of the cerebrum. One thing was definitely ruled out, concluded Broca: a bumpy organ of language in the very front of the lobe, as the old phrenologists would have it.

In short, Broca was only testing the principle of localization. He was *not* testing the precise localization of the faculty of speech, and definitely not whether there was a hemispheric asymmetry. The same holds for Broca's third 1861 communication to the Anthropological Society. This time Broca presented another case of speech loss. M. Lelong, an 84-year-old patient in full possession of his intellectual faculties, had suddenly lost his ability to speak. He had only a few words left, but did not show any sign of paralysis. He was brought into the clinic on October 27 and died two weeks later, on November 8. Broca's autopsy showed a by-and-large normal, healthy brain, with one clearly circumscribed lesion with an extension of about 15 mm. It was located largely in the third convolution of the left frontal lobe, with some extension into the second convolution, right at the border of the Sylvian fissure. The lesion was not one of softening, as in Tan's brain; it was the obvious result of a stroke. From these two cases, Broca stated, I cannot "prevent myself from saying until more amply informed, that the integrity of the third frontal gyrus (and maybe the second) seems indispensable for the exercise of the faculty of articulated

[48] "et les faits acquis jusqu'ici à la science permettent d'admettre, comme je l'ai dit ailleurs, que les grandes régions de l'esprit correspondent aux grandes régions du cerveau" (Broca (1861b), p. 338).

language."[49] And indeed, Broca was quite surprised to find the two lesions being "exactly in the same place," "and precisely on the same side (left side)."[50] But then, even more surprisingly for us, Broca, when referring to the variability of frontal lobe lesions in earlier documented cases of loss of speech, concluded: "I am thus inclined to attribute to a pure coincidence, the absolute identity of the lesion sites in my two patients."[51]

Is the discovery one the scientist must be aware of? If so, this was not yet a discovery. We will never know whether, at this point, Broca was unable to detach himself from equipotentialist doctrine and from Bichat's law. But one can imagine Gustave Dax's excitement when he read these reports (if he did).

In 1863, Broca returned to the localization issue in a short presentation to the Anthropological Society on April 2. Referring to the two cases of speech loss in his 1861 papers, he now added six more. Three were Charcot's patients, all with a lesion in exactly the same site. A similar case was presented by a M. Gubler and there were two more cases from Charcot and Trousseau, carefully examined by Broca himself. The latter two both had major lesions, extending into the third convolution. Broca's conclusions from these cases are of some importance for the priority issue. Let me cite them in full:

> Hence, here are eight facts where the lesion is sited in the third frontal convolution. This number appears to me sufficient to invite strong conjectures. And, quite remarkably, in all these patients the lesion was on the left side. I don't dare to draw a conclusion from that and I wait for new facts.[52]

Apparently, he had not seen the paper that Gustave Dax had submitted just ten days earlier to the two academies, abundantly providing such relevant facts. So, if the "strong conjectures" do not concern the left hemisphere locus of these lesions, what is it that Broca intended to conclude from these facts? He does not leave us in doubt:

> Everything I just said only relates to a single localization, the one for articulated language, a well-defined issue and it is already much to be almost certain about at least one well-determined site; but will it be possible to provide the same demonstration for other less circumscribed faculties? That may be very difficult.[53]

[49] "... je ne puis toutefois m'empecher de dire, jusq'à plus ample informé, que l'intégrité de la troisième circonvolution frontale (et peut-être de la deuzième) paraît indispensable à l'exercice de la faculté de langage articulé." (Broca 1861c, p. 406).

[50] "... elles l'étaient exactement dans le même point, immédiatement en arrière de leurs tiers moyen, vis-à-vis du lobe de l'insula, et précisement du même coté (coté gauche)." (ibid. p. 407).

[51] "Je suis donc disposé à attribuer à une pure coincidence, l'identité absolue du siége des lésions chez mes deux malades." (ibid. p. 407).

[52] "Ainsi, voilà huit faits où la lésion a siégé dans le tiers postérieur de la troisième circonvolution frontale. Ce chiffre me paraît suffissant pour donner de fortes présomptions. Et, chose bien remarkable, chez tous ces malades la lésion existait du coté gauche. Je n'ose tirer de là une conclusion et j'attends de nouveaux faits." (Broca 1863, p. 202).

[53] "Tout ce que je viens de dire ne se rapporte qu'à une seule localisation, celle du langage articulé, chose bien définie et c'est déjà beaucoup d'être à peu près certain qu'une faculté au moins a un siége rigoureusement déterminé; mais sera-t-il possible de fournir de même démonstration pour d'autres facultés moins circonscrites? Peut-être sera-ce bien difficile." (ibid. p. 202).

The theoretical case he was making was the same one as his extensive 1861 paper: it was to use the loss-of-speech phenomenon as a test case for the localizational issue. Do mental faculties have a specific localization in the brain? His "strong presumption" was that we now possess at least one demonstrated case that it does.

Eventually, Broca's 1865 paper, published on June 15 in the *Bulletin* of the Anthropological Society, addressed the left hemisphere issue. Under a reference to his 1863 paper he states: "I persist in thinking, till more amply informed, that real aphemia, that is loss of speech without paralysis of the articulatory organs and without destruction of intelligence, is connected with lesions of the third frontal gyrus."[54] This statement was not totally honest because, as we saw, Broca did not draw that conclusion in the 1863 paper. The present paper, he added, considered the "singular predilection of aphemic lesions for the left hemisphere of the brain." Then he made reference to the Dax paper, which had appeared on April 28. "I don't like priority discussions," he said, and he was certainly willing to believe that the elder Dax *wrote* his paper in 1836, but of course it was not published at that time. In fact, he, Broca, had done his best to check with Gordon, the librarian of the Montpellier medical faculty, to see whether any trace of that paper could be found. The *Montpellier Revue* of 1836 did not mention the topic of language at the congress. Further checks by Gordon with five different doctors produced no trace of the paper nor memory of it. The paper only surfaced, according to Broca, "two years after my first publications on aphemia."[55] This can only mean in 1863, when Gustave Dax submitted it to the academies.

In short, as far as dates go, Gustave Dax must be given priority for the claim that the faculty of articulated language has its seat in the left hemisphere. However, there are two things that we will probably never know. The first is whether Broca changed his position only after reading the report Dax submitted to the Medical Academy. It is most unlikely that it was not shown to him by his friends there (such as Bouillaud, who was on the committee); it was a hot issue. The second is whether Gustave Dax felt urged to submit his paper to the academies upon reading Broca's 1861 papers and, if so, to what extent he got his idea of the "anterior external part of the left middle left lobe"[56] from Broca's two cases. In contrast to Broca, he did not refer to his competitor in his 1865 paper, which he certainly could (and should) have done. If the Dax family are to be granted priority in discovering the left hemisphere's role in the production of articulated language, Broca should be credited for his superb brain anatomy in singling out the third frontal gyrus as the likely site of this faculty. William Ogle had introduced the eponym "Broca's region" for this site as early as 1867.[57]

[54] Broca (1865, p. 378).

[55] "il m'était impossible de deviner l'existence d'un manuscrit qui n'a été exhumé que deux ans après mes premières publications sur l'aphémie."(Broca 1865, p. 379).

[56] "La partie *antérieure externe* du *lobe moyen gauche* est blessé, le malade est muet." (Dax 1865, p. 261).

[57] W. Ogle (1867, p. 119). Wernicke (1874, p. 14), called it "die Broca'sche Stelle"; Ferrier (1876, p. 126), named it "Broca's area."

The bulk of Broca's 1865 paper concerned the potential causes of the left hemisphere predilection. The basic functional symmetry of the brain is not at issue, Broca argued. The case is similar to right-handedness—not so much caused by imitation or culture, but by early brain development. Left hemisphere development is just somewhat in advance of right hemisphere development in the young child.[58] The early development of the intellectual faculty of articulated language capitalizes on the most advanced hemisphere. If the relevant area of the left hemisphere happens not to be functional (due to epilepsy or developmental atrophia) articulated language will develop normally in the right hemisphere. Broca carefully avoided the claim that left-sided language should always go with right-handedness. He was also quite explicit in restricting his conclusions to the faculty of articulated language. The general faculty of language, which involves the connection of signs and ideas, is as much a matter of the left as the right hemisphere: "the faculty of conceiving such relations belongs equally to both hemispheres."[59] This explains why language comprehension is often unaffected in patients with loss of speech.

Trousseau seeds confusion

The discovery of Broca's area and its function in articulated speech had a major and immediate impact, but it did not create consensus. The *Gazette* of April 28, 1865, which featured the Dax paper, also reported on the April 25 session of the Medical Academy. There, academy member Armand Trousseau (1801–1867) used the Dax paper to comment on the localization of what he himself had coined "aphasia," "correlative trouble of language and thought."[60] In Bouillaud's presence he argued that the latter's 1825 paper had set the agenda for localization studies of language. The frontal lobes are certainly often involved in aphasia, although Bouillaud was wrong in claiming that speech motor innervation itself originates in the frontal lobes (here Bouillaud protested). He then attacked the idea of a circumscribed left hemisphere site, seeding confusion by misrepresenting Broca's position on localization: "Mr. Broca, first incredulous, has subsequently become one of the most fervent partisans of that localization doctrine . . . He went even much further than the two Daxes, in placing the site of the faculty of language in the posterior part of the third left frontal gyrus."[61] However, if Broca had been careful about anything in his papers, it was to *not* identify the faculty of articulated speech with the general faculty of language. The latter is a more general intellectual power, involving the

[58] This idea, Broca (1865, p. 383) acknowledges, was proposed by Gratiolet.

[59] ". . . la faculté de concevoir ces rapports appartient à la fois aux deux hémisphères" (ibid. p. 386).

[60] ". . . trouble corrélatif du language et de la pensée" (Trousseau 1865, p. 266). Glozman's (1996, p. 2), remark, under reference to Tarnovsky (1867), "It should be pointed out that V. Tarnovsky used the term 'aphasia,' accepted in Europe only in 1877 after the contribution of A. Trousseau (1801–1867)." is incorrect. Trousseau had already used the term in his 1865 paper (p. 266).

[61] "Vous savez que M. Broca, d'abord incrédule, est devenu ensuite un des plus fervents sectateurs de cette doctrine de la localisation. . . . Il alla même beaucoup plus loin que Dax, et il plaça le siége de la faculté du langage dans la partie postérieure de la troisième circonvolution frontale gauche." (ibid. p. 266).

ability to connect ideas to signs and the ability to comprehend language, and involving both hemispheres. Trousseau then cited numerous pathological cases of left frontal lobe damage without loss of speech, of right frontal lobe damage with loss of speech, of language loss with left hemiplegia, aphasia with intact frontal lobes, aphasia with damaged occipital lobes, and so on. In the presence of all these facts, he had to conclude that "until present, the doctrine of our eminent colleague [meaning Bouillaud], and those of Mr. Dax and of Mr. Broca, with respect to the localization of the faculty of language in the anterior portions of the brain; those doctrines, I say, are not immune to all reproach."[62] In short, back to square one.

Trousseau was one of France's best known clinicians. His diatribe was to reverberate for more than a decade. In 1884 Wernicke still wrote that although Trousseau had correctly argued that lesions at quite different sites could cause language disorders, he had also "by the power of his eloquence,"[63] tacitly substituted Broca's quite limited syndrome by another much broader notion, so that all talk about whether Broca's notion of aphemia or Trousseau's notion of aphasia was more correct, was in fact comparing apples and oranges.

Trousseau's aphasia was a complex set of syndromes. What was urgently needed was to distinguish, within the broad language faculty, all specific faculties, including Broca's faculty of articulated speech, which together constitute its functional architecture. It was Carl Wernicke who conceived of this program in 1874.

Carl Wernicke and the diagram makers

Baginsky

It is the historical fate of some creative minds to end up as precursors of others. Such was the case of Adolf Baginsky (1843–1918), who was a pediatrician broadly involved in clinical medicine. As a one-time sideline to his main research in pediatrics, he published a theoretical paper on aphasia, occasioned by two of his kidney patients who, at various stages of their serious illness, were more or less handicapped in their speech. However, the core of the paper hardly touched on these patients. Instead the paper contained an analysis of aphasic syndromes. Figure 3.4 presents the diagram Baginsky drew for this purpose. It displays the functional architecture of speech. The 8 and 6 o'clock branches of the diagram represent the speech input and output channels, respectively. Speech input runs from *a*, the endpoint of the acoustic nerve, via *b*, the center for acoustic perception, and *c*, the center for sound memory, to D, which is the main conceptual center. From there, speech output runs through *e*, the center for coordinated movements, via motor nerves *f* to the speech musculature. The 10 o'clock branch represents the optical input tract to the conceptual center. Optical images leave their traces in *c'*. If a sound image in

[62] "... je crois qu'on peut conclure que, jusqu'au présent, la doctrine de notre éminent collègue, et celles de M. Dax et de M. Broca, relatives à la localisation de la faculté de langage dans les parties antérieures du cerveau; ces doctrines, dis-je, ne sont pas à l'abris de tout reproche." (p. 268).

[63] "... durch die Macht seiner Beredsamkeit" (Wernicke 1884, p.1; 1892/2006, p. 71)

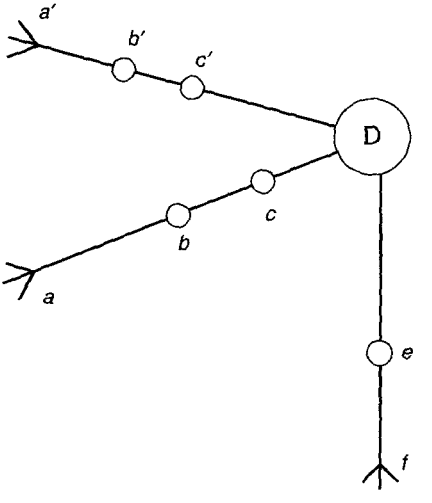


Fig. 3.4 Baginsky's (1871) diagram.

c (for instance "Daddy") and optical image in *c′* (for instance Daddy's face) flow together into D, the concept of the person called "Daddy" may arise. Baginsky then used this elementary bit of psychology to distinguish aphasic syndromes through lesions of either centers or their connections. Two sets of syndromes naturally arise as a result: either the input or the output channel is affected. If the peripheral part of the input channel is affected, you have deafness. If the peripheral part of the output channel is affected, you have paralysis of the speech muscles. If D is affected, you have a conceptual disorder. None of these are strictly aphasic syndromes. Aphasia arises from lesions in the region of *b*, *c*, and *e*, or in a lesion of their connections to D. The main syndromes are loss of sound memory (*c*) and loss of speech motor coordination (*e*). Without sound memory you cannot speak fluently, because some or all speech sounds are lost (notice, however, that this presupposes some unmentioned connection between *c* and *e*). Without speech movement coordination (lesion of *e*), you cannot express the well-formed verbal concepts in D. Baginsky remarked that patients of the former type of aphasia are unconscious of the dropping out of sound images. But patients of the latter, "centrifugal" type of aphasia are fully conscious of their speech problem.

Wernicke 1874

When Carl Wernicke (1848–1905, Fig. 3.5) wrote his 68-page classic monograph *The aphasic symptom complex* (1874), he made passing reference on p. 65 to Baginsky's classification of centrifugal and centripetal aphasias. But it is one thing, Wernicke added, to "think up theoretically different centers,"[64] but quite another thing to establish their anatomical foundations. He intended to achieve the latter. In our terminology, Wernicke wanted to anchor the functional architecture for speech in the neural architecture.

[64] "... theoretisch verschiedene Centra zu fingieren" (1892/2006, p. 66).

Fig. 3.5 Carl Wernicke.

Wernicke had obtained his medical degree in Breslau. While there he wrote his monograph, taking about six months, basing his work on ten cases referred to the Allerheiligen Hospital in Breslau. Shortly before this, Wernicke had paid a study visit to the famous neuropathologist Theodor Meynert in Vienna. Although Wernicke admired Meynert, as is clear from his monograph and many later publications, Meynert's specific influence on Wernicke's theory of aphasia was small, as Eling (2006) convincingly argues. But Wernicke was certainly influenced by Meynert's theory of projection, the topography of sensory and motor pathways into and out of the brain. The cortex is, initially, a kind of *tabula rasa*, but sensory input leaves its traces around entrance points of the sensory nerves. These regions become the sites of visual, acoustic, tactile, etc. memories. Similarly, regions around the outgoing motor nerves come to store the images of motor actions performed. Meynert's cytoarchitectonics supported this cortical localization theory. Right after publishing his monograph, Wernicke went to the Charité Clinic in Berlin. There he wrote his important *Textbook of brain disorders* ("Lehrbuch der Gehirnkrankheiten"). In 1885 he returned to Breslau, as a professor of neurology and psychiatry. In 1904 he moved to Halle University, but he died in 1905 as a result of a bicycle accident.

Wernicke's core diagram is reproduced in Figure 3.6. The critical part is the fiber trajectory $a–a_1–b–b_1$. Following Baginsky's logic, this trajectory can be lesioned in five places, with each lesion causing a particular speech problem. Let us follow Wernicke's classification:

I. Disruption of the acoustic nerve $a–a_1$. This causes deafness. A child born with such a defect becomes deaf-mute, because no speech–sound images will be collected in the auditory center a_1. Without sound images as reference, speech movement images cannot be formed in movement center b (Broca's area).[65] Deaf-muteness is not an

[65] Here Wernicke ridicules Steinthal, who claimed that speech naturally emerges from concepts. Not so, the spoken word evolves as the reproduction of a sound image (Wernicke 1874, p. 18).

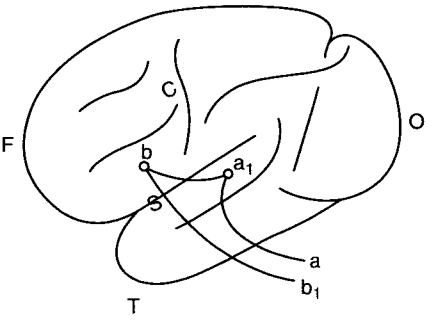


Fig. 3.6 Wernicke's core diagram. F, O and T are the frontal, temporal and occipital extremities of the left hemisphere. C is the central or Rolandic fissure, S the Sylvian fissure. The line segment a–a_1 represents the incoming acoustic nerve from the oblongata (a) to its projection site in the temporal lobe (a_1). Broca's area is b. The segment b–b_1 represents the centrifugal speech motor nerves back to the oblongata (b_1).[66] The segment a_1–b represents the fibers connecting a_1 and b across the Sylvian fissure through the insula. From Wernicke (1892).

aphasic syndrome, but one should look out for cases of developmental aphasia where only the tract a–a_1 to the left hemisphere is disrupted. How well will sound images collected by the auditory center in the right hemisphere feed to Broca's area in the left hemisphere?

II. Center a_1, the site of the speech-specific sound images, is affected. It is Wernicke's everlasting scientific contribution to theoretically predict a "sensory speech center"[67] near the projection site of the acoustic nerve in the first temporal gyrus, to find patients with a lesion in that area, and to describe and theoretically account for the aphasic symptoms of such patients. When the sensory speech center a_1 is fully lesioned, the patient cannot understand spoken words although he is not deaf. The patient cannot repeat spoken words. The speech such a patient hears is just noise. Can the patient speak? To some extent, yes. If site b, Broca's area, is intact, the speech movement images can still be activated from the object memory images in the brain. The full spoken-word repository is intact. In an additional diagram, Wernicke drew a tactical image site in the parietal lobe and an optical image site in the occipital lobe. The concept of an object that you have seen and touched is nothing other than the active fibre connection between the two corresponding images in these two sites. In order to express the concept by means of a spoken word, you activate speech movement center b from either image site. Still, dependent on the degree of the lesion, such speech will be disordered. Not only did we acquire the speech movement images (in b) by imitating the speech sound images (in a_1), but the tract a_1–b stays permanently involved in the monitoring of speech output. Speakers are always checking

[66] The subscript of b_1 is an omission in the diagram on page 17; it only appears in the text. The diagram is copied from Wernicke's own republication of the monograph in his *Gesammelte Aufsätze* (1892). Although essentially the same as the original 1874 one, it is much more clearly related to the text.

[67] ". . . sensorisches Sprachcentrum" (ibid. p. 22).

whether their spoken words match the relevant stored sound images. That monitoring facility breaks down in the sensory aphasic patient. As a consequence, the patient will produce *paraphasias*,[68] replacing target words by other words that are meaning- or sound-related, and there is no way for the patient to become conscious that he or she is doing this. There are, according to Wernicke, two other characteristic symptoms of the pure sensory aphasic patient. One is that the patient is not hemiplegic, as Broca patients typically are. The other is that such patients cannot write; they are *agraphic*. This is because our writing movements are mediated by word–sound images; there is no direct connection between concepts and writing movements. In short, this passage in the monograph is the native site of the notions of "Wernicke's area" and "Wernicke's aphasia."[69]

III. Disruption of connection a_1–b. Anatomically, the lesion is in the area of the insula, the small gyrus deep in the Sylvian fissure, roughly between Broca's and Wernicke's areas. The sound images are intact and the patient can fully understand speech. The movement images are intact and the patient can speak. But as in the previous case, there is no direct self-monitoring, thus resulting in paraphasia. Here, however, the patient can become conscious of the paraphasia. The patient can hear and understand what he has just said. If the person happened not to express the intended concept, there is still the possibility of a highly conscious and strenuous means of self-monitoring and self-correction.[70] The patient is also alexic. The letters seen will still activate their sound-images in a_1, but these, alone or in combination, can no longer activate the corresponding movement images in b. If the reader is experienced, the whole optical word image may directly activate a concept, without mediation of the sound image. The concept, in turn, may activate the right word image in b, leading to pronunciation. It is also likely that the patient cannot write, because if the connection from a_1 to movement center b is damaged, the same is likely to hold for the tract from a_1 to the motor area involved in writing. Wernicke called this newly defined syndrome, which is caused by a disrupted connection between the two speech centers, "conduction aphasia."[71] Unlike sensory aphasics, most conduction aphasics

[68] A term introduced by Kussmaul (1877).

[69] The syndrome of sensory aphasia had not entirely escaped attention before Wernicke (1874). We will cite William Ogle's (1867) and Broadbent's (1872) descriptions of the syndrome. Glozman (1996) claims that Alexey Kojevnikov (1874) published a case of sensory aphasia, with injury in the left temporal region, just weeks before Wernicke's monograph appeared. Wernicke (1903) cites Schmidt and Bastian as predecessors.

[70] This roundabout self-monitoring facility corresponds to Levelt's (1983) "outer loop," whereas the direct connection a_1–b corresponds to the "inner loop." Again Wernicke uses the case of "strenuous speech" to swipe at Steinthal: "If Steinthal in his 'Abriss der Sprachwissenschaft' has already found thinking difficult it will definitely be even harder to think and speak simultaneously." (Wernicke 1874, p. 27). ("Wenn nun Steinthal in seinem 'Abriss der Sprachwissenschaft' schon das Denken allein schwer gefunden hat, so ist es jedenfalls noch viel schwerer, zu denken und zu sprechen zugleich.").

[71] Leitungsaphasie (ibid. p. 47).

show right-sided hemiplegia, because the lesion is adjacent to the motor centers in the frontal lobe. Wernicke did not mention the symptom that was to later become a crucial test for conduction aphasia: the patient should not be able to immediately repeat words spoken to him; the sound image cannot activate the corresponding movement image.

IV. Disruption of speech movement center b. This leads to muteness, with a few simple words spared. Wernicke calls it Broca's aphasia.[72] Agraphia is often, but not necessarily, involved. It depends on whether the lesion extends over areas that control writing movements.

V. Disruption of tract $b–b_1$. This should cause the same kind of motor aphasia, but the pure case will rarely occur because the outgoing fibers from Broca's area run very close to the fibers from other motor sites that innervate the tongue, mouth, and facial muscles. This type of aphasia will therefore differ only slightly from alalia, paralysis of the speech muscles.

The second part of Wernicke's monograph considered ten cases of aphasia, which were discussed from the perspective of this new theoretical framework. Almost none of the patients were "pure" cases. Lesions happen to be rather careless experiments of nature. Nevertheless four of the cases are particularly relevant. The first two patients showed sensory aphasia, of which "yet no single succinct case has been reported in the literature."[73] The first patient was a relatively pure case of syndrome II. Wernicke presented a long excerpt of her paraphasic speech. The patient improved rather quickly; hence there was no autopsy. The second patient was in a much worse condition, but her speech was clearly paraphasic; she did not understand what was said to her and she was not right hemiplegic, the main symptoms of syndrome II. There was no test conducted for whether she could read or write. She soon died and the autopsy showed some general atrophy, as well as the expected softening damage to the first temporal gyrus. Wernicke presented the third and fourth patients as cases of conduction aphasia. Case three, a Mr. Beckmann, had no problem understanding speech and showed no motor aphasia. But it was difficult for him to name objects, persons, and locations. He had to search for the target word and often checked to see if what he had said was correct. He was also alexic and agraphic, as predicted, but could read words, such as names, that were very familiar to him. However, he did not show right hemiplegia, which is a predicted symptom of syndrome III. The fourth patient, a Mr. Kunschkel, showed all the symptoms of syndrome III, including right leg paralysis. But as his condition improved, the paralysis disappeared and his speech became largely normal, although writing continued to be difficult.

In short, Wernicke presented the first explicit cases of conduction aphasia and the first autopsy-supported case of sensory (or Wernicke's) aphasia. The neural evidence for these

[72] Wernicke (1874), p. 39.

[73] ". . . ist von der reinen sensorischen Form, so viel mir bekannt, noch kein einziger prägnanter Fall in der Literatur verzeichnet." (ibid. p. 39). Later, Wernicke (1903) would, as mentioned, correct this, making reference to the work of Bastian and Schmidt.

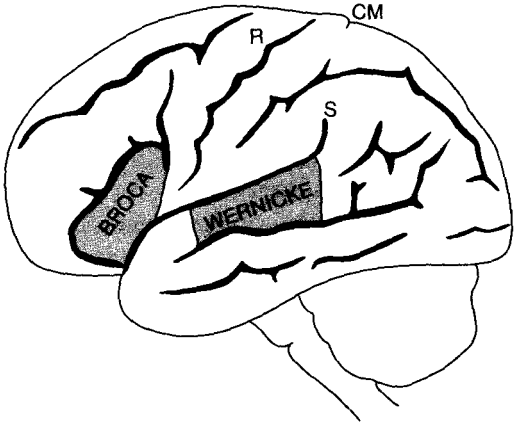


Fig. 3.7 William James's (1890) representation of Broca's and Wernicke's areas.

cases was still limited: no autopsies for conduction aphasia were conducted and there was only one autopsy of a sensory aphasia patient, a case where the brain damage was not limited to Wernicke's area.

It took some time before Wernicke's new approach was adopted, but when it was eventually picked up it became a fad. Adolf Kussmaul introduced the diagram paradigm in his influential (1877) text on speech disorders. William Preyer used Wernicke's schema as a starting point for his 1882 theory of language acquisition (see Chapter 4). He extended the system with a third component, a diction center relating words and images (concepts). Much the same move, although without any cross reference, was made by Ludwig Lichtheim. In a lucid paper (1885), to be discussed, he extended Wernicke's theoretical analysis, which in turn triggered Wernicke (1885/1886) to write an extensive response. William James adopted the new aphasia paradigm in his classical *The Principles of Psychology* of 1890, explicitly representing Broca's and Wernicke's areas in a brain diagram (Fig. 3.7).

We will next consider the productive interaction between Lichtheim and Wernicke, which led to a standard model of aphasia, usually called the Wernicke–Lichtheim model. We will then turn to Kussmaul's book, which had served only to irritate Wernicke.

The Wernicke–Lichtheim model

Ludwig Lichtheim (1845–1928) met Carl Wernicke in Breslau. Lichtheim was a medical assistant there between 1869 and 1872. Aphasia was one of Lichtheim's many clinical interests. He must have been aware of the significance of his own contribution to the theory of aphasia, because he simultaneously published his extensive paper *On aphasia* (1885) in both German and English.[74] Indeed, Lichtheim was highly conscious of the scientific power of his method. "Starting from the observation of facts, it culminates in the explanation of these facts. The correctness of our explanations must be subjected to

[74] 65 pages in the original German version, 53 pages in the slightly shortened *Brain* paper.

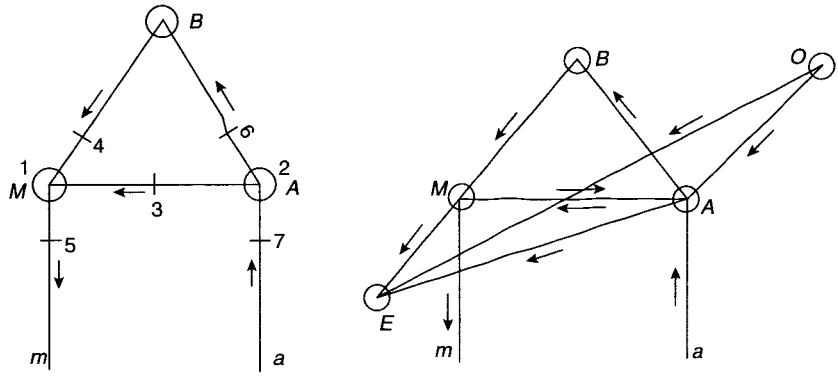


Fig. 3.8 Two diagrams from Lichtheim (1885). The left schema, "Lichtheim's house," represents the functional architecture of spoken language. *A* is the center of auditory word images, *M* the center of motor images. The reflex arc runs from auditory impressions *a* via *A* and *M* to *m*, the motor impulses for the speech organs. *B* is the center of concepts (*Begriffe*), which is connected to both *A* and *M*. The right schema adds the functional reading/writing system. *O* is the optical representation center for letters and letter combinations. *E* is the center from which the organs of writing are innervated.

the control of further observations. Precisely the same course is followed in experimental research, with the exception that, in our present subject, the experiments are not instituted at the will of the investigator, but are supplied to him by nature, and that he thus depends for them upon a happy chance."[75] Lichtheim introduced a new empirical methodology, which had been implicit in Wernicke's paper, one that is still successfully practiced as the single-case approach. The idea is that lesions in the functional model predict well-defined syndromes. The neurologist's or neuropsychologist's task is then to be on the lookout for such clear cases. Finding an example case (partially) confirms the proposed functional architecture. Notice that this verification procedure is independent of autopsy data. It can run entirely on live patients, the clinician's *Patientengut* (patient property), as it was affectionately called in those days. Of course, Lichtheim did stress the importance of post-mortem brain anatomy. Where available, he used such data to link the functional lesion to neural architecture, just as Wernicke did. Still, Lichtheim's approach is entirely functionally driven. It is the functional architecture that dictates his empirical procedure.

Figure 3.8 shows the two key diagrams in Lichtheim's paper. Making lesions in the centers or their connections in "Lichtheim's house" (left schema), naturally leads to seven distinct syndromes. These include Wernicke's original five, which result from disruptions in the *a–A–M–m* reflex arc, plus two additional possible disruptions in the "roof," in tracts *B–M* and *B–A*. A lesion of the conceptual center *B* itself leads to a disorder of intelligence, which is outside the domain of aphasic language disorders. For each of the seven syndromes Lichtheim systematically listed what is lost and what is preserved in the

[75] Lichtheim (1885) in *Brain*, pp. 433–434

patient's language. We will exemplify this from the two syndromes Lichtheim added to Wernicke's five, disruption of either *A–B* or *B–M*. But this requires a quick look at the right hand schema. Lichtheim supposed that volitional writing follows the roundabout path *B–M–A–E*. The target concept activates the movement image in *B* (many people even make tacit speech movements when they write). This in turn activates the word's sound image in *A*, which directs the corresponding letter writing movements in *E*. In (silent) reading the optical image in *O* activates the corresponding word sound image in *A*, which in turn activates the corresponding concept in *B*; the reflex arc is *O–A–B*. In reading aloud, the sound image in *A* activates the corresponding speech movement representation in *M* and its execution via *m*. Hence, the reflex arc for overt reading is *O–A–M–m*.

Back to Lichtheim's two additional proposed syndromes. They are both "transcortical," because the disrupted path runs from cortical center to cortical center.

1. *Transcortical sensory aphasia.* Lesions of the path *A–B* will lead to a loss of:
   - understanding of spoken language (auditory words have lost their connection with the corresponding concepts)
   - understanding of written language (the silent reading path *O–A–B* is interrupted between *A* and *B*).

   There will be preservation of:
   - volitional speech (the path from concepts *B* via movement center *M* to speech muscle innervation *m* is intact)
   - volitional writing (the writing path *B–M–A–E* is intact)
   - faculty of repeating words (that follows the intact reflex arc *a–A–M–m*)
   - faculty of reading aloud (the reading path *O–A–M–m* is intact)
   - faculty of writing to dictation (which follows intact path *a–A–E*)
   - power of copying words (involving intact path *O–E*, or alternatively *O–A–E*).
2. *Transcortical motor aphasia.* Lesions of the path *B–M* will lead to a loss of:
   - volitional speech (the concept in *B* does not connect to its word movement representation in *M*)
   - volitional writing (the writing path *B–M–A–E* is interrupted between *B* and *M*).

   There will be preservation of:
   - understanding of spoken language (path *a–A–B* intact)
   - understanding of written language (silent reading path *O–A–B* intact)
   - power of copying words (paths *O–A–E* and *O–E* intact)
   - faculty of repeating spoken words (path *a–A–M–m* intact)
   - writing to dictation (via intact path *a–A–E*)
   - reading aloud (path *O–A–M–m* intact).

Lichtheim discussed relatively pure cases of each of his seven syndromes; they form the existence proof, as it were, of his proposal. But, of course, most patients show damage to more than a single center or connection. Lichtheim argued that the schema is nevertheless useful for the diagnosis of cases. Just as for single lesions, the diagram predicts the loss/preservation syndrome for double or more complex lesions, and Lichtheim provided a set of examples. Finally, Lichtheim attempted to settle the nomenclature for the seven pure syndromes. He proposed, in particular, to use the term "aphasia" only for the motor disorders and reserve the term "speech deafness" for the sensory cases. Hence "transcortical sensory aphasia" is better called "transcortical speech deafness." On this terminological point Wernicke disagreed with Lichtheim, with whose "house" schema he was in general agreement.

The most important additions/qualifications Wernicke made to Lichtheim's paper were in his 1885/86 response. They concerned the representation of reading/writing, a discussion of the neural architecture underlying the functional schema and the nomenclature. The first step in handling reading and writing was for Wernicke to draw a "Lichtheim house" for written language, with a motor center for writing movements and a visual center for the representation of letters and their combinations. This led, logically, to seven types of written language disorder.[76] The crucial issue was, of course, how the two "houses" were mutually related. Lichtheim had made the specific proposal shown in the right-hand schema of Figure 3.8. Wernicke's was slightly different. Figure 3.9 shows the paths involved in reading aloud (left) and in spontaneous writing (right). For the sake of argument, I used Lichtheim's format for the representation of these solutions, but the schemata are topologically identical to the one proposed by Wernicke.

Like Lichtheim, Wernicke had originally believed that an optical word image (in *O*) can directly activate the corresponding word sound image in *A* via a path *O–A*. But a paper by Grashey, which Wernicke called "the most important advance we have scored in explaining the aphasia issue during the last 10 years,"[77] had convinced him that a word sound image had to be built up from the visual image, letter sound by letter sound. In reading aloud, his assembling of the word sound would, letter sound by letter sound, activate the corresponding speech movements (in *M*). Wernicke now supposed that this path was also involved in silent reading. Reading or writing always involves a letter-by-letter to and fro between *A* and *M*. Conduction aphasics (with disrupted connection between *A* and *B*), in particular, should be handicapped in both reading and writing. We will return to Grashey's paper in Chapter 5. The paper provided the first theory on the step-by-step time course in reading, writing, speaking, and perceiving words: a genuine

[76] One of which does not exist because, according to Wernicke, there is probably no direct connection between the optical center and the conceptual center.

[77] Wernicke (1885/6) on Grashey (1885) ". . . ich stehe nicht an, sie für den bedeutendsten Fortschritt in der Aphasiefrage zu erklären, den wir in den letzten 10 Jahren zu verzeichnen haben" (p. 110).

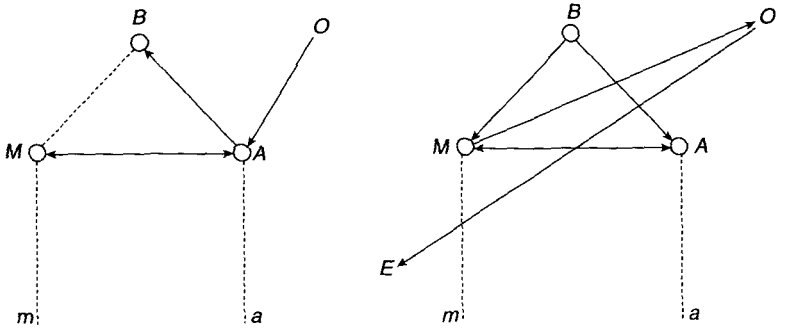


Fig. 3.9 Wernicke's proposals for reading (left) and writing (right). Reading follows path *O-(A–M–A)–B*, where *A–M–A* has to be repeated for each successive letter. In spontaneous writing the concept activates both its movement and sound images (in *M* and *A*). An iterative interaction between these two spells out the individual speech movements for each letter, which in turn activate the visual letter images in *O*. From there the individual letter-writing movement gestures in *E* are excited.

innovation. It fascinated Wernicke, because Grashey described a patient whose syndrome was not explicable by lesions within the "Lichtheim house," but instead by a memory defect, i.e., a very rapid decay of memory for sensory impressions. A heard speech sound or a seen letter would have no persistence in the auditory or the optical center. Wernicke was happy to consider this a clear case of *amnesic* aphasia, distinct from the seven syndromes in the Wernicke–Lichtheim model. For him it would end the rather chaotic use of the term "amnesic" in the aphasia literature, which was used indiscriminately for any patient with an inability to recall words.

Relating functional to neural architecture was, once more, the main concern for Wernicke in this paper. He happily reported the wide agreement that had been reached on the localizations of the center for speech movements in Broca's area, and his own center for speech sound representations in the first temporal gyrus. In addition, many details of the motor tract out of Broca's area through the basal ganglia had become clarified.[78] At the same time, Wernicke, following Lichtheim, warned against attempts to localize the conceptual center *B* in a single site. Instead he proposed that the conceptual center must involve a variety of association areas around the end points of the sensory channels in different modalities (such as vision, audition, touch, and smell). In spontaneous speech "the whole cortex as organ of consciousness is the principle of the motor speech center,"[79] and *B* is also the recipient of messages from the sensory speech center. Similarly Wernicke argued against a dedicated optical site for visual letter and word images (O). It seemed unlikely to him (in contrast to Darwin, say) that this had become a hereditary brain site. The existence of a localized center for writing movement

[78] Not least in his own 1884 paper on the subject.

[79] "... sind beim spontanen Sprechen die gesammte Grosshirnhämispheren als Organ des Bewusstseins die Auftraggeber für das motorische Sprachcentrum b_1." (Wernicke 1885/6, p. 99).

(*E*), as proposed by Exner, seemed equally unlikely to him. Although you normally write on paper with the right hand, it is entirely possible to do it in the sand with your left foot; these do not share an innervation site.

It should be noted that the Wernicke–Lichtheim model deals with the processing of *words*. Wernicke could not always suppress his disdain for philosophers and linguists (in particular for Steinthal) who consider the sentence as the basic unit of speech. Words are the hard core of language and speech; the clinician should not go beyond them because all the rest is a matter of general consciousness. When Broadbent proposed a "propositioning center" for the building of syntax, Wernicke judged that the "ability to think something and to want to express it, can only occur in parallel to the state of consciousness and cannot be localized."[80]

Finally, in his paper Wernicke attempted to settle the terminology conundrum. It was too late, he said, given current practice, to follow Lichtheim in restricting the term "aphasia" to just motor syndromes. Let us instead talk, Wernicke proposed, of "sensory" and "motor" aphasias. There were to be three varieties of each. When speech centers *A* or *M* are knocked out, you have "cortical sensory" (3) and "cortical motor aphasia" (1), respectively (the numbers refer to those in "Lichtheim's house," Fig. 3.8). When connections between these centers and the conceptual center are disrupted, you have the "transcortical" sensory (6) and motor (4) aphasias. When acoustic path into *A* (7) or the motor innervation out of *M* (5) are disrupted, you have the "subcortical" varieties. Finally, there is "conduction aphasia," when tract *A–B* (3) is severed. This terminology for seven primary or pure aphasic syndromes has survived the ravages of time.

Diagram makers and making diagrams

It was Henry Head (1861–1940) who, in 1926, coined the term "diagram maker" in his fierce attack on the German localizationist school, with their "definite location of centres and paths", and "their easy dogmatism at the bedside," being "compelled to lop and twist their cases to fit the procrustian bed of their hypothetical conception."[81] This was clearly unfair, half a century after the first diagrams were proposed. Visual diagrams increasingly figured in nineteenth-century scientific discourse. We have already encountered them in Schleicher's evolutionary language trees ("pedigrees"). Wundt was the first, in 1880, to draw syntactic tree diagrams. Where Broca still exclusively used extensive verbal descriptions to present his autopsy results, Wernicke began drawing schematic diagrams of the brain. But the really innovative idea was to draw the functional architecture underlying the psychological skills, to decompose them into their component processes so as to visualize them. Such diagrams are theoretical conjectures that are—or should be—subject to empirical verification. Although this approach has had its ups and downs in the history of psychology, it now belongs to the basic toolkit of any cognitive scientist.

[80] "Die Fähigkeit, etwas zu denken und aussagen zu wollen, kann immer nur dem Zustande des Bewusstseins parallel gehen und unmöglich etwas Localisiertes sein." (Wernicke 1885/6, p. 98).

[81] Head (1926), pp. 54–66.

Also in Head's own country of England (as he admitted) some of the leading clinicians occasionally drew diagrams of functional language architecture. One of Head's targets was Henry Bastian (1837–1915), who was the first to clearly describe word deafness and word-blindness.[82] Bastian drew a functional diagram to represent auditory and visual word centers and the corresponding motor output centers.[83] That was a failure, according to Head, because the expected lesion localizations did not manifest in the autopsy of the relevant patient. However, Bastian's diagram was a *functional* one. It was Bastian's neural architecture (not diagrammed) that was debatable. But Bastian was a good clinical observer. One of his useful notions was to categorize aphasic patients according to their degree of severity,[84] such that increasing severity could be related to the appearance of more symptoms.

William Henry Broadbent (1835–1907), an excellent neuroanatomist, was Head's next target. He denounced Broadbent's drawing diagrams, which not only depicted auditory and executive speech centers, but also centers for naming and propositioning. Head did not refer to Wernicke (1886) on this point, the latter having also criticized these additions to the functional diagram.[85] In fact, it would have been more appropriate for Head to compliment Broadbent for being one of the first to describe sensory aphasia (in 1872, two years before Wernicke) in terms of a functional disruption: "And if a similar interruption occur in the channel from the auditory centre to the intellectual centre, so that articulate sounds no longer revive the accustomed intellectual associations, the patient will on the one hand be incapable of understanding what is said to him, and, on the other, will have no check on his own utterances."[86]

What was at issue for Head in his confused writing on diagram makers was the issue of relating functional disorders to neural architecture and further to brain damage. He supposed that diagram makers were all-out localizationists, committed to finding precise locations for all their centers and connections. As early as 1867, William Ogle (1837–1915), who also provided the first clinical description of what he called "agraphia" (a specific loss of writing ability), described a large number of cases confirming Broca's region as the critical site in loss of speech, and he added the (then) most comprehensive statistics of hemiplegias in language loss, definitively confirming the Dax findings on hemiplegia in aphasic patients. In 1881, the famous Viennese professor of physiology Sigmund Exner (1846–1926) conjectured that there was a localized writing center in the foot of the second frontal convolution, i.e., just above Broca's area, a conjecture accepted by Charcot, Ballet, and many others (1891). Not only Ogle, but also Broadbent and in particular Bastian, were committed to localizing speech and language functions, including

[82] Bastian (1869).

[83] In his 1897 "Lumleian Lectures."

[84] Bastian (1869, p. 222).

[85] Wernicke (1885/1886, p. 98).

[86] Broadbent (1872, p. 27).

a graphic center. Still, both Lichtheim and Wernicke had been very explicit in denying the possibility of localizing things such as the conceptual center or writing movements center in a single neural site. They knew that there was unlikely to be a one-to-one topological mapping of functional to neural architecture for these cases. We will return to Head's "holism" in Chapter 11.

Sigmund Exner, who had been a student of Hermann von Helmholtz in Heidelberg, should especially be mentioned in connection with a different conjecture that turned out to be largely correct. Exner was the very first to formulate a cohort theory of spoken word perception, some 90 years before Marslen-Wilson (1978, 1984) formulated his initial version of the cohort theory. The core idea of cohort theory is that the initial speech sound of a word activates all words in the listener's lexical memory beginning with that sound. As further speech sounds follow, the initial cohort of activated words narrows down, step by step, excluding non-fitting members until just a single word, the target, remains. This is how Exner (1894) put it:

> When you for instance hear the sound *K*, with . . . very low intensity the traces are activated which in many earlier cases were simultaneously active with the perception of *K* and which correspond to the images of "Knabe" [*boy*], "Kuh"[*cow*], "Kirsche" [*cherry*], "Kugel" [*ball*], "Kern" [*kernel*], etc. . . . This activation doesn't disappear however with the disappearance of the sound *K*, but continues . . . as a trace for the duration of a number of seconds . . . If during the existence of this activation . . . also the sound *I* is heard, then a further bit of activation will be received by those traces that are associatively connected to the sound *I*. This should not mean that the image of Fisch [*fish*] is not also activated by the *I*-sound because of its connection to the *I*-sound, but it is obvious that all images whose name begins with *KI* have a remarkable advantage, because they were already activated by the previous *K*-sound. . . . Hence, the image "Kirsche" will be closer to the activation value needed for clear consciousness than the image "Fisch." In addition, it [the *I*-sound] will . . . suppress the vague images "Knabe," "Kuh," "Kugel," "Kern," etc . . . ["Kirsche"] will however still be at the same activation level with other words beginning with "Ki". . . . If then the further sound *R* is added, the total activation process of the traces in the brain is narrowed down following the same principle, so that only the traces representing the images "Kirsche" and "Kirche" are activated; the further sound *Sch* then hits a relatively very small number of active brain traces, but it is intensive and it will, during the pause that follows completion of the word, develop itself into the full activation of the image traces of "Kirsche."[87]

[87] "Mit ähnlicher, sehr geringer Intensität werden beim Hören, z.B. des Lautes *K*, die Bahnen erregt werden, welche in vielen Fällen gleichzeitig mit der Empfindung des *K* in Action waren und die den Vorstellungen von 'Knabe,' 'Kuh,' 'Kirsche,' 'Kugel,' 'Kern' etc. entsprechen. . . . Diese Erregung verschwindet aber nicht sofort mit dem Aufhören des Lautes *K*, sondern besteht als Bahnung, wie wir gesehen haben, noch eine nach Secunden zählende Zeitdauer fort. . . . Wenn nun während des Bestehens der Bahnung dieser Rindenfasern . . . noch der Laut *I* gehört wird, so werden dadurch aus dem ganzen Bereiche der gebahnten Vorstellungen jene Bahncomplexe einen weiteren Zuschuss an Erregung bekommen, welche asssoziativ mit dem Laute *I* verknüpft sind. Es soll dabei nicht gesagt sein, dass nicht auch die Vorstellung Fisch durch den *I*-Laut gehoben wird, indem auch sie mit dem Laute *I* zusammenhängt, aber es leuchtet ein, dass alle Vorstellungen, deren Wortbezeichnung mit *KI* beginnt, einen bedeutenden Vorsprung haben, da sie durch das vorgehende *K* bereits gehoben waren. . . . Es wird also die Vorstellung 'Kirsche' näher dem Erregungswerthe liegen, bei dem sie dem Bewusstsein klar vorschwebt, als die Vorstellung 'Fisch.' Sie wird weiterhin nach dem

Will we ever have an entirely new idea after the work of such pioneers?

Adolf Kussmaul's textbook

"Unfortunately he was seduced into constructing diagrams," Henry Head wrote about Kussmaul's 1877 monograph, "but his views were not susceptible of schematic formulation and the figure was of such complexity that it failed to make a general appeal." (Vol. I, p. 64). This sounds just like Kussmaul's own judgment of Wernicke's 1874 diagram: "His schema . . . is hard to survey." Kussmaul's main objection was: "Wernicke made the error of drawing the centers in specific regions of the brain. The localization of elementary speech functions is not ready for that."[88] What Wernicke himself had considered as his major addition to Baginsky's approach, to specify the anatomical foundation of the centers, was rejected by Kussmaul. Instead Kussmaul sketched a diagram, reproduced in Figure 3.10, which was purely functional; it took the additional step of including centers and connections for written language and for lip reading. But Head was right that the diagram was of unnecessary complexity. The diagram was actually of minor importance in the book and we will not consider it in detail, except for one point: in his explanation of the diagram, Kussmaul stated that volitional speech follows the trajectory *c–b–d*. This means that the innervation of the word motor image must always run through innervation of the word sound image; there is no direct path from *J* to *C*. Or in terms of "Lichtheim's house," spontaneous speech takes path *B–A–M–m*; there is no direct *B–M* innervation.

Adolf Kussmaul (1822–1902, Fig. 3.11) was a professor of internal medicine in Strassbourg from 1876, and wrote his monograph for the 12th volume of Ziemssen's *Reference book (Handbuch) of Disorders of the Nervous System*. It appeared in 1877 as an

Prinzip der centralen Hemmung die dunklen Vorstellungen 'Knabe,' 'Kuh,' 'Kugel,' 'Kern' etc. unterdrücken, sie wird aber nicht allein dies thun, da sie mit der Lautfolge 'Ki' noch nicht voll entwickelt ist, vielmehr wird sie . . . noch auf gleicher Erregungsstufe stehen mit den Vorstellungen, welche anderen mit 'Ki' beginnenden Worten angehört, und diese werden gemeinschaftlich die centrale Hemmung erwecken. Reiht sich dann weiterhin der Laut *R* an, so wird der gesammte Erregungsprocess der Rindenbahnen nach demselben Principe noch weiter eingeschränkt, so dass etwa nur mehr die Bahnen, welche der Vorstellung 'Kirsche' und 'Kirche' entsprechen, gebahnt sind; der weitere Laut *Sch* trifft nur mehr eine verhältnissmässig sehr geringe Anzahl von Rindenfasern gebahnt, diese Bahnung aber ist eine intensive und wird mit der Pause, welche nach Vollendung des Wortes eintritt, sich zur vollen Erregung der Vorstellungsbahnen der 'Kirsche' entwicklen können." (Exner 1894, pp. 307–308).

Note that Exner just missed the notion of "uniqueness point." On the other hand, he did predict the (minor) later activation of additional words that were not members of the word-initial cohort, in his example "Fisch" when the *I*-sound appears.

[88] "Wernicke beging den Fehler, die Zentra bestimmten Gegenden des Gehirns einzuzeichnen. Dazu ist die Lokalisation der elementären Sprachfunktionen nicht reif. Sein Schema berücksichtigt nur die Lautsprache und ist schwer zu übersehen." (Kussmaul 1877/1910, p. 194; here and in the following, page references are to the 1910 edition).

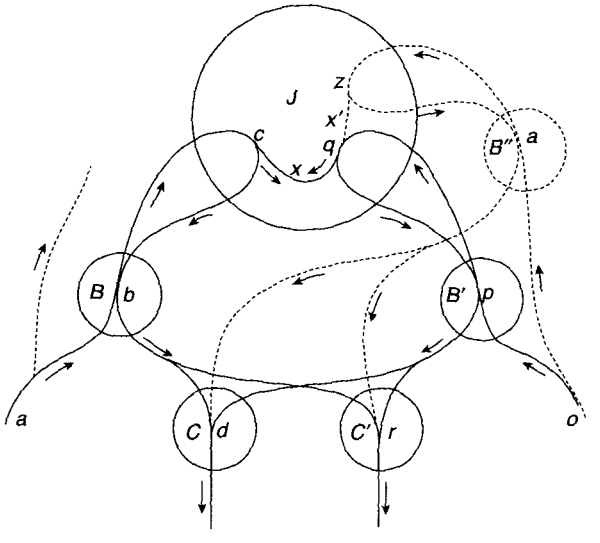


Fig. 3.10 Kussmaul's 1877 diagram. *J* is the conceptual center, *B* and *B'* are the spoken and written sensory input centers, C and C' are the motor output centers for speaking and writing, *B''* is the optical sensory input center for lip reading.

appendix volume to the main 1875 edition. But this appendix soon began to have a life of its own. The text appeared, independently and unchanged, in three subsequent editions. Over several decades it was used as a clinical textbook, as it was the first comprehensive monograph on disorders of speech and language. In his preface to the fourth edition of 1910, Hermann Gutzmann wrote: "Kussmaul's sharp logical presentation is moreover clothed in the seductive forms of elegant diction and often glowed through by sunny,

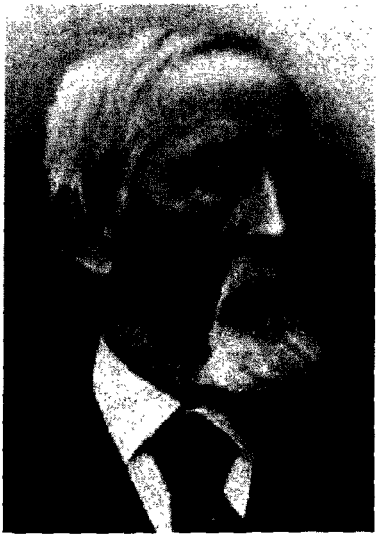


Fig. 3.11 Adolf Kussmaul.

Olympic humor, which makes the reader not infrequently forget that he has a rigorous scientific work in front of him."[89]

Indeed, it is still a pleasure to read this very first psycholinguistic textbook. The author discussed every conceivable speech condition—from atactic (Broca's) aphasia to amnestic, paraphasic, and agrammatic aphasia, from word deafness to word blindness and agraphia, from stuttering to hesitant and confused speech—but still never becomes tedious. That was not only due to the lucid writing style, but also to the broad theoretical framework Kussmaul adopted. The book begins, in staccato style, with short chapters on the nature of human language and its origins. What Steinthal could not achieve, i.e., writing concisely and clearly about the origins of language, Kussmaul could. He was well-informed about the language-origin discussions of his day, and often respectfully, but not uncritically, cited Steinthal, Müller, and others. It was Steinthal's inspiration that made Kussmaul introduce the term "agrammatism" (Steinthal's "akataphasia") to denote a specific inability to "grammatically form words and to order them syntactically in the sentence."[90] He also coined the term "paraphasia" for the jargon produced by some aphasics, target words in an utterance being replaced by sound- or meaning-related ones. There is a whole chapter on grammatical disorders, in which Kussmaul displayed his knowledge of inflection and syntax. Following the linguist Gabelentz, he claimed that in most languages speakers prefer to express the psychological subject (what the sentence is about) before the psychological object (what is said about it). This is most markedly and most obviously the case in Deaf sign language, where the subject is placed before the attribute ("horse black"), and the object before the action ("hat black bring"), etc.

Early in the book, Kussmaul formulated a stage-theory of the speaker's formulation, which is relevant even today. First, there is the stage of "preparation in mind and mood" with the outcome of the "*thought* that we have *conceived*, and an *affective urge* which drives us to express it."[91] Second, there is the stage of "diction" or "the building of internal words, together with their *syntax*," selecting "the *words* that the language we have acquired makes available to us in memory."[92] And, finally, there is "the *articulation* or the composition of overt words or expressions, irrespective of their coherence in the expression";[93] this is an entirely reflexive mechanism. This triplet of conceptual preparation, formulation, and articulation still figures in modern theories of speaking.[94] Kussmaul

[89] "Kußmauls scharf logische Darstellung ist zudem in die verführerischen Formen einer eleganten Diktion gekleidet und oft durchglüht von sonnigem, olympischem Humor, der den Leser nicht selten vergessen läßt, daß er ein streng wissenschaftliches Werk vor sich hat." (Kussmaul 1877/1910, p. iv).

[90] "... das Unvermögen, die Wörter grammatisch zu formen und syntaktisch im Satze zu ordnen." (ibid. p. 164). "Paraphasie" (same page).

[91] "... so bedarf es zuerst eines *Gedankens*, den wir *konzipiert* haben, und eines *gemütlichen Antriebes*, der uns drängt, ihn zu äussern." (ibid. p. 14).

[92] "... die Diktion oder die Bildung der inneren Worte samt ihrer Syntax" (p. 14). "die Worte , die uns die erlernte Sprache im Gedächtnis zur Verfügung stellt." (ibid. p. 14).

[93] "... die Artikulation oder die Bildung der äußern Worte" (ibid. p. 14).

[94] For example elaborated in Levelt (1989).

used the stage account throughout the book to handle disorders of verbal expression from the perspective of the normal speaker. In his extensive treatment of paraphasias, for instance, Kussmaul signaled that normal speakers also produce them as slips of the tongue, due to lapses of attention. In discussing the role of attention, Kussmaul adopted Steinthal's useful notion of narrowness of consciousness. You can be conscious of the *theme* of your sentence (prepared in stage one), "but the syntactic development of this thought into words runs its course by the speech mechanism half-automatically"[95] (i.e., in stage two). It is not mainly the thought that reigns here, but "the linguistically lawful word, the laws of grammar and syntax are powers, with which the thought has to cope."[96] It is not necessary for the speaker to keep the whole developing sentence in consciousness. Where the narrowness of consciousness makes such full consciousness impossible, there is the "instinctive power" of arranging words, unconsciously, around a few hinge words in the sentence. Wernicke clearly took no inspiration from such linguistically motivated considerations, but others, in particular Wilhelm Wundt, did, as we will soon see.

Kussmaul's discussion of the third stage, preparing and executing articulatory movements, far transcends Broca's and Wernicke's global characterization of dysphasias. At this stage, words are composed of individual speech sounds and syllables. For normal speech to occur these elements should, first, be firmly joined together in the word image and, second, correctly strung together in the motor coordination center. These are distinct mechanisms, the former presumably cortical, the latter subcortical.[97] If this process fails, the syllabification becomes disrupted and speech sounds end up in the wrong position or are replaced by closely related ones.

Kussmaul's broad treatment also includes a chapter on the infant's and toddler's acquisition of speech. This was remarkable because at the time there was hardly any systematic knowledge of this topic. He drew a parallel between the first three stages of speech development and the phylogenetic origins of language. The child's first productions are reflexive, primordial sounds; mostly vocal and lip sounds. In a second stage, "these random sounds are slowly superseded by the common ones of the mother tongue."[98] These imitation sounds are initially of a simple interjectional kind, pure vocals or consonant–vowel repetitions, and they are usually still meaningless. Only in the third stage do they acquire meaning, initially often onomatopoeic as in primeval man.

Not surprisingly for a book on clinical neurology, there is an extensive chapter on the brain structures involved in speech and language. Kussmaul adopted the then dominant

[95] "... das Thema des Satzes steht allein klar vor uns, aber die syntaktische Entwicklung dieses Gedankens in Worten geht durch den Sprachmechanismus halb automatisch vor sich." (Kussmaul 1877/1910, p. 209).

[96] "... das sprachgesetzliche Wort, das grammatische, das syntaktische Gesetz sind Mächte, mit denen der Gedanke zu rechnen hat." (ibid.).

[97] See ibid. pp. 52, 83, and 221. This anticipates the distinction between phonological and phonetic encoding (as in Levelt *et al.* 1999).

[98] "... werden diese wilden Laute allmählich durch die gebräuchlichen der Volkssprache verdrängt." (Kussmaul 1877/1910, p. 48).

reflex arc neurology and in particular Meynart's theory of projections. Throughout the monograph, well-chosen case descriptions exemplify the speech and language disorders resulting from anatomical, physiological, and functional derangements, thus naturally providing a comprehensive review of speech and language pathology.

Why was Wernicke so irritated by Kussmaul's book? It was, Wernicke said, the fact that Kussmaul in his book:

> . . . newly describes the same form of language disorder which I sketched above and called sensory aphasia, without acknowledging my claims to the record. Newly describes is not saying too much, because Kussmaul at the same time sent it into the world newly baptized as word deafness, without informing the reader that it had previously existed as sensory aphasia . . . In addition, Kussmaul, in total denial of the principles involved concealed that I had localized the sketched defect on the basis of two autopsies, to wit in a lesion of the first left temporal gyrus.

According to Wernicke, not only is "word blindness" a misnomer, because much more is at issue than not recognizing words—the patient is aphasic—but Kussmaul also ignores the anatomical basis of this disorder which is a grievous omission. "Mr. Kussmaul has had the opportunity to correct his presentation," but he did not. "In affairs of such major principle and importance we cannot let our property rights be encroached upon."[99]

It is indeed enigmatic why Kussmaul essentially ignored Wernicke's discovery. He did cite Wernicke and his 1874 monograph and he was a master in open-mindedly presenting the work of his many colleagues. When Kussmaul began writing his text, young Wernicke, unknown and half his age, had just published his booklet. The anatomical evidence it provided for sensory aphasia clearly did not impress Kussmaul. It took some time before the relevance of Wernicke's discovery became more generally recognized. That had not happened by the time Kussmaul wrote his text. After its publication, Kussmaul, who had just moved from Freiburg to Strasbourg, turned to entirely different matters. He never updated his text for its reprints. But he did, finally, respond to Wernicke's cited attack. He repeated his preference for the term "word deafness" over "sensory aphasia," because such patients can talk and are therefore not aphasic. Wernicke, moreover, was not the first to describe such cases. Others, such as Lordat, Schmidt, and Baillarger, had made similar observations. Also, Kussmaul thought Wernicke's anatomy was dubious. The role assigned to the insula, for instance, connecting the sensory and motor speech areas "floats entirely in the air"[100] and no proof was provided for the localization of the sensory speech

[99] ". . . in welchem er dieselbe Form der Sprachstörung, die ich oben geschildert und sensorische Aphasie genannt habe, neu beschreibt, ohne meine Ansprüche nur der Erwähnung werth zu halten. Neu beschreibt ist wohl deswegen nicht zu viel gesagt, weil Kussmaul sie gleich neu getauft hat und als Worttaubheit in die Welt gehen liess, ohne seine Leser damit bekannt zu machen, dass sie schon vorher als sensorische Aphasie existiert hat." "Des weiteren aber hat Kussmaul in gänzlicher Verkennung des prinzipiellen Gesichtspunktes verschwiegen, dass ich den geschilderten Defect auf Grund von zweier Sectionsbefunde localisiert und zwar auf die Zerstörung der ersten linken Schlafewindung zurückgeführt hatte." "Bei Thatsachen von so grosser prinzipieller Bedeutung können wir unser Eigenthumsrecht nicht antasten lassen." (Wernicke 1883a, pp 184–185).

[100] "Die Lehre des Herrn Wernicke, wonach die Insel der Reflexbogen des Sprachcentrums ist, schwebt ganz in der Luft" (Kussmaul 1883, p. 311)..

center in the first temporal gyrus. Quite probably there was no such narrowly circumscribed speech center. "I do not expect absolution for these my heresies from followers of the modernized Gall doctrine."[101] And so forth. In a short reply Wernicke totally rejected the cases described earlier, because they had not been theoretically explained. Wernicke repeated his priority claims for the discovery of the sensory aphasic speech disorder, its theoretical explanation, and the localization of the sensory speech area in the first left temporal gyrus. Kussmaul "did not hold my corresponding claims worthy of mention."[102] And contemporaries agreed: "As an utter opponent of localization he had yet ignored, without any valid reason, the outstanding discoveries of Wernicke."[103]

One more diagram maker: Jean-Martin Charcot

Diagram making became an industry during the final 20 years of the century. In a wonderful review, François Moutier (1908) presents the diagrams of some 20 different authors.[104] We will not consider the whole body of work, but concentrate on one diagram in particular. This was designed by Pierre Marie (whom we will meet again in Chapter 11) on his teacher Charcot's instruction. In his day Jean-Martin Charcot (1825–1893) was already considered the founder of modern French neurology. His fame attracted students from far and wide, among them Sigmund Freud. His contributions to neurology and psychiatry were formidable, although rather limited in aphasiology. Charcot was, initially, quite sceptical of Broca's story. In 1863 he sent Broca a letter suggesting modifications to his theory. Broca had been present at an autopsy of one of Charcot's patients. That patient had been aphasic, but the brain damage was not in the third frontal convolution, but more in a left parietal region (we would now guess that the patient was paraphasic). However, after Broca's ultimate victory, Charcot joined the general applause. He wrote, in 1883, "Like Broadbent, I have never seen a true violation of Broca's law, and like him I believe that none of the cases presented as counter examples can be held up under serious examination."[105]

Charcot adopted the four word centers that Kussmaul in particular had proposed: one for motor images, one for auditory images, one for visual images, and one for (motor) graphical images. They appeared in his often cited "clock" diagram, here represented

[101] "Die Absolution für diese meine Häresien erwarte ich nicht von den Anhängern der modernisirten Gall'schen Lehre, sondern von den weiteren Aufklärungen der Zukunft." (Kussmaul 1883, p. 311).

[102] "In Wirklichkeit hat er meine diesbezüglichen Ansprüche nicht der Erwähnung werth gehalten." (Wernicke 1883b, p. 315).

[103] "Als äusserster Gegner der Lokalisation hat er dennoch ohne jeden triftigen Grund die vorzüglichen Entdeckungen Wernickes ausser Acht gelassen." (Oltuszewski 1901, p. 42).

[104] In addition to Baginsky, Wernicke, Lichtheim, and Kussmaul, they were Magnon, Charcot, Ballet, Ferrand, Brissaud, Banti, Grasset, Elder, Mills, Grashey, Mouli, Leube, Goldscheider, Freud, Bastian, Wyllie, Shaw, and Hughes.

[105] "Comme Broadbent, je n'ai jamais rencontré de véritable infraction à la loi de Broca, et comme lui je crois qu'aucun des cas présentés comme infirmatifs ne soutient un examin serieux" (cited in Marie 1926, p. 91).

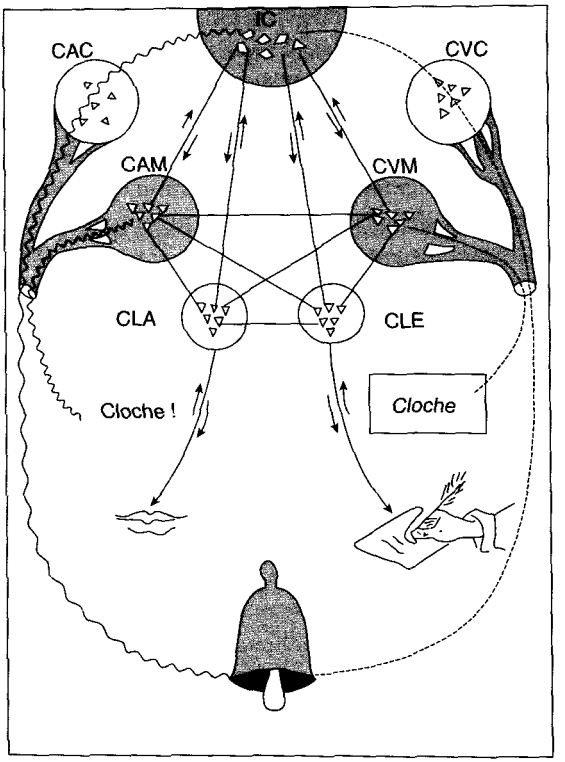


Fig. 3.12 Charcot's 1885 "clock" diagram. IC (on top), ideation center; CAM, auditory word center; CLA, center for articulate language; CVC, common visual center; CVM, visual word center; CLE, language writing center; CAC, common auditory center. The two output centers (CLA and CLE) are also impression or input centers. From Moutier (1908, Vol. I, p. 46).

as Figure 3.12. According to Charcot, each word in the mind has all four components: auditory, visual, motor, and graphic. His major use of the diagram was to explain his theory of psychological types. Language users are, by inheritance and experience, visual, auditive, or motor types. One must know a child's type for an optimal didactic approach.

Charcot's types found some limited application in French aphasiology, in particular in Gilbert Ballet's (1888) clear textbook on aphasia. The book largely follows the Wernicke–Lichtheim classification of aphasias, but being a diagram maker himself, Ballet adds (on p. 165) another functional diagram, based on Charcot's four types.

Some non-localizationist sounds

By the end of the nineteenth century, the localizationists were dominant. After the demise of phrenology it took some time before entertaining localizational views on language in the brain had become prominent again. But after Broca's slow and effortful conversion, the tide had indeed turned. In Germany this was not such an issue to start with, because neither phrenology nor faculty psychology had gained much of a footing there. The dominant reflex arc neurology (Meynert) and psychology (Lotze) provided a natural ground for localizationist views. Still, it was entirely respectable to entertain different views. Two examples will suffice to complete this account of nineteenth-century theorizing on language in the brain.

Sigmund Freud's (1856–1939) first monograph, which appeared in 1891, was on aphasia. Freud had some clinical experience with stroke patients in Vienna, which aroused his interest in aphasia. The monograph, however, is entirely theoretical; it does not provide any new case materials. Freud set out, "to undermine an easy and appealing theory of language disorders,"[106] meaning the Wernicke–Lichtheim model. He did this in two ways. First he reviewed some of the phenomena that were not adequately accounted for by the model. Second he criticized some of the model's basic assumptions.

An example of the first kind is Freud's critique of the treatment of paraphasia by Wernicke. A patient with central sensory aphasia is paraphasic, according to Wernicke, because self-monitoring breaks down when the acoustic sound images in center *A* are disrupted. But then why is there paraphasia in conduction aphasia, i.e., if the connection between sound center *A* and motor center *M* is disrupted? According to Freud, in that case the patient can still normally perceive his own production. If it activates a non-intended concept (in conceptual center *B*), a correction from *B* is possible, because path *B–M* from the conceptual to the motor center is still intact.[107] A solution within the model would be to entirely skip the direct *B–M* path (as proposed by Kussmaul previously). Freud added several such considerations, some more reasonable than others, to argue for an approach that was altogether different.

It would be necessary, according to Freud, to reject the distinction between disruption of centers and disruption of connections. What is a center in the model? It is a site where word images are stored, but that is a curious conception, according to Freud. How can a word image (a psychological entity) be localized at the end point of a sensory or motor nerve (a physiological entity)? The psychical can only be a parallel process to the physiological, or, citing Hughlings Jackson, a *dependent concommitant.*[108] It is particularly absurd to localize a word image in a single nervous cell, Freud repeatedly claimed, as such misrepresenting Wernicke's view.[109] According to Freud, it is not the case that the language processing region in the brain consists of gray-matter cortical centers and their white-matter connections. Rather, there is only gray matter, i.e., connected nerves all over the relevant region of the cortex.

Freud then proposed replacing this localizational center/connection view by a different one, which can be summarized as follows. There is one continuous language region in the cortex with four local anchor sites: the entrance points of the acoustic and the optical nerves, and the exit points of the nerves that innervate the speech motor and handwriting musculature. The highly complex speech/language association region spans these four "corners" (somewhat like the roof of a tent between its poles). The effect of a lesion will depend on *where* it affects the region. The symptoms will be more sensory, motor, alexic,

[106] "Ich habe eine bequeme und ansprechende Theorie der Sprachstörungen zu erschüttern gesucht" (Freud 1891, p. 106).

[107] As mentioned before, this roughly corresponds to "outer loop" self-monitoring in Levelt (1989).

[108] Freud (1891, p. 57).

[109] Ibid. p. 3, p. 56. He may not have misrepresented Meynert's views. Like Wernicke, Freud had spent some time with Meynert in Vienna, but in contrast to Wernicke, he came to reject Meynert's projection theory, as Meynert did Freud's work on hypnosis.

or agraphic, depending on the distance to the corners, but mostly they will be some mix of all four. And the effect will also depend on the size of the lesion, larger lesions causing more severe aphasic symptoms than smaller ones. Here Freud adopted Bastian's severity classification. In less severe cases, the patient still keeps some voluntary control of speech and writing, but this control disappears under more severe conditions. With these tools in hand, shifting the lesion around from corner to corner and varying it in severity, Freud sketched some characteristic aphasic syndromes. According to him they are of three major kinds: purely verbal aphasias (where word-internal information is affected, i.e., by lesions in the region spanned by the acoustic and the speech motor "corners"), agnostic aphasias (where object recognition is affected, i.e., by lesions in the optical field), and asymbolic aphasias (where word denotation is lost, i.e., through lesions of connections between the former two regions). Freud admitted that his was not a comprehensive treatment. "I know well that the preceding expositions cannot have left the reader with a satisfying impression."[110] And that was indeed the fate of this rather singular monograph: it was largely ignored by later students of language in the brain.

Whereas Freud was clearly challenged by the localizational approach to aphasia, John Hughlings Jackson (1835–1911) was not. Hughlings Jackson's main clinical interest was in epilepsy. He was 32 years old when he joined the staff of the Hospital for the Epileptic and Paralysed in London and he stayed there till the end of his career. He published on and off about aphasia, mostly in the form of case studies. His last paper on this topic appeared in 1894, but there was never any mention of sensory aphasia, of temporal lobe involvement in speech understanding, let alone of Wernicke or Lichtheim. Jackson rather positioned himself in the tradition of Baillarger, Gratiolet, Dax, and Broca: his life-long concern was for the differential roles of the left and right hemispheres.

In addition, he was inspired by Herbert Spencer's evolutionary theory of mind, which claimed a progressive transformation from the simplest reflex actions to the most complex forms of intelligence: "If we study the development of the nervous system, we see it advancing in integration, in complexity, in definiteness. If we turn to its functions, we find these similarly show an ever increasing inter-dependence, an augmentation in number and heterogeneity, and a greater precision."[111] Jackson liked applying Spencer's notion of "dissolution," the inverse of evolution, to mental disorder. In dissolution the higher, later-acquired volitional functions of mind give way to the older, more primitive automatic processes. This occasioned Jackson to distinguish negative and positive symptoms in mental disorder, and in particular in disorders of language. In the case of loss of articulate speech, the negative symptom is the patient's inability to willfully speak. This is not so much the inability to produce words, but the deeper inability to propositionalize, whether overtly or internally, the proposition being the unit of speech. There is no single faculty of speech that can be lost. Rather, the patient "has lost those words which serve in

[110] "Ich weiss wohl, dass die vorstehenden Auseinandersetzungen dem Leser keinen befriedigenden Eindruck hinterlassen haben können" (Freud 1891, p. 106).

[111] Spencer (1855), cited from 1887 edition, p. 627–628.

speech, the nervous arrangements for them being destroyed."[112] Other ways of propositionalizing, through writing or pantomime, are not preserved when there is real loss of speech.

Positive symptoms in loss of speech are the patient's retained ability to understand speech and to utter the occasional stock phrase. These abilities are retained in the intact right hemisphere and they are fully automatic, according to Jackson; only speaking is voluntary, speech understanding is not. Aphasia, then, is dissolution of control. Jackson thought in terms of hierarchies: the left, educated hemisphere exerts (and must exert) control over the more primitive, automatic language processes in the right hemisphere. When control fails, automatisms are released and have free play. These hierarchical relations in language are properties of the frontal regions of the brain. The hierarchy reverses, however, for the intellectual, conceptual functions. Recalling at will objects, places, persons, as well as recognizing them, is a property of the right hemisphere's "hinder part," "in the region of the thalamus opticus." And it "would seem that the posterior lobes are more important in intellectual operations."[113] Here, Jackson approvingly cited Bastian's remark that the evidence "points to the posterior rather than to the anterior lobes of the cerebrum as those concerned more especially with the highest intellectual operations,"[114] but he added the idea of a cross-over in the hemispheric control of language and intellectual functions. Disorders of posterior right hemisphere lead to "imbecility."[115]

The cases Hughlings Jackson discussed are almost all cases of loss of speech, i.e., Broca patients. Jackson provided extensive phenomenological analyses of the automatic utterances preserved in such patients.[116] Words such as *yes* and *no* can sometimes still be used voluntarily, but more often they group with a few, recurring utterances that are produced automatically, as emotional reflexes. They can be interjections, such as *oh!* and *ah!*, they can be "inferior speech," such as swearing. But sometimes emotion can release real speech, such as in the speechless Broca patient who uttered *fire* when she noticed a fire across the street from the wards. Finally, Jackson conjectured that some last phrase the patient was producing or preparing at the moment of stroke or accident, such as *Come on*, or *Oh! my God!* may turn into a recurring, automatic utterance.[117]

Hughlings Jackson's careful clinical descriptions, in particular of emotional language, were often cited, for example in Kussmaul's text and in Freud's monograph. Max Müller extensively cited Jackson[118], summarizing that "if a certain portion of the brain on the left side of the anterior lobe happens to be affected by disease, the patient becomes unable to use rational language: while . . . he retains the faculty of emotional language."

[112] Hughlings Jackson (1878–79/1996, Vol. II, p. 160).

[113] Ibid. p. 142.

[114] Ibid. Bastian (1865, p. 71).

[115] Ibid. p.144.

[116] Hughlings Jackson (1879–1880/1996, Vol. II, p. 171).

[117] Ibid. p. 172.

[118] Müller (1887, p. 200–220).

Müller used this fact in support of his own distinction between emotional and rational language. Emotional language is still around in our very limited vocabulary of interjections and sound imitations, emotional expressions which cannot have been at the origin of real language because we share them with animals. Rational language is real, conceptual language that is unique to humans. Another much-cited statement of Hughlings Jackson concerns the issue of localization: "To locate the damage which destroys speech and to locate speech are two different things." (1874/1996, p. 130). Localized damage reverberates throughout the whole brain. The resulting symptoms are not specifically due to loss of function in the damaged region. Hughlings Jackson would become the patron saint of twentieth-century holists.

Retrospect

Although the two examples of Freud and Hughlings Jackson show that by the end of the nineteenth century there was still substantial diversity of opinion about matters of brain and language, it is also obvious that spectacular progress had been made from the physiognomic beginnings early in the century. This chapter discussed some of the highlights, but they are the peaks of an intensive research effort. A flood of case descriptions appeared in the medical journals and autopsies were performed by the thousand. This tedious clinical work was combined with creative, even bold theoretical conjectures about what we have called the neural-to-functional architecture. In 1890 William James rightly judged, "Meanwhile few things show more beautifully than the history of our knowledge of aphasia how the sagacity and patience of many banded workers are in time certain to analyze the darkest confusion into an orderly display."[119] The functional architectures proposed still lie at the root of psycholinguistic process theories today. Their relation to the neural models varied throughout the century between locationalist, regional, and diagram or network types. That range, or rather the tension of theoretical perspectives, was not lost in later developments of psycholinguistics.

[119] James (1890, Vol. I, p 56).

Chapter 4

Language acquisition and the diary explosion

Perspectives on language acquisition

Child language has no single, natural habitat in science. When Rousseau discussed it in his *Émile* of 1762, his perspective was educational. He intended to revolutionize educational practice and called upon teachers to carefully observe their children: "Hence, begin by better studying your pupils, because surely you don't know them at all."[1] Knowing your child is a condition for its "natural" education. But his own observations on children's language were actually quite limited. Still, many pedagogues took his advice seriously and began collecting observations on children's language. Historical linguists, as we have seen, had an entirely different interest in child language. Remember Steinthal's early belief that "the laws that are still today operative in the child's acquisition of language, were also the driving forces in the invention"[2] of language, and Max Müller's ridicule of it. A standard point of view among physicians and physiologists was that both the normal and the disordered development of the child's articulatory and hearing abilities emerged as innate "reflex actions." Together with evolutionary biologists, such as Darwin himself, they theorized about the phylogenetic development of speech in the human species and its potential innate recapitulation in ontogenesis. And last, but not least, there were the psychologists. They considered the ontogenesis of language as the natural companion of conceptual development. Child language was seen as a chapter of developmental psychology. How does the growing speech sound repertoire increasingly associate with images and ever more abstract concepts? Vocabulary growth, the acquisition of morphology, and word and utterance meaning became central themes of this work.

These diverse approaches did not converge until the 1880s, but they did have one thing in common. Most publications were authored by the parents of at least one of the observed young participants. Diary keeping became widely practiced by the end of the nineteenth century, with ever-developing standards of precision, objectivity, and comprehensiveness. This diary tool, the biographical method, would survive all theoretical and empirical hurdles up to and including the present day.

[1] "Commencez donc par mieux étudier vos elèves; car très-assurément, vous ne les connoissez point" (Rousseau 1762, Préface, p. 3).

[2] "insofern die Gesetze, die heute noch beim Erlernen der Sprache sich in jedem Kinde wirksam zeigen, auch die treibenden Kräfte bei der Erfindung waren" (Steinthal 1855, p. 233).

Later in this chapter we will consider some of the leading ideas and controversies that shaped language acquisition research during the nineteenth century, but as a chronological frame of reference it is important to first introduce some of the landmark scholars and their work within that history.

Early scholars of language acquisition

Jean Héroard

A century and a half before Rousseau's *Émile*, one of the greatest efforts ever in diary writing was taking place. Jean Héroard (1551–1628) was the doctor assigned by King Henri IV to monitor the bodily and mental development of his son, the dauphin Louis XIII. Héroard began keeping a diary from Louis' birth on September 27, 1601, and continued doing so for over 26 years, till January 29, 1628. The author died ten days later, on February 8. Regrettably, the first three years of the diary have not been preserved in the original version. However, an apparently rather precise transcription thereof, with only "shocking details"[3] deleted, has survived. A condensed version of the diary was ultimately published in 1868, with an extensive introduction by the editors, Soulié and Barthélemy.[4] A much more extensive publication was edited by Foisil (1989).

The diary contains, on average, about one observation per day, although the rate is much higher during the first few years. It provides an extensive record of all the dignitaries who came to pay their respects to the infant dauphin: the recording of speech development is not Héroard's priority. There were only two observations during Louis's first year of life. At the age of 0;7[5] the dauphin said *ghi* in response to a lady calling him *mon ami.* Shortly before his first birthday, he said *hé* to draw somebody's attention. The record of the second year is hardly better, but then there were over 80 cited utterances recorded during the third year of life. At 2;8 little Louis is recorded hiding in the doctor's study and saying: *Moucheu Heoua, je suis en vote petite chambe.* ("Mister Heooua, I am in your small room"). When, at 2;9 his wet nurse offered him her breast, he turned his back on her, saying *Faites teter mon cu.* ("nurse my ass"). Héroard also jotted down some speech-corrections. The queen had asked the child *Donnez-moi de votre soucre* ("Give me some of your suger"), using his pronunciation of *sucre.* Louis (at 2;11) answered: *Du soucre! du sucre.* Somebody, also using the dauphin's pronunciation, said *J'ai vu qu'il étoit proculeur* (instead of *procureur*). The child (3;0) then laughed and said *Il a dit proculeu!* Héroard asked *Comment faut-il dire?* ("What should you say?") and Louis answered: *Procureu.* These examples show Héroard's effort to provide precise transcriptions of the child's

[3] "on a supprimé la plus grande partie des détails qui choquaient Tallemant des Réaux" (Héroard 1868, p. LXIX).

[4] There is a Google edition of Vol. I, years 1601–1610: http://www.archive.org/details/journaldejeanhr00hrgoog.

[5] We follow Clara and William Stern's standard notation, 0;7 meaning seven months, 2;8 meaning two years and eight months.

speech, which makes the journal an important source on spoken Middle French and its acquisition.

A first, preliminary, psycholinguistic analysis was provided by Ingram (1981).[6] The diary was completely ignored during the wave of diary studies emerging during the last quarter of the nineteenth century; it left no trace in the acquisition literature. Even Leopold's quite comprehensive *Bibliography of child language* (1952) does not mention it.

Dietrich Tiedemann and Moritz von Winterfeld

Dietrich Tiedemann (1748–1803) had one claim to fame: his critical empiricist attack on Immanuel Kant and the latter's response. We also met him in Chapter 2 as an author on the origins of language. Tiedemann was a professor of philosophy, first in Kassel and then from 1786 in Marburg. Here we are concerned with the diary he published in 1787 of his son Friedrich's development. By the end of the eighteenth century Rousseau's theories became increasingly influential in German pedagogical circles.[7] Joachim Heinrich Campe, an enlightened theologian and pedagogue in Hamburg, organized a Society of Practical Pedagogues who jointly published, in the period from 1785 to 1792, a General Revision of the educational system in 16 volumes. In his preface,[8] Campe pleaded for well-off philanthropists to make available a thousand thaler for a competition on diary writing. Such a diary on "the bodily and mental changes of a child" from birth would "indescribably"[9] enrich our knowledge of the growing child. This idea must have been around for some time among enlightened educationalists. Tiedemann had kept his diary from 1781 to 1784. There is no evidence that Campe's competition materialized, but a few attempts in diary writing followed his plea; it may also have driven Tiedemann to publish his existing diary.

Tiedemann's is the first and most interesting diary that survives from that period. It is a loose, 40-page observational record of Tiedemann's son Friedrich's first 30 months from his birth on August 23, 1781. Its main value is that it is a primer, the first psychological record of its sort since Héroard's. I came across some 15 observations on Friedrich's language acquisition over the period recorded. Around 7 months, for instance, Friedrich began to imitate spoken sounds, such as *ma*. At 8 months he would point to X when asked *where is X?* At 19 months he would produce a variety of words, but the productions were monosyllabic, usually the word's last or stressed syllable.[10] Preferred onsets were *p*, *t*, *k*, evaded ones were *z*, *sch*, *w*, *st*, *sp*. At 21 months the first sentences appeared, usually combining an infinitive verb and a nominal noun, i.e., no inflection and no articles.

[6] I am grateful to Dan Swingley for pointing me to this paper. See also Ingram and Le Normand (1996).

[7] See Schmidt *et al.* (2003) for an excellent presentation of this movement.

[8] Campe, J.H. (1785–1792, Vorrede, pp. xxiv–xxv).

[9] "Das ist die Anlegung eines Tagebuchs über alle bemerkten körperlichen und geistigen Veränderungen eines Kindes", "der daraus entspringende Nützen unbeschreiblich groß seyn würde" (ibid, p. xxiv).

[10] Forehadowing Slobin's (1973a) operating principle "Pay attention to the end of words."

The record was completely forgotten until Perez published, in 1881, a short French version of "what appeared to me most substantial, pruning the banal and superfluous."[11] That version was, in turn, translated into English by Louis Solden in 1890. Finally, a full publication of Tiedemann's original text reappeared in 1897. Tiedemann was back, now as "the founder of child psychology."[12] That was definitely overdoing the case. The honor should have gone to Jean Héroard.

Another diary from that period was published by the mathematician Moritz Adolph von Winterfeld (1744–1819). His diary mostly concerns the bodily development of daughter Amalie Louise (born January 13, 1785). There is far less on her mental development, even though the announcement stipulates that it will also deal with "the gradual formation of the language of the very simple child grammar."[13] The only observations are these: at her first birthday her only word is *da*. A month later she adds *na* as a refusal. At 1;10 she has *bra* for bread and a name for her brother. At the age of 2;2 she calls meat *Kopf* and (at 2;3) she is unable to pronounce *h*, although this is "one of the easiest letters." At 2;8 she confirms Rousseau, by saying *nich all!* (*not all*) when something is taken from a tray. She does not understand the meaning of these words. That is all, or almost all. There is one sentence recorded. It is from her younger brother Karl in 1790: *Why then did father make me so many smallpox?* (*Warum hat mir denn Vater so vielen Pocken gemacht?*). Indeed, daddy had on his own initiative (successfully!) inoculated his children. Winterfeld was certainly enlightened.

Berthold Sigismund

It took another 60 years before any further advances were made on the diary front. Between 1847 and 1856 four further diary-based studies of infant development were published, which included some focus on speech.[14] The most cited of them was the 1856 monograph by Berthold Sigismund (1819–1864). Sigismund's career alternated between being a family doctor and a teacher. He kept a diary of his son's development while he was a science teacher at the grammar school in Rudolfstadt. He used the diary, but also many observations he had made as a doctor, when he wrote his highly readable booklet on child development during the first two years.

Sigismund dedicated some 50 pages to language acquisition in the second year of life. Linguistic observations on the first year are few, and for us remarkable for their underestimation of the child's capacities. During the first three to eight weeks, the ear is "open," "but the mind is still without sense for the sound waves."[15] The child probably does not yet recognize the mother's voice. Sigismund talked about the beginnings of

[11] "On ne saura gré d'en avoir extrait ce qui me paraissait le plus substantiel, élaguant le banal et le superflu." (Perez 1881, p. v).

[12] "Der Begründer der Kinderpsychologie" (Ufer's Preface to the 1890 edition, p. 1).

[13] Winterfeld (1788, 1791). ". . . allmähliche Bildung der Sprache der ganz eigenen, sehr simpeln Kindergrammatik." (1788, p. 405).

[14] Goltz (1847), Löbisch (1851), Eschericht (1853), and Sigismund (1856).

[15] ". . . aber die Seele ist noch ohne Sinn für die Tonwellen." (p. 17).

babbling, but imitations of speech did not occur, according to him, until well into the second year. The earliest imitations are of tone rather than of articulation. Echoing song may be easier than echoing speech.

But then, there is much carefully observed detail on first words to first sentences during the second year. First words refer to concrete entities in the environment: family members, clothing, animals, body parts, places. They are initially nouns, but then other parts of speech quickly follow, in particular spatial adverbs such as *down* (*herab*), greetings, and interjections. Then come infinitival verbs, such as *eat* and *carry*. The first personal pronoun appearing is *mine*, indicating an early notion of possession.[16] Then come adverbs, *big*, *small*, *good*, *pretty*, *all*, and count words (*one more*).

A strong point, which was only taken up decades later by Wundt and Meumann, is Sigismund's claim about the function of all these first words: "That the little speaker uses the first uttered words at once, mainly or maybe exclusively, as expressions of will," "The protolanguage is nothing but a will made audible."[17]

Sigismund also discussed the child's early phonology. The very first speech sound imitations are, after tone, vowels and onomatopoeic sounds. The first consonants are those that are easiest to produce: *b*, *m*, *n*, *d*, and *s*. Harder to produce and later to appear are *g* and *w*, followed by *f*, *ch*, and *k*, with *l*, *sch* and *r* closing the ranks. This "least effort" notion was to play a major role in evolutionary explanations of language ontogeny. Initially, hard consonants are replaced by easy ones (*k* by *t*, etc.) and consonant clusters are simplified (*toss* for *gross*).

The first sentences appear by the end of the second year, but they are pronounced as sequences of single words. Sigismund liked to draw analogies between the child's speech and the language of "savage" peoples. He pleaded for cross-cultural research on language acquisition: "If only scientific explorers would collect exact observations about the development of the child's mind in uncivilized tribes!"[18]

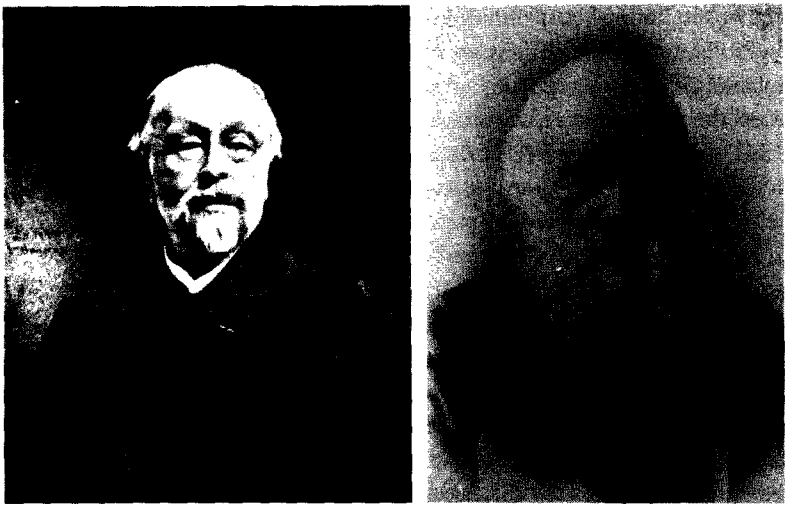
Hippolyte Taine and Charles Darwin

After Sigismund's diary there was silence again for another 20 years. But then, what had been no more than a marginal, scattered business suddenly became an explosive scientific development. The French man of letters Hippolyte Taine (1828–1893) provided the fuse and Charles Darwin (1809–1882) set fire to it (Fig. 4.1). In 1876 Taine had published a report of the diary notes he had made on his daughter's language development, a report making ample reference to evolution theory. In it, Taine stresses the force of the child's own initiative and invention. "In short, example and education were only of use in calling

[16] This observation received strong confirmation in Deutsch and Budwig (1983).

[17] "Daß der Redling die ersten Wörter, welche er spricht, gleich hauptsächlich, vielleicht aussschließlich als Willensäußerungen verwendet" (ibid. pp. 112–113), "Die Ursprache ist nichts als ein vernehmlich gemachter Wille" (ibid. p. 74).

[18] "Wie schön wäre es doch, wenn Eltern aus allen gebildeten Nationen und wissenschaftliche Reisende unter unzivilisierten Stämmen über die Entwicklung der kindlichen Seele genaue Beobachtungen aufzeichneten!" (ibid. p. 69).



(a) (b)

Fig. 4.1 (a) Hippolyte Taine and (b) Charles Darwin. © Library of Congress.

her attention to the sounds that she had already found out for herself, in calling forth their repetition and perfection, in directing her preference to them and in making them emerge and survive amid the crowd of similar sounds. But all initiative belongs to her."[19] The next year the new journal *Mind* published an English translation.[20] This triggered Darwin to publish, in the same year of 1877 and in the same journal, a 10-page *Biographical sketch* of his own son William's development as an infant. The sketch was based on copious notes Darwin had made between 1839 (upon William's birth) and 1841. Darwin being a celebrity, his paper appeared the same year in French, German and Russian, which clearly helped to promote, as we will see, the diary business on a large scale. The sketch includes some observations on the development of William's language skills, hardly more than what Tiedemann had provided, and certainly less than Sigismund. Darwin stressed in particular the invention of first words, such as "mum" to express the wish for food. He also noticed the "instinctive" use of intonation patterns—"voice modulation"—to express various modes, such as interrogation and exclamation. Here he concluded, repeating what he had expressed in *The descent of man,* that "before man used articulate language, he uttered notes in a true musical scale,"[21] suggesting that these were the singing origins of language.

The importance of Darwin's paper was not so much in its content as the fact that in one swoop it made the study of child development a respectable branch of human biology. This included the study of language acquisition.

[19] Taine (1877, p. 253).

[20] Taine (1876 in *Revue Philosophique*, 1877 in *Mind*).

[21] Darwin (1877, p. 293).

Indeed, Charles Darwin is key to understanding language acquisition research during the final decades of the nineteenth century. As we saw in Chapter 2, August Schleicher had been the first and fiercest promotor of evolution theory in linguistics. Schleicher was friends with Haeckel, who formulated the biogenetic law: the idea that embryonic development recapitulates phylogenesis. Haeckel extended this law himself to the child's mental development: "the mental development of every child is only a short recapitulation of that long phylogenetic process."[22] It was, from there, an obvious move for students of language acquisition to conjecture that the ontogenesis of language recapitulates the evolution of language in the human species. In fact, Steinthal had suggested that recapitulation idea as early as 1855. It had been in the air long before *On the origin of species*; Sigismund also had made similar allusions. After Darwin's 1877 paper practically all authors of language acquisition made reference to evolution theory in general and the phylogenetic law (which claims ontogenesis to be a recapitulation of phylogenesis) in particular. Diary keeping became almost a fad, often with explicit reference to Darwin's example (as, for instance, in Pollock 1878 and Deville 1890, 1891).

Jan Baudouin de Courtenay

The phylogenetic law had also been an initial inspiration to Jan Baudouin de Courtenay (1845–1929), a much underestimated Polish linguist, born near Warsaw. My source for his work is Joachim Mugdan's excellent 1984 monograph.[23] In Chapter 7 we will discuss Baudouin de Courtenay's role as a pioneer of structuralism in Europe; here we mention only his work on language acquisition. When he was 23, Baudouin de Courtenay spent a study semester in Jena with Schleicher and Haeckel, where he may have met William Preyer. He then moved to Berlin to study Sanskrit. He can hardly have missed Steinthal there, with whose work he was definitely familiar. His first paper on language acquisition (1869) discussed the parallel of ontogeny to phylogeny, but later in his life he rejected the recapitulation story. This resulted from what made Baudouin de Courtenay unique during the nineteenth century: his huge, systematic collection of acquisition data on the (Polish) language of his daughter and his son. The format of this collection set entirely new standards that were not going to be matched until the work of Clara and William Stern (1907). The database, now in the Warsaw National Library, consists of over 13,000 pages and contains some 11,452 speech transcriptions. They are all in the same systematic format, with three headings: "circumstances" (*Umstände*, i.e. metadata), "phonation" (i.e. transcription), and "cerebration" (*Zerebration*, i.e. interpretation), of which this is an example:

Umstände	***Phonation***	***Zerebration***
36. Monat, 153. Woche, 1065. Tag	*púlo táke ćelvone?*	*Pióre takie czerwone?* ["ist die Feder auch rot?"][24]

[22] ". . . die Seelen-Entwicklung jedes Kindes ist nur eine kurze Wiederholung jenes langen phylogenetischen Processes." Haeckel (1874, p. 706).

[23] With thanks to Magdalena Smozynska, who introduced me to Baudouin de Courtenay's work.

[24] "Is the pen also red?"

Baudouin de Courtenay also collated a similar database from a child with a developmental speech disorder. He argued that the child's pathological phonology at the age of six was not different from a transitional state in early normal development. He also noticed—as Jakobson would do half a century later (and without being aware of Baudouin de Courtenay's acquisition work)—parallels to sound shifts in language evolution, such as the "fronting" of *k* and *g* to become *t* and *d* in the child's speech. Another diary study on the acquisition of Polish was provided by Oltuszewski (1897), a study which was published in German.

Bernard Perez

In France, after Taine's paper, the doctor Bernard Perez (1836–1903) took up the challenge of developing a psychology of the child. In two volumes (1878 and 1886) he presented the development of both infants and children between the ages of 3 and 6 years. Only the first volume has any substantial discussion of language acquisition. It contains a 20-page, rather discursive chapter on language in which Perez reviewed mostly French acquisitional data, among them Taine's and Egger's (1879) on the early development of two boys and a girl. Like Taine, Perez adopted Darwin's evolutionary perspective, but without making Taine's strong nativist claims. Perez took a comparative approach instead, viewing early speech as just another case of animal expressive movement. Perez was a supporter of the notion that the child's early speech development reflects the phylogenesis of language in the human species. It is a window on the intellectual development of man. Perez was much impressed by Preyer ("the most eminent of all"[25] child psychologists) and Preyer's book *The mind of the child*, which appeared four years after Perez's own and which he had immediately translated and published in French. In particular, in later editions of his book Perez adopted Preyer's perspective on speech disorders, to which we will turn shortly.

Fritz Schultze

In Germany, Darwinist Fritz Schultze (1846–1908), a philosopher and pedagogue in Dresden, proposed an explanation for the parallel between the ontogenesis and phylogenesis of language. It was the "law of least effort," he suggested, an idea which we will consider. He formulated it in his succinct technical paper of 1880, *The language of the child* ("Die Sprache des Kindes"), one of the first studies exclusively on child language, to which we will presently return.

Ludwig Strümpell

A different approach was found in Ludwig Strümpell's monograph published in the same year. Strümpell (1812–1899) was a professor of education in Leipzig, who was still working within Herbart's tradition of image mechanics, with some additional touches of reflex theory. His treatise on education contains a chapter on language acquisition, which makes no reference to any other source. It starts from the promising proposition that

[25] "... le plus éminent de tous, le physiologue Preyer" (Perez 1886, p. viii).

speaking is a form of action,[26] but then it sketches the child as an entirely solipsistic mind, somewhat as Piaget would do half a century later. The principle is the somewhat cryptic claim that "the child speaks long in advance, before it speaks."[27] Speech is an entirely inner act, which runs like a reflex arc from an image received to a desire, to mental excitation to a reflex activity of the vocal organs. The first conversations are with the objects in the environment, which acquire an animistic status for the child, akin to the primordial mythical world. Only when the child discovers that the human beings in his environment are the major satisfiers of his desires does speech become an external act. The book almost seems to be describing an autistic child, and one would doubt that this pedagogue ever observed a real infant. But he did: the book provides a 17-page appendix with diary notes of his own apparently perfectly normal daughter Emmy.

William Preyer

Perez was rightly impressed by William Preyer. Preyer's *Die Seele des Kindes* (*The mind of the child*), which appeared in 1881,[28] became a classic of nineteenth-century child psychology. It also became a pivotal source for the study of language acquisition. The third and last part of the book, over 150 pages long, was dedicated to the acquisition of language. It more or less comprehensively reviewed earlier diary studies, such as those by Löbisch, Sigismund, Strümpell, Taine, Darwin, Vierordt, Friedemann, Holden, and Humphreys,[29] but this was preceded by an extensive diary study of his own, based on the speech development of his son Axel over the first 1000 days of his life. This, in turn, was preceded by a thorough review of speech disorders, largely following Kussmaul (1877). How could this be conceived of as an introduction to speech development? Here Preyer followed this principle: "That every known form of adult speech disorder finds its complete reflexion in the child that learns to speak."[30]

Indeed, Preyer's perspective was medical. William Preyer (1841–1897, Fig. 4.2) had been trained as a physiologist. He had absorbed the state of the art in physiology and related disciplines in personal contact with such outstanding leaders as du Bois-Reymond, Kirchhoff, Bunsen, Virchow, Helmholtz, and Claude Bernard. In addition, his first reading of *On the origin of species* had been overwhelming to him: "It was as if suddenly a veil through which I had been looking until then, was taken away."[31] When he wrote his book

[26] Tacitly citing Herbart (1824/1968, vol. 2, p. 233): "Das Sprechen ist ursprünglich eine Art des Handelns" ("Speaking is originally a kind of action").

[27] "das Kind spricht lange vorher, ehe es spricht" (Strümpell 1880, p. 250).

[28] The title page has "1882," but Preyer refers himself to its year of appearance as "1881." Darwin acknowledges reception of the book in his letter to Preyer of October 28, 1881.

[29] Löbisch (1851), Sigismund (1856), Strümpell (1880), Taine (1877), Darwin (1877), Vierordt (1879), Friedemann (personal communication to Preyer), Holden (1877), and Humphreys (1880).

[30] "Dass eine jede bekannte Form der Sprachstörung Erwachsener beim Kinde, welches sprechen lernt, ihr vollkommenes Gegenbild findet" (Preyer 1882, p. 375).

[31] "Es war, als wenn plötzlich ein Schleier, durch den ich bis dahin gesehen, weggenommen würde, und statt dessen das geistige Auge in größere Fernen und Tiefen als bisher zu schauen vermöchte" (Preyer 1896, p. 131).

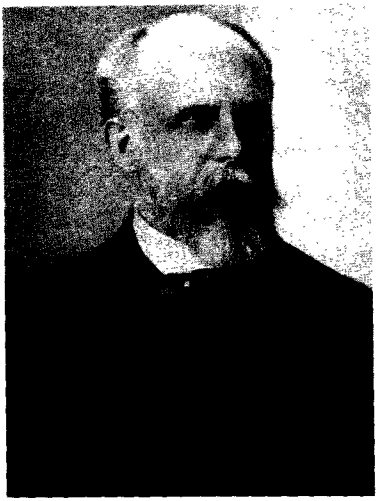


Fig. 4.2 Thierry William Preyer. Image courtesy of Wikipedia Commons.

on child development he occupied the chair of physiology in Jena, where evolutionary zoologist Ernst Haeckel (1834–1919) was his close colleague. As the publishers of the 1989 edition of the book[32] convincingly argue, Preyer's move to write it must have been triggered by Haeckel's challenge, expressed in his 1874 *Anthropogenie*, to scientifically study the "stepwise development of the child's mind"[33] and, in addition, by the 1877 appearance of Darwin's *Biographical sketch.* Later that year, in November, Preyer's son Axel was born and Preyer set out to keep a diary of the child's development.

Preyer used insights from speech pathology to provide a theoretical account of language growth in the child, following the diagram makers. Like Lichtheim, but coming three years earlier, Preyer began with Wernicke's reflex arc with its five components, cf. Figure 3.6, (in Fig. 4.3: the acoustic input *a*, the acoustic reception center *K*, the fiber connection which connects *K* to the motor speech center *M*, and the motor nerves *h* from *M* to the articulators). To this reflex arc he added, like Kussmaul before him and Lichtheim after him, a conceptual center *D*, which he called a diction center. Together, this mapped exactly onto the topology of "Lichtheim's house" (see Fig. 3.8). There was only one further addition. On the basis of Kussmaul's review of speech disorders, Preyer further partitioned the reception center *K* into three subcomponents, a sound center *L* for the reception of any sound, a syllable center *S* for the reception of syllables, and a word center *W* for the reception of words. This he deemed necessary because, according to Kussmaul, patients could be specifically deaf for spoken syllables or for spoken words. Figure 4.3 shows Preyer's diagram[34] and my reshuffling of it to demonstrate its similarity to "Lichtheim's house."

[32] Georg Eckhardt, Eckart Scheerer and Lothar Sprung.

[33] "... die stufenweise Entwicklung der Kindesseele" (Haeckel 1874, p. 704).

[34] Preyer (1882), his Figure 4 on p. 242. The previous page shows an alternative diagram, but Preyer sees no possibility of distinguishing the two empirically.

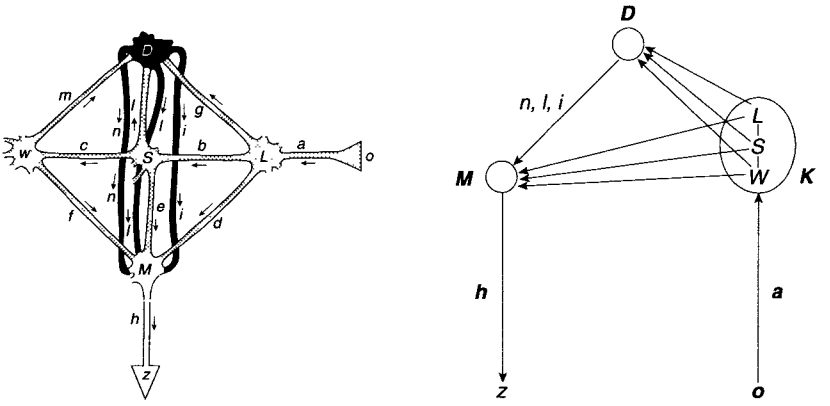


Fig. 4.3 Preyer's diagram of the language/speech system (left) and its mapping onto "Lichtheim's house" (right). *L*, *S*, and *W* are the receptive sound, syllable and word centers, respectively. Together they form the auditory reception center *K*. *M* is the speech motor center and *D* is the central conceptual "diction center."

Language acquisition, according to Preyer, is nothing other than the maturation of this system and all of its components. He worked this out in great detail by focusing on each of the centers and connections, mentioning, on the one hand, which speech disorder results when it is distorted and, on the other hand, how the immature functioning works in the child's language or speech. Here are two examples. When the motor center *M* or its innervation is dysfunctional you have atactic (Broca's) aphasia. This corresponds to the child in a stage where it understands and remembers words that it cannot yet produce. The child is in a phase where it uses expressive intonation and gestures. When the receptive word center *W* or its connection to conceptual center *D* is damaged, the patient is word deaf (Preyer acknowledges Wernicke's alternative "sensory aphasia"). When these connections are not yet fully mature in the child, it can hear but not understand spoken words.

Generally, Wernicke's peripheral reflex arc is the first connection to develop in the child. Its functioning can be demonstrated by the child's correct imitation of spoken speech sounds. But that ability by no means implies that these speech sounds are understood by the child as meaningful. The second connection to develop is "Lichtheim's roof," connecting the reflex arc to the images or concepts that words and their syntactic combinations can express. Whereas the reflex arc mostly matures, the "roof" is slowly built up through imitation, i.e., through learning. Word memory images are deposited in *K* and *M* and they are connected to the corresponding conceptual representations in *D*.

Preyer was a major agent in the formulation of leading ideas in acquisition theory, to which we will come shortly. His influence was substantial, but in Germany he caused, as it were, a refractory period, which would last till 1899, when Ament's short text on the acquisition of language was to appear.

George Romanes

The other major contributor of leading ideas was George Romanes (1848–1894). Romanes, Canadian-born but of Scottish descent, received his academic training in medicine and physiology at Cambridge. There he met and became friends with Charles Darwin. He was a fierce advocate of evolution theory, prolifically publishing, in his short life, several volumes dealing with animal and human evolution. His *Mental evolution in man* (1889) was referred to time and again in the acquisition literature. The book was a full-scale attack on Max Müller's rubicon theory of language evolution, which we discussed in Chapter 2. Remember that Müller had "informed" Darwin that the idea of there being a gradual evolution from ape to *homo sapiens* was untenable because of the simple fact of the existence of human language capacity; that language "rubicon," as Müller had expressed it, could not have been crossed by evolution. *Mental evolution in man* dealt largely with the evolution of language. It was the most elaborate presentation of the phylogenetic law, the ontogenesis of language repeating its phylogenesis.

The book is a pleasure to read. It is a continuous dialogue with his "opponents," his way of referring to Max Müller. It is also a sharp analysis of the psychology of word use, which proceeds, both phylogenetically and ontogenetically through several stages. Each step is one of degree; there is nowhere a single rubicon to be crossed. Romanes was not a diary keeper himself. His empirical approach was anecdotal: "I have fished the seas of popular literature as well as the rivers of scientific writing,"[35] often somewhat uncritically citing empirical "evidence" reported by others. Still, he was a powerful and influential theoretician of language evolution.

Gabriel Compayré and Gabriel Deville

Compayré's (1843–1913) careful chapters on language acquisition in his 1893 book on mental development took from Preyer the analogy of acquisition to adult language disorders and from Romanes the evolutionary stance. Still, more than either of them, Compayré stressed the dominant role of imitation. According to him, the contribution of the child's own invention is limited: "under the social conditions in which the child is called to live right from birth, it is the imitation of the language of others that plays the largest role; it is experience which is the almost sovereign master. Instinct, innateness, personal invention only have a very limited effect."[36] Even stronger, "the innate logic makes of the small child a born enemy of grammar."[37] The logic is analogical derivation. Words engender new words by morphological analogy, often regularizing irregular morphology or syntax. Syntax initially follows the natural order, without any inflectional

[35] Romanes (1882, p. vii).

[36] "Il n'en est pas moins vrai que, dans les conditions sociales où l'enfant est appelé a vivre dès la naissance, c'est l'imitation du langage d'autrui qui joue le plus grand rôle; c'est l'experience qui est presque souveraine maîtresse. L'instinct, l'innéité, l'invention personelle n'ont fait qu'une action très restreinte" (Compayré 1893, p. 246).

[37] "Montrons comment cette logique innée fait du petit enfant un ennemi né de la grammaire" (ibid. p. 249).

marking. Compayré made liberal reference to various data sources, among them Pollock's diary. Curiously absent, however, is reference to Gabriel Deville (1854–1940) and his diary publications (1890/91), which were one of the few French language sources (next to Taine's and Egger's), and moreover were highly systematic. They contain a detailed table of an unnamed child's vocabulary development till the age of 2. The 668 words are alphabetically ordered according to adult language orthography, but for each word its phonetic development over the months is also presented. In addition, for each word, its use in context is described.

Frederick Tracy

Well-organized vocabulary tables were also the specialty of the Canadian Frederick Tracy (1861–1951) of Toronto. Based on his dissertation (with Stanley Hall as supervisor at Clark University), Tracy published a paper on language development in Hall's *American Journal of Psychology*. That paper became a chapter in Tracy's 1894 book *The psychology of childhood*, which became quite popular and went through several later editions (with Joseph Stimpfl as German co-author). The chapter on language presents a careful and informative tabulation of early vocabulary data of some 25 children. One major table provides the parts of speech distributions and word initial consonants for 12 children. The authors do remark that initially part-of-speech categorization is not always possible, but nevertheless they make a statistical comparison of part of speech to the corresponding distribution in adults. This raised complicated questions about type and token distributions, but eventually the authors concluded that young children have a marked preference for verbs over nouns. This is, they mentioned, exactly what Max Müller had observed for the roots of Sanskrit. The child is like primordial man, with activity and movement on his mind. In a response to these statistics, John Dewey (1894) argued that any word-class distinctions in child language should be based on the original sense in which the child used the word.

There are interesting detailed tables on phonetic change. The authors categorize some 700 mispronunciations by the phonemic change that took place. This was done for all target phonemes and for different positions in the word. These tables are used to derive a hierarchy of sound difficulty (from hard to easy: *r, l, th, v, sh, y, g, ch, s, j, e, f, t, n, ng, d, k, o, w, a, h, m, p, b*) and the obvious rule is that the hardest sounds are deleted most, or replaced by easier ones. The book was the first one on child psychology on the American market and made plentiful reference to other American sources. Among these references were Hellenist Milton Humphreys' (1880) diary statistics on a single child and the 1880 diary observations of the famous astronomer Edward Holden. Like many others in the child language literature (including Romanes and Stevenson[38]), they also made uncritical reference to a paper by ethnologist Horatio Hale (1886). That study reported on twin boys who had supposedly invented their own spoken language, completely unrelated to their mother-tongue. "They refused to utter a syllable of English." The ethnologist

[38] Stevenson (1893) also presents tables of sound replacements in a child under 4. There is no reference to Tracy's (1892) paper or to Schultze (1880), but he concludes with a tribute to Horatio Hale.

had not observed the children himself, but did relate that their word for "carriage" "was 'ni-si-boo-a', of which . . . the syllables were sometimes so repeated that they made a much longer word," (p. 287), which indeed sounds like a reliable piece of evidence.

James Sully

Preyer's influence is very much evident in the work of the British child psychologist James Sully (1849–1923). His well-composed and readable 1895 book *Studies of childhood* contains two chapters on language acquisition. They are not based on his own diary studies, but review much of the available data, among them that of Pollock (1878) and an anonymous father's diary on his son "C." In discussing the babbling phase, Sully stressed the importance of auditory feedback in iterative babbling: "there is without doubt in these infantile iterations a rudiment of self-imitation" (p. 137). Gestures, in particular pointing gestures, play an important role when the first symbolic use of language emerges. In fact, intentional gesturing often precedes spoken language, according to Sully: "children are capable of inventing gesture-signs," just like "deaf-mutes," "among themselves" (p. 138). Sully agreed with Darwin that early words are often self-invented, and imitative. But there is no evidence in children of Noiré type co-operative inventions (see Chapter 2).

There is a good review of phonological development with its simplifications and elisions, followed by the "logical side" of acquisition, the development of (word) meaning. Here mechanisms of association, analogy, and generalization are at work. Turning to sentence building, Sully (like Romanes before him) made the perceptive remark that using conventional word class terminology (noun, verb, adjective, etc.) should not imply the claim that words used in the early sentences already have full grammatical value: "substantives do duty as verbs and so forth." (p. 171). Most of his colleagues were less careful in that respect. Word order, Sully claims, often does not follow adult grammar, but it has "some resemblance to the curious order which appears in the spontaneous sign-making of deaf-mutes" (p. 173), which is probably the more natural one. This was becoming a general conviction, also adopted by Wilhelm Wundt (see Chapter 6).

Sully has a refreshing discussion of the child's problems in deciphering the language spoken to him; "we may say that the child does as much in deciphering his mother-tongue as an Oriental scholar in deciphering a system of hieroglyphs" (p. 183). Word meanings are heavily context dependent, and many just do not give away their meanings to the child. Misunderstandings are aplenty. "Many of us have had our special horror, our *bête noire* among words, which tormented us for months and years" (p. 185). Why on earth would we pray "Harold be thy name"? Sully was an evolutionist; his approach was "genetic." For him this meant that the environment had to play a major role in shaping the child's capacity for language.

Kathleen Carter Moore

Kathleen Carter Moore (1866–1920) was probably the first woman to receive a PhD degree (at the University of Pennsylvania) based on a diary study. The dissertation was

published as a monograph supplement of *Psychological Review* in 1896. The fourth and last chapter of the dissertation concerns her son's speech development over the first two years. The text of the chapter is supplemented by a number of tables presenting sound changes, substitutions and omissions in the early words of the child, meaning changes (in particular extensions) of these words over time and the full vocabulary with parts-of speech indication at the end of the observation period. There is also a table on the structure of first sentences, with the distribution of parts of speech therein. In a prefatory note, the author explicitly acknowledges the inspiration she received from Wundt's *Physiological Psychology* and Preyer's *Die Seele des Kindes*.[39]

The first woman to receive a PhD at Berkeley, in 1898, was Millicent Washburn Shinn (1858–1940). The work was based on a diary she kept of her brother's daughter Ruth. It was published as *The biography of a baby* in 1900. Neither the biography, nor the extensive papers based on the diary contain more than a few words on language development.

Wilhelm Ament

Language acquisition was most often studied as a chapter of child development, but some, such as Wilhelm Ament (1876–1956), argued for the establishment of an independent science of child language. In fact, Ament (1899) claimed to have established that science: "After the knowledge of child language has reached such a scale," child psychology should no longer deal with it, but let it go as an "object of an independent science, which one would call *child language science* or something similar, and whose further development should rest exclusively in the hands of trained language researchers. After I have this argument with detached child language science from the framework of child psychology, I am in a position to expound" how both disciplines "are to be treated in the future."[40] And indeed Ament treated language acquisition in its own terms. The work was written under the supervision of Oswald Külpe, Wundt's brilliant student who had founded the Würzburg school of thought psychology (see Chapter 7). Although Külpe had begun studying thought experimentally, both men agreed that it would not be possible to do this for thought and language in the child. The appropriate tools to study the latter are the biographic method and the comparative study of diaries. Observing a single child is not enough, according to Ament, but considering too many children leads to empty statistics. The aim is to produce a grammar of child language. Using diary data

[39] In his rather critical review of the monograph, Sully (1897) remarks that the author had only consulted a condensed translation of Preyer.

[40] "... so wenig hat sich die Kinderpsychologie, nachdem die Kenntnis der Kindersprache einen solchen Umfang erreicht hat, weiter mit der Kindersprache abzugeben und kann das an der eigenen Mutterbrust genährte und grossgezogene Kind als Gegenstand einer selbständigen Wissenschaft entlassen, die man *Kindersprachwissenschaft* oder sonstwie nennen mag, deren weiterer Ausbau aber nur noch von fachgeschulten Sprachforschern in die Hand genommen werden darf. Nachdem ich durch diese Begründung die Kindersprachwissenschaft aus dem Verbande der Kinderpsychologie ausgeschieden habe, bin ich in der Lage, auszuführen, wie das Arbeitsgebiet beider Richtungen der Kinderpsychologie in Zukunft zu behandeln sei" (Ament 1899, p. 26).

on his four cousins plus additional, more anecdotal data from different sources, Ament provided a detailed account of phonological development, the development of word meaning, and the development of morphology and syntax. To explain these developments, Ament played with Haeckel's law, as we will see, and more specifically with the law of least effort. He also used Gutzmann's (1894) idea of comparing phonological development to the structure of speech errors in adults. Meanwhile, Meringer and Mayer's (1895) famous study had appeared and we will take up this story in the next chapter.

The community of child language researchers

These were the major players in the diary-based acquisition research of the nineteenth century. During its last two decades, students of language acquisition began to form a real scientific community. There was mutual reference among authors in different countries. Translations abounded. Taine's paper, for instance, was immediately translated and published in English. As mentioned, Darwin's *Observations* appeared in French, German, and Russian. Perez published a translation of (the forgotten) Tiedemann in French. His own work was translated into English (with a preface by Sully) and German. Parts of Deville's diary were published in German. Preyer's book soon appeared in both French and English. Tracy's book appeared in Germar., as did that of the Italian psychologist Paola Lombroso on child psychology in 1894. Sikorsky (1883) published his work on the acquisition of Russian partly in French and partly in German. German was also the publication medium for Baudouin de Courtenay's and Oltuszewski's studies of the acquisition of Polish. Machado y Álvarez (1885–1887) published his work on the early acquisition of Spanish speech sounds (1887) in English. And so on.

Issues and controversies in child language

What were the main issues and controversies in the community of child language researchers? In 1927 Friedrich Richter considered this question in an excellent and detailed review and we will, by and large, follow his plan. It mentions, first, the issue of child language origins, second the staging of sound and meaning development and the explanations thereof, and third the relation of onto- and phylogenesis. I will add the important nineteenth-century topic of gesturing in the genesis of language.

Origins of child language

The millennia-old issue of whether language is acquired or innate, empiricism versus nativism, got a new twist during the last quarter of the nineteenth century. The accumulation of biographical studies invited an empirical, evidence-based approach. Although occasionally fierce battles were waged over the interpretation of the empirical data, it appears, from our historical distance, that there was more agreement than disagreement on these issues. The issue about the origins of the child's receptive language abilities was hardly ever raised. Discussions almost exclusively concentrated on children's early speech production and there was largely agreement on two points.

First, the phases of wailing and babbling during the first eight to ten months of life follow an innate process of maturation. Initial crying and wailing are reflexive, expressive responses to inner states caused by hunger, pain, or whatever. The expressive response is not intentional, but it is a purely physiological discharge. Tracy "has no hesitation in saying that the child's earliest vocal utterances are not acquired by imitation at all, either of sound or of movement, but that they are purely impulsive in their character. They are the result of a simple overflow of motor energy."[41] There is as yet "no relation of the expressive processes to acoustic impressions," Preyer writes.[42] This is different for the babbling phase. Babbling, the playful production, reduplication, and concatenation of ever more and more complex, but meaningless syllables is based on the "newly established connection between the impressive (acoustic) and expressive (emotional-speech) fibres."[43] But babbling is, initially, still entirely self-generated. It is the innate auditory–articulatory loop at work. It generates a much greater variety of sound patterns than will be used at later stages.

Second, there was considerable agreement on the dominant role of imitation in the acquisition of words in both their form and meaning. In fact, as Preyer and most of his colleagues argued, children begin imitating words that they do not understand, and start understanding words that they cannot yet imitate, but they all acquire from the environment many meaningful words before they start speaking. During the second year, imitation is the main motor of vocabulary development. There is, of course, the issue of whether imitation is an instinct but, whatever its origin, it provides the child with the drive and ability to acquire the language of the environment.

What were the battles about? A recurring theme is whether children invent language and whether they invent words in particular. Herder's position had been that they do: "Parents never teach children language, without the children always co-inventing it."[44] Herder's views are long forgotten, but Darwin was also convinced that children do invent words. His son created the word *mum*, which meant "give me food." Taine had, similarly, observed his son inventing the term *tem*, "a natural vocal gesture" for roughly *give, take, see*.[45] Romanes concurred. Citing Horatio Hale's report and other equally dubious evidence, he concluded "that the to all appearances arbitrary word-making, which is more or less observable in all children when first beginning to speak, may, under favorable

[41] Tracy and Stimpfl (1908 edition, p. 154).

[42] "Auch hier noch kein Zusammenhang der expressiven Vorgänge mit akustischen Eindrücken" (Preyer 1882, p. 259).

[43] "das erste Zeichen der eben hergestellten Verbindung von impressiven (akustischen) und expressiven (emotionell-sprachlichen) Bahnen" (ibid. p. 260).

[44] "Eltern lehren die Kinder nie Sprache, ohne daß diese nicht immer selbst mit erfänden" Herder (1772, p. 53).

[45] Taine (1877, p. 23).

circumstances, proceed to an astonishing degree of fullness and efficiency."[46] Carter Moore lists a few inventions of her son (such as *bizz* for his shadow) and so does Stevenson (1893). But Preyer and most of his followers disagreed. There may be the occasional spontaneously produced onomatopoeia, but "no word formation from interjections without imitation has been observed,"[47] and Preyer "deconstructs" some of the examples Steinthal, Kussmaul, and Taine had given. So-called new formations are just distorted imitations. Taine's *tem* example, for instance, would probably be an imitation of the often-used word *tiens*. Wundt followed Preyer and summarized: "After all, the language of the child is the product of the child's environment, in which the child essentially only participates passively."[48] But then, what is invention? For the patent office, Carl Stumpf[49] remarked, an invention must have a purpose and an effective means of realizing it. That may be asking too much of the child's word invention, but there is certainly some spontaneous production in the child's speech, which is by no means purely passive. As Compayré had argued, this innate spontaneity is even the "enemy of grammar" in the child's early syntax. It overruns or blocks the child's correct imitation of adult speech. It rather seems to reflect the (universal) syntax of gesture.

Sound development

Almost all diaries reported the order of the acquisition of speech sounds. It is the development of "outer speech," as it was still often referred to, in opposition to the development of meaning, "inner speech." There was, as mentioned, rather general agreement on the successive emergence of wailing and babbling sounds, vowels dominating in the former, syllabification in the latter. It was also widely observed that babbling in the second phase involves almost any consonant—labial, dental, glottal or otherwise—and any vowel, whatever the child's linguistic environment. As Preyer remarks, the child already produces almost all of the speech sounds of the target language and "aimlessly" many others that do not appear in any "civilized language" (*Kultursprache*).[50] Although the timing of these phases and, in particular, the appearance of first articulated meaningful words differs widely among the observed children, the ordering of phases in both inner and outer speech is uniform over children, or at least that is what Schultze suggested was so for outer speech. Controversy arose about the sound structure of early word formations. It was triggered by Schultze's "law of least effort." This says that the "speech sounds in the child's mouth are produced in an order, which goes from sounds that are produced with least physiological effort to those requiring more, and ending with speech sounds that are

[46] Romanes (1889, p. 144).

[47] "Die Wortbildung aus Interjektionen ohne Nachahmung ist nicht beobachtet" (Preyer 1882, p. 278).

[48] "Nach allem dem ist die kindliche Sprache ein Erzeugniss der Umgebung des Kindes, an dem das Kind selbst wesentlich nur passiv mitwirkt" (Wundt 1900a, Vol. I, p. 296).

[49] Stumpf (1901, p. 422).

[50] Preyer (1896, p. 266).

Table 4.1 Consonants arranged according to increasing effort in two dimensions, place, and manner of articulation (Schulze, 1880, p. 36)

	Plosives	**Resonants**	**Fricatives**	**Trills**
Lips	*p, b*	*m*	*f, (v), w*	*r* (labial)
Tongue	*t, d*	*n*	*l, s, sch*	*r* (lingual)
Palate	*k, g*	*ng*	*ch, j*	*r* (palatal)

produced with the greatest physiological effort."[51] In a careful phonetic analysis, Schultze derived what the order of effort should be. For the (German) vowels, and following Helmholtz's analysis of the vowel space, Schultze derived the following order of difficulty: *ä, a, u, o, ee, i,* followed by the diphtongs *ei, au,* and *eu/aü*. He arranged the consonants in Table 4.1.[52] The rows denote the place of articulation, lip-, tongue-, and palate sounds. The columns denote the manner of articulation: plosives, resonants, fricatives, and trills.

Difficulty increases from lips to tongue to palate. For the lip and tongue consonants it also increases from plosives to resonants to trills. For the palatals, however, the order is reversed, from trills to plosives. Combining all this, the physiological effort of producing the consonants increases in this order: first to come are *p, b, m, f, w, t*[53], *d,* and *n*. These are followed by *l* and *s*. Then come *ch* and *j*, followed by *sch*, labial *r*, and finally *k, g* and *ng*.

Schultze could only use Sigismund's and his own data and warned that this ordering is not a "dogmatic certainty." More data, also from other languages, are required to substantiate it. In a further "law" Schultze predicted that in case a child could not produce a particular speech sound, it would be replaced by the most closely related, but least strenuous one.[54] For instance, *b* would replace *m*. If the closest alternative was also too difficult, the next in line would be used, for instance *b* for *w*. Or the hard consonant may be dropped altogether (*Grossmamma* becoming *omamma*). Schultze also mentioned a further determinant of consonant replacements: assimilation. Children replace later consonants in a word by earlier ones, such as *Topf* becoming *tot*, i.e., progressive assimilation.

Here was a testable theory. How did it fare? It obviously did not hold for the babbling phase, in which just about any speech sound was produced, as Preyer, Lindner, Wundt, and others had observed. But was it true for the early word formation phase? Interestingly, as many observed, sounds that had appeared fluently in the babbling phase could no

[51] "... daß die Sprachlaute im Kindermunde in einer Reihe hervorgebracht werden, die von den mit der geringsten physiologischen Anstrengung zustande kommenden Lauten allmählich übergeht zu den mit größerer, und endet bei der mit größter physiologischer Anstrengung zu Stande gebrachten Sprachlauten" (Schultze (1880), p. 34). The same idea had been expressed much earlier by Friedrich Schwarz (1829), pp. 214–222, who used Kempelen's theory of speech sound production (realized in his 1791 talking machine) to derive an order of difficulty.

[52] Ibid. p. 36.

[53] Actually, Schultze forgot to insert *t*.

[54] Ibid. p. 38.

longer be produced. They were left out entirely, or replaced by other speech sounds. Were they the "strenuous" ones? That was indeed Tracy's idea when he derived from his tables the order from hard to easy: *r, l, th, v, sh, y, g, ch, s, j, e, f, t, n, ng, d, k, o, w, a, h, m, p, b.* Palatal and glottal sounds are harder to acquire than dental and labial ones. There are differences in detail, though: *f, t* and *l* are harder, whereas *k* and *ng* were easier for these English-speaking children than Schultze predicted. Carter Moore confirmed the value of Tracy's principle, but denied that it was the *only* principle at work. Misperception of certain sounds in the context of the whole word was another factor. Gutzmann was sympathetic to Schultze's law, but added that the production of frontal and, in particular, labial sounds was much more visible to the child. The child looks at the speaker's mouth rather than the eyes, according to Gutzmann.[55] That contradicted Schultze's own claim that "the child when it listens to what is said focuses not on the mouth, but on the eye and the facial expressions of the speaker."[56]

Wundt[57] agreed with Gutzmann and stressed in particular the importance of Schultze's other factor governing replacements, that of assimilation. Children show much progressive assimilation, according to Wundt, replacing a word like *Nase* by *Nana* or *Gasse* by *Gack*. In the latter case an "easy" dental sound *s* is replaced by a "hard" glottal sound *k*. Children tend to perseverate. Wundt also suggested that the child's auditory recognition of speech sounds is not yet fully developed at this stage; the child often does not have the right sound images for the words to be produced.

Preyer, finally, provided another reason for Schultze's law being of limited applicability. The frequency of occurrence for different speech sounds varies markedly among languages. Already in the babbling phase the child begins to pick up the more frequent sounds and incorporates them into his babbling. This will affect the order of acquisition.[58]

Inner speech development

The evolutionist Romanes theorized that there was a stepwise development of intentional expression in the child, which was to replace Max Müller's miraculous "crossing of the Rubicon," where the child suddenly acquired words for abstract concepts and predication. Romanes argued that these successive modes of expression in the child are not specifically human, except for the ultimate one.

First is the *indicative* stage. The child, or the animal, produces signs to express emotion or desire. This can be done by either gesture or voice. In the next stage signs are used *denotatively,* to indicate a particular object, quality or action. This is not yet naming, because the word does not yet generalize to similar objects, etc. From this develops the *connotative* stage. If the child initially indicated its father by *papa,* it may then extend the

[55] Gutzmann (1894, p. 201).

[56] "daß das Kind, während es auf die Anrede horcht, nicht den Mund, sondern das Auge und die Mienen des Sprechenden betrachte" (Schultze (1880), p. 33).

[57] Wundt (1900a, Vol. I, pp. 298–299).

[58] Preyer (1882/1989, p. 349).

word's use to other men. In that case *papa* has become a sign for an attribute, shared by some set of people. This is an act of classification. Such extensions can make almost random walks. Romanes relates a personal communication that Darwin gave him: the child first used *quack* to refer to a duck. This then became extended to all birds and insects and to all fluid substances. Finally also coins became *quack*, because the child had seen an eagle on a French sou.[59] Examples such as this show that denotational usage is *receptual*, as Romanes called it. The child is not considering the concept that the word denotes, but is subject to analogue impressions, which make the use of the word *generic*, but not *general* or abstract. There are animals that share this denotative ability, according to Romanes, who makes (quite uncritical)[60] reference to numerous reports on talking parrots. This stage can, in the child, gradually develop into a truly *denominative* stage of reference. Here the child has the intentional *conceptual* use of a sign. There is "introspective appreciation" of the term's function. It requires a certain degree of self-consciousness, which is not or hardly ever reached by animals. That makes it eventually possible to predicate, intentionally using one denomination to connote something of another one. This ability develops slowly in the child. What may look as a predication may initially be hardly more than an association. When the two-year-old child says *dit ki* (*sister crying*), it only gives expression to the association of two denotations, according to Romanes.[61]

Romanes's functional evolutionary approach to the development of expression in children inspired many psychologists. Compayré, for instance, adopted Romanes' stage approach in his influential, often re-issued, text on the child's intellectual development. Max Müller's rubicon notion may be expressive, according to Compayré, "but, as far as the child is concerned, language is not a single Rubicon to be traversed, it is rather a multitude of little rivers to be crossed, one after another, with subsequent little jumps, before reaching the promised land."[62]

Although this more functional approach was quite an improvement over the Herbartian idea, that the child learns to associate words to images, there was a further idea in the literature, which only fully took shape in Wundt's theory (see Chapter 6) and the application thereof by child psychologist Meumann (1902, 1903). This was the idea that the child's words initially have *volitional*, rather than referential, use. This had on and off been noted by educational theorists, amongst whom was Friedrich Schwarz, who noted this as early as 1829. The child's first words, according to Schwarz, express its inner response to the exciting external object, a response of "sensation or desire" or both at the

[59] Romanes (1889, p. 283).

[60] Many examples on pages 130 and 189–190, such as one saying "Mother come back to little Cockie: Mother come back to little cockie. Come and love me and give me pretty kiss. Nobody pity poor Cockie. The boy beat poor Cockie" (ibid. p. 190).

[61] Ibid. p. 227.

[62] Compayré (1893/1913, p. 254).

same time.[63] Sigismund considered the first speech of the child to be an "expression of will": "The proto-language is nothing but a will made audible."[64] Compayré speaks of the "expression of desire" in the child's first gestures and words.[65] Meumann put it this way: "The child initially does not indicate any objects or events in the environment, but rather the emotional or volitional aspect of these things."[66] The word *tul* (*Stuhl*, "chair") does not mean *that's a chair*, but *I want to go on the chair.*

These different views on the meanings of a child's first words all co-existed to the end of the century. While Meumann was developing his volitional theory, Ament produced a detailed account of the types of concept expressed in the first 200 words of his daughter Louise. They were "naming concepts" for objects, properties, and states. Or they were "relational concepts," mostly time, place, and manner adverbials. In extensive tables Ament listed how the child's first words would shift in meaning over time, and how their "part of speech" status would evolve. The process is depicted in graphs and fomulae. They demonstrate, among other things, that Louise shows both characteristic processes of meaning change: the widening and the restriction of meaning. The word *mmammamm*, which first meant *bread*, also was used for other kinds of food. Alternatively, the word *babab*, which was initially used for any person with a deep voice, slowly became the name of a single person (daddy). The graphs showed that the more concepts acquired by the child, the less widening and the more narrowing of meaning that occurred. Ament's explanation was that first words are connected with undifferentiated ideas, which he called "proto-concepts." The seeming generalization in subsequent uses of the word was due to lack of specific knowledge. Louise would use the word *Baum* for any tree, not because she had mastered the abstract concept of "tree," but rather because she could not yet distinguish different kinds of tree. Romanes had called this *generic* (as opposed to *general*) use. As finer distinctions are made by the child, more words are coined and they become more restricted in meaning. Ament goes all out to relate these developments to what is observed in "natural peoples," ontogenesis reflecting phylogenesis.

On the other hand, there was broad agreement that the child's first words function in a sentence-like fashion, that they are one-word sentences, somewhat like commands or requests. As Dewey and others had observed, it is in that stage impossible to categorize them as nouns, verbs, or whatever part of speech. Part of speech categorization only becomes possible when multiword syntax emerges.[67] Here also, the child was often compared with "natural man" in some stage of development. In the child's first multiword sentences, there is no inflection whatsoever. The child speaks an isolating language, such as Chinese. Words are uttered in some "natural order" also observed in gestural languages.

[63] Schwarz (1829, p. 299).

[64] "Die Ursprache ist nichts als ein vernehmlich gemachter Wille" Sigismund (1856, p. 74).

[65] Compayré (1893/1913, p. 230).

[66] "Das Kind bezeichnet anfangs überhaupt keine Gegenstände oder Vorgänge der Umgebung, sondern die emotionale oder volitionale Seite dieser Gegenstände" Meumann (1902, p. 182).

[67] Carter Moore (1896), however, provides part-of-speech tables for both.

An important process in the child's acquisition of inflection and other morphology is *analogy*. It is, according to Lindner,[68] governed by a "simplification drive": The child uses weak inflection in all cases, saying *goed* and *eated*. But luckily, there is also a "differentiation drive," moving the child to mark case, tense, and other things.

Ontogenesis recapitulating phylogenesis

Haeckel's (1874) claim that "the mental development of every child is only a short recapitulation of that long phylogenetic process" became a source of much inspiration and some controversy among students of language acquisition. Most striking was the general approval of this phylogenetic law. Taine put it this way in 1877: "Speaking generally, the child presents in a passing state the mental characteristics that are found in a fixed state in primitive civilisations, very much as the human embryo presents in a passing state the physical characteristics that are found in a fixed state in the classes of inferior animals."[69] Following Taine, almost all major scholars, such as Schulze, Perez, Preyer, Gutzmann, Sully, and Ament, expressed their implicit or explicit agreement with the notion that language acquisition in the child is, to use an anachronism, like running an accelerated film of language evolution in *homo sapiens*. They saw parallels between the child's predilection for easy speech sounds and the speech sound repertoire of primitive languages. Like "natural peoples," children replace fricatives by plosives, and like "natural peoples," they reduplicate a lot. Typically childish speech sounds, such as click sounds, can be observed in languages of uncivilized societies, according to Gutzmann.[70] Others tried to apply Grimm's laws to children's early speech. Children, like savage man or uncivilized man were also thought to have very limited vocabularies. Archdeacon Farrer's estimate of vocabulary size in English laborers is widely cited (e.g., by Romanes[71]): it does not exceed 100 words. Laurie (1890) estimates the vocabulary of children "up to the eighth year" at "not more than 150 words," which Salisbury (1894) corrects to 1528 for a 5;6-old child that he tested. Horatio Hale, in a personal letter to Chamberlain (1893), reports the language of the Iroquois, and the children learning "the few words of which it is composed."[72] These underestimates quickly faded during the 1890s, but in general the consensus was, for both children and uncivilized man, that words are concrete and that there is a lack of general concepts. This kind of talk was almost standard. There was complete conflation of "primeval man" and "primitive man," and the latter was conceived from the colonial and class perspective which was so dominant in fin-de-siècle culture and anthropology. The study of children's language had become detailed and reasonably critical, but one was free to make any unchecked conjectures about the language of

[68] "Vereinfachungstrieb," "Differenzierungstrieb." Lindner (1882, p. 339).

[69] Taine (1877, p. 259).

[70] Gutzmann (1897, p. 432).

[71] Romanes (1889, p. 280).

[72] "... les quelques mots dont il compose" (Chamberlain 1893, p. 321). Chamberlain (1890) listed "child words" in Abbé Cuoq's dictionary of Algonkin.

"savages." That is how Haeckel's law could survive for over a quarter-century. However, there was also some discussion that was slightly more sophisticated.

Romanes's major 1889 treatise was almost nothing more than an elaboration and qualification of the phylogenetic law: "To use biological terms, this the earliest evolution of speech has been greatly foreshortened in the ontogeny of mankind, as compared with what it appears to have been in phylogeny" (p. 327). But why is it that the child can run the film so much faster than humankind? Here, Romanes accepted Max Müller's criticism of the phylogenetic law. "If we watch the process by which children begin to speak and think, we see that they begin at once with ready-made words," Müller had said. Meaningful words are offered to them immediately. Not so for "the early framers of language," who had, "by their own mental effort" to come into "the possession of a name which was significant of one attribute."[73] Romanes agreed: "how vastly different must have been the case of primitive man!"[74] This was, according to Romanes, why the ages-long gestural stage in language evolution becomes so contracted in the infant's communicative development. What was offered to "infantile" man as "gesture and grimace," the modern child receives as a ready-made grammar of speech. In conclusion, "there is here no exact parallel between ontogenesis and phylogenesis."[75] Compayré, though strongly influenced by Romanes, rejected his idea that the child is a great inventor of language. The child's language grows through imitation, indeed imitation of the ready-made language offered to him. Wundt took the same position and reversed the argument. If child language can tell us anything about the origins of language it is that we apparently instinctively adapt to our interlocutors and have an early preference for using gestures. With that, Wundt says, the biogenetic law is "disposed of."[76]

Gestures and gesture languages

If the diary hype showed anything, it was that the infant's first expressive movements are gestures. Tiedemann had noted it, Darwin noted it almost a century later, and all others followed. Schultze distinguished between a limb gesture and a sound gesture language in infants. The former uses mime and gesticulation, the latter voice modulation, ranging from plain crying to a variety of tone modulations. Both quickly become intentional. These kinds of gesturing precede speech; the first words usually accompany gestures, in particular pointing gestures. Preyer reviewed various kinds of early limb movements and their uses: demonstrative pointing and posture gestures, head shaking, gaze directing, finger gestures, and in particular grip-mimicking "please" gestures.[77] Such sign-making is, according to Sully, unlearned. Gestures such as "stretching out the arms when the child desires to be taken by the nurse" are used instinctively. Also "the gesture of pointing,

[73] M. Müller (1887, pp. 519, 522).

[74] Romanes (1889, p. 330).

[75] Ibid. p. 332.

[76] Wundt (1900a, Vol. I, p. 296).

[77] Preyer (1882/1989, p. 120, 183).

though no doubt commonly aided by example, is probably capable of being reached instinctively as an outgrowth from the grasping movement."[78] Romanes proposed the interesting idea that "grammar is the child of gesture,"[79] in the following sense. Initially the child's words are one-word sentences. However, the simultaneous use of such a sentence-word and a gesture is a first shimmering of grammar. The gesture, for instance a pointing gesture, acts as a pronoun; it particularizes the meaning of the sentence-word.

Charles-Michel de l'Épée and Joseph-Marie Degérando

All this was not controversial, but considering gesture language also began to play another, theoretical role. The argument, which was quite generally accepted, was as follows. There happens to be a universal, natural grammar. It is the grammar or logic that underlies sign language. It emerges naturally in people born deaf who cannot speak, in the infant, in languages of the savage, and presumably in primeval man. These ideas were not at all new. In fact, they had been expressed in a much more sophisticated way by French Enlightenment philosophers during the decades surrounding the French revolution. The trigger had been l'Abbé Charles-Michel de l'Épée's (1712–1789) founding of the Paris Institute for the Deaf in 1754, mentioned in Chapter 2. The learned abbé began educating his pupils with a sign language based on their own natural gestures and adding whatever he could think of to enrich and conventionalize the system for the deaf community involved with the Institute. From there the pupils were to acquire written French as a second language. The often remarkable results of de l'Épée and his successor l'Abbé Roch Sicard (1742–1822), who led the Institute for another 30 years, struck the public imagination and not only in France. Kempelen, we saw, was convinced of the fully fledged conventional potency of sign language, and so was his fellow Viennese, Abbé Storch, who founded the first Institute for the Deaf in Austria.

Sign language was generally not only recognized as a potentially fully fledged means of human communication, it also became idealized as a language that was clearer, more distinct, and less confused than spoken language. Deaf people became the linguistic *bons sauvages* of revolutionary philosophy. This story has been told in great detail by excellent historians of sign language, such as Harlan Lane and Sophia Rosenfeld.[80] It would defeat the purpose of this chapter to present this fascinating history here. Rather, I will turn to one exceptionally lucid treatise of sign language, published in 1827 by Joseph-Marie Degérando. (1772–1842, Fig. 4.4). As an esteemed member of the *Institut de France*, a philosopher, and anthropologist,[81] Degérando had been assigned the task of evaluating French education of deaf people after Sicard's death in 1822. The outcome was a huge two-volume treatise on all aspects of sign language, in the larger framework

[78] Sully (1895, p. 139).

[79] Romanes (1889, p. 297).

[80] See, for instance, Lane (1984) and Rosenfeld (2005).

[81] Degérando composed an anthropological field guide for the French Baudin expedition to Australian waters (1800–1803), which was in use for over a century. Degérando Island was named after him.

Fig. 4.4 Joseph-Marie Degérando.

of the Enlightenment philosophy of language. We will consider this in some detail and then return to our students of child language and their views on sign language, noticing how much had been lost in the course of just half a century since Degérando.

Here is a critical extract from Degérando's introductory chapter: "Indeed, the deaf-mutes, assembled among them, institute a language of signs, a true conventional language, but founded on analogy, and which, developed by time and reflection, as it would be perfected in a people of deaf-mutes, will be susceptible of acquiring all the richness of our [spoken] languages."[82] The next chapter describes how normal children acquire language. The child is intensively monitoring its environment. Initially he cannot understand the words spoken to him, but he can understand the natural signs of facial expression, tone of voice, gaze, and touch (representing joy, sadness, aversion, etc.) and develops its own first language of signs. It will then attend to the effects of these first intentional signs, be they gestures or words. Early words are seemingly of general meaning, "papa" standing for all men. But actually their meaning is just vague, undetermined (like Romanes's "generic"). How does the child acquire words, word order, and phrases? When the child listens to a new phrase, some of the words are already known to him, but the phrase may contain a new word, which appears "a thousand times" in different phrases. Conversely, the same phrase keeps being repeated with only a few words changed. This combination of repetition and variation eventually makes it possible for the child to "instruct itself" on the interpretation of words and phrases.[83] In another chapter Degérando sketches a convincing phenomenology of the deaf child in a loving hearing family; the child

[82] "En effet, les sourds-muets, réunis entre eux, instituent un langage de signes, un vrai langage conventionnel, quoique fondé sur l'analogie, et qui, élaboré par le temps et la réflexion, comme il pourrait être perfectionné chez un peuple de sourds-muets, serait susceptible d'acquérir toute la richesse de nos langues" Degérando (1827, p. 18).

[83] pp. 53–54. This is somewhat like reading Tomasello (2003).

enterprisingly proposes signs for everything, inventing his own simple sign language and "teaching" it to its environment: "this language is susceptible to undergoing indefinite development."[84] When such children get together to form a community, "each bringing along his or her contribution to the common crowd, there will naturally be formed, for their use, a sufficiently abundant language, and that language will be growing, day by day, as their community grows closer."[85]

Degérando distinguished three steps in the development of any language, but in particular sign languages. There are, at first, just natural signs, such as crying or fleeing. These signs are not intentional, but they can nevertheless be understood. There is next a stage of creating "analogical" signs. These have a transparent meaning. In sign language they are the first indicative signs (to refer to something in the environment) and "recall" (*rappel*) signs in memory. But this stage quickly turns into the final stage of a conventional language with arbitrary signs. During the stage of analogy, signs get simplified quickly, losing their transparency. Also new signs are intentionally introduced and conventionalized for various purposes. As a consequence the language of the third phase is, to some extent, "artificial," as is any spoken language. One can "elaborate it in thousand ways; because it makes itself available for all one demands. Such are our languages."[86]

The Abbé de l'Épée had begun his enterprise when he met two young deaf sisters who had developed their own signing system. Degérando developed from this case a thought experiment on how a (sign) language would emerge when two persons without language come together. They "will soon *understand one another*, because each of them *understands himself*. Hence, the similarity of their actions furnishes to both the natural means to apply to his like what he knows about himself. It is like a mirror in which he recognizes himself[87] *Understanding* someone, is judging about him after oneself. We believe we can read each other's minds, but we read in our own interior, and we ascribe to the other what we would think in his place." To put it in modern terms, Degérando puts our theory-of-mind capacity at the core of our language ability. Here man differs from animals: "they don't understand one another, because none of them understands itself." Their "faculty of imitation does develop, but it is entirely blind imitation."[88] This is quite different for people: "Actions, being the consequences of their motives, naturally become, among

[84] "... cette language est susceptible de recevoir un développement indéfini" (Degérando 1827, p. 81).

[85] "Si les sourds-muets se réunissaient entre eux pour former une societé, chacun apportant son tribut à la masse commune, il se formerait naturellement une langue assez abondante, et cet langue croissant de jour en jour, à mesure que leur société se perfectionne" (ibid. p. 83).

[86] "... on l'élaborera en mille manières; car il se prête à tout ce qu'on lui demande. Telles sont nos langues" (ibid. p. 172).

[87] Degérando regrettably missed the discovery of mirror neurons. But the idea that understanding an action is like performing it ourselves also led Rizzolatti and Arbib (1998) to put that reflective capacity, rightly or wrongly, at the origin of language.

[88] "Mais ils se ne comprennent point les uns les autres; car chacun d'eux ne se comprend point lui-même." "Chez les animaux apprivoisés, la faculté de imitation se développe: mais c'est une imitation entièrement aveugle" Degérando (1827, p. 166).

people, signs of these motives: for them they compose a first, still unreasoned language."[89] And for the ones observing such natural signs, for instance fleeing from some danger, recognizing the motives of actions can "become a new motive, suggesting a new action; he will defend someone who is threatened, assists someone who is wanting. In an analogous situation, he is, in his turn, understood, protected, helped."[90] Following the mirror analogy, it should be expected that the experience of having understood someone will naturally also be adduced to the other person: "I understand and I am understood." This invites further acts of mutual understanding and "the language, which was first involuntary, becomes reasoned."[91] Then follow the further stages of language development. Degérando applied the same principles to a Frenchman and an Arab who meet as new interlocutors. Developing their pidgin is equally guided by the motive to understand and be understood. First, natural signs will be conventionalized between them. But they will proceed much faster because they already have an abundance of ideas and enormous experience in linguistic communication.

Turning now to the child, Degérando applied the same principles. Here are two new interlocutors, the newborn infant and the mother or caregiver. The first move in the child's acquisition of language is, as in the other cases, to recognize motives of actions. That will work first for actions such as smiling, caressing, and presenting objects. Here, the caregiver adopts the child's repertoire of actions. The child, being speechless, is in the position of Degérando's first couple. But the carer, being a fluent speaker, is rather more in the position of the Arab and the Frenchman. Hence, the situation is asymmetrical. The first words offered by the mother do not reveal their motives to the child. It is like a contract, where the carer proposes a sign, which may become shared if the motive can be discerned by the child.[92]

Degérando's treatise was experience-based. French sign language had been developed, used, and studied in quite favorable circumstances. It was considered to be a natural language with maybe even special affordances for clarity and explicitness. But, as a philosopher of language, Degérando was among the last of his generation. His treatise was completely lost on the end-of-century psychologists, even on the French authors. Perez and Compayré never cited him. Romanes, who discussed many of the same issues, apparently never saw his work.

[89] "Les actions, étant les conséquences et les effets de leurs motifs, deviennent naturellement, entre les hommes, les signes de ces motifs: elles composent ainsi, pour eux, un premier langage encore irréfléchi" (Degérando 1827, p. 166).

[90] "Il a *compris*, et cette découverte devient un motif nouveau qui lui suggère une nouvelle action; il défend celui qui est menacé, assiste celui qui désire. Dans une situation analogue, il est également compris, protégé, aidé à son tour" (ibid. pp. 168–169). For the relation between the infant's theory-of-mind and helpfulness, see Warneken and Tomasello (2006).

[91] "*Je comprends et je suis compris,*" "et le langage, qui d'abord était involuntaire, deviendra réfléchi." (Degérando 1827, p. 169).

[92] Similarities to Clark's theory of "proposals" are obvious (Clark 1996).

The demise of Deaf sign language

The work of Degérando's contemporaries, the brothers Charles and Rembt Guyot from Groningen in the Netherlands, met with the same fate. Their father Henri Guyot established, in 1790, a school for deaf people in Groningen, following the principles of de l'Epée, whom he had visited in Paris. In 1824, two years after Degérando's treatise, the two sons published, in Dutch, a bibliography of literature relevant to education of deaf people. In 1842 they published an augmented edition of this bibliography, but now in French, "a language for the civilized world." It was a service to their *confrères* in Deaf education. Degérando's work is extensively referenced in this work. The book's text on Deaf education leaves no doubt that it should be based on signing, but then extended to training the spoken language in both its written and oral modalities. This was, mid-century, the dominant, although not exclusive view. In France, the Pereire banker family were staunch supporters of the oral method, in opposition to de l'Epée and his followers. In Germany Samuel Heineke (1727–1789) had been developing an "oral method", which became the dominant practice in German Deaf education; see, for instance, Schmalz (1848).

The historical break with de l'Épée's tradition took place in 1880. Banker Eugene Pereire, together with the French and Italian oralist teacher communities (mostly consisting of Roman Catholic priests and brothers), staged a coup in organizing the Milan *Second International Congress of Teachers of Deaf-Mutes*. Participants and delegates had been carefully selected to be oralists in the very large majority. Eight resolutions were tabled for vote by the delegates. Resolution 2 was:

> Considering that the simultaneous use of speech and signs has the disadvantage of injuring speech, lip-reading and precision of ideas,
>
> *Declares* –
>
> That the Pure Oral method ought to be preferred.

It received a vote of 150 to 15 in favor. The pressure on non-oralist teachers was high enough for some of them to "convert" in public. *The Times* reported the convincing evidence for the effectiveness of the "pure oral method" (i.e., without any support from signing). They had observed demonstrations of deaf pupils: "Deaf children were addressed just as if they were not deaf, in spoken language, and they one and all answered in spoken language, though in our country we call them dumb." The interactions with these, most probably non-native, deaf children, had been staged. The one formidable opponent during the Congress was Edward Gallaudet of the National Deaf-Mute College in Washington. In his own words, he was representing some 6000 American pupils, more than all other delegations together. But the coup worked. The "pure" oral method was almost universally adopted (at least officially), with a split American situation. The National Association of the Deaf took on its mission to save deaf language and culture.

With the oral method dominant, scientific expertise in Deaf sign language quickly declined. The oral approach fed on and was fed by a general conviction that sign language was inferior. In 1896 Moritz Friedeberger from Bern published a dissertation arguing for the oral method. It is a characteristic diatribe against the use of sign language. We know "that the so-called sign language has, in no phase of its use in different countries, worked

its way up to sentence construction of its own and formation of syntax." Sign language is "of primitive nature," "definitions and grammatical rules are against its nature."[93] It is a hopeless enterprise to further develop such a language. Why introduce deaf people to the (spoken) language of their environment, which they have to learn anyhow, via "the desert of gestures and auxiliary signs?" In a passage, too obnoxious to translate,[94] Friedeberger compared this with trying to cultivate the incomplete language of "savage Africans" when such a family would be transplanted to "our" regions. It would not help them to integrate.

Preyer was more careful and more practical. He went to the Institute for the Deaf in Weimar to convince himself that it was possible to think in sign language. In an appendix of his book he listed a large number of concepts and (roughly indicated) corresponding signs used by the deaf pupils. Many of these are quite abstract. "In truth it was not language, which generated reason, it is reason which once invented language, and nowadays, too, the newborn human being brings along into the world much more intelligence than talent for language." Here is Steinthal's ghost in the background.

Schultze remarked that, surprisingly, deaf children happen to invent quite similar signs, which makes it easy for them to communicate. In an institute for deaf people the differences are "polished away,"[95] and eventually there are some 5000 signs available for storytelling, praying, and what have you. But all signs remain very concrete and sensory. Teaching in the mother tongue, as done in Germany, is inescapable for a "real tendency to liberate and develop the mind of these stepchildren of nature"; the "French method" is insufficient.[96]

Romanes, who was more of a theoretician, did not exclude the possibility that "gesture-language admits of being developed to a degree which renders it a fair substitute for spoken language, where the ideas to be conveyed are not too abstract."[97] But the devil is in the word "abstract." Sign language is "the natural expression of the logic of recepts,"[98] not of concepts. And although there was, no doubt, some conventionalization in American-Indian sign languages (as described by Colonel Mallery), the gesture systems of the Deaf, of American-Indians, and of Neapolitan signers, were essentially the same. Romanes approvingly cited the Colonel: "the result of the studies, so far as presented is,

[93] "Dass die sogenannte Gebärdensprache in keiner Phase ihrer Handhabung in verschiedenen Ländern sich zu einer eigenen Satz-konstruktion und syntaktischen Ausbildung verstiegen hat, wie wir sie in modernen Sprachen finden.," "primitiver Natur," "Definitionen und grammatische Regeln sind ihrer Natur zuwider" (Friedenberger 1896, p. 42).

[94] Ibid. p. 51.

[95] "Die individuellen Differenzen werden in der Anstalt . . . vollends abgeschliffen zu einer allgemeine gültigen Geberdensprache." (Schultze 1880, p. 29).

[96] ". . . und deshalb in der deutschen Methode des Unterrichts der Taubstummen in der Wortsprache eine wirkliche Tendenz zur Erlösung, Befreiung und Entwicklung des Geistes dieser Stiefkinder der Natur liegt, gegenüber der französischen Methode" (ibid. p. 30).

[97] Romanes (1889, p. 113).

[98] Ibid. p. 111.

that that which is called the sign language of Indians is not, properly speaking, one language; but that it, and the gesture-systems of deaf-mutes, and of all peoples, constitute together one language – the gesture speech of mankind – of which each system is a dialect." (p. 111).

This view was widely shared. According to Schultze, tests had demonstrated that European, Oceanic, Chinese, and Lappish people born deaf and unable to speak can immediately have a lively conversation among themselves in sign language.[99] Kussmaul wrote: "Deaf-mutes also make themselves easily understood with savages. The pantomimes of savages and deaf-mutes behave like dialects of a protolanguage."[100] Even Wilhelm Wundt spoke of a "general gesture language,"[101] which was not only the cradle of language, but which can still be observed alive and kicking in deaf people and in children. "It seems to me highly suggestive," says Romanes, "that the earliest forms and records of spoken language that we possess ... follow so closely the model which is still supplied to us in the idiographic gestures of deaf-mutes. Such syntax as there is ... so nearly resembles the syntax of gesture language, that we can at once perceive their common psychological source." (1889, pp. 341–342). Also shared is the idea that sign language has no real grammar. According to Romanes, this is evident from the fact that it has no parts of speech, no copula, and no inflection. These features it shares with archaic, isolating languages, such as Chinese. He extensively cites Scott's transliterations of deaf signers,[102] which demonstrate that sign language is "the most natural or immediate mode of giving expression to the logic of recepts; it is the least symbolic or conventional phase of the sign-making faculty" (p. 120). Ideas are expressed in natural order, mostly chronological order or the order in which the ideas appear in mind. They are "presented in as concrete a form as possible," pronominal reference is spatially made and it is a fact "that no ideas of any high abstraction are ever expressed at all" (p. 120). "No sign-talker, with any amount of time at his disposal, could translate into the language of gesture a page of Kant" (p. 147). Gesturing in child language, according to general belief, follows the same natural logic and it continues into the early phases of speech. For the child to acquire the grammar of his language, it has to overcome this "tacit logic" as Compayré called it, which is "the enemy of grammar."

Retrospect

These were the major issues on the agenda of language acquisition research by the turn of the century; most of them were never entirely lost during the twentieth century. The study of child language had become an empirical enterprise and a community of dedicated researchers had been established. In spite of many differences of opinion, there existed no

[99] Schulze (1880, p. 30).

[100] "Auch Taubstumme verständigen sich leicht mit Wilden. Die Pantomimen der Wilden und Taubstummen verhalten sich wie Dialekte einer Ursprache" (Kussmaul 1877, p. 5, n. 1).

[101] Wundt (1900a, Vol. II, p. 607).

[102] James Scott Hutton (1833–1891) was a teacher of deaf people, and, from 1857, principal of Halifax Institution for the Deaf in Nova Scotia.

marked schools, factions, or dissident movements. The community was, as it were, on speaking terms. And the community was a truly international one, considering (mostly) diary acquisition data in many languages, among them Algonkin[103], Arabic[104], English, French, German, Hungarian,[105] Iroquois, Italian[106], Polish, Russian[107], Spanish, and sign languages. These languages were rarely studied comparatively, with the exception of sign language(s) versus spoken ones. The endeavor was to discover the general patterns in the acquisition of speech sounds, vocabulary, morphology, and syntax and to provide theoretical explanations.

Alas, this communality would not last. Two world wars, behaviorist dogmatism, and extreme pragmatism would soon split the research community. This breach would not be healed until the American "cognitive revolution."

[103] Chamberlain (1890).

[104] Sayce (1889).

[105] Balassa, J. (1893).

[106] Garbini (1892), Lombroso (1894).

[107] Sikorsky (1883), Blagoveščenskij (1886).

Chapter 5

Language in the laboratory and modeling microgenesis

Two Dutch eye doctors, *A* and *B*, put their heads, close together but separated by a partition, in front of a phonautograph (Fig. 5.1). *A* would loudly shout *ki!* and *B* would immediately respond by also shouting *ki!* The sound waves of both shouts were recorded on a turning cylinder, together with the sound wave produced by a 261 cps tuning fork (Fig. 5.2). By counting the number of tuning fork cycles from *A*'s *ki* onset to *B*'s *ki* onset, the reaction time from auditory stimulus to *B*'s vocal response could be determined. When *A* was Johan de Jaager and *B* was Franciscus Donders, the mean reaction time was 180 ms; when *A* was Donders and *B* de Jaager, it was 250 ms. This was the first psycholinguistic reaction-time experiment ever. It was simultaneously reported in 1865 by Donders himself and in de Jaager's Utrecht dissertation.[1]

This chapter will review the nineteenth-century beginnings of experimentation and research into speech error and the psychology of speech and language. I will first discuss experimental and phonetic approaches to the production and perception of verbal materials, both spoken and written. Then I will discuss the first experimental studies of verbal memory, association, and analogy. And last, but not least, I will cover the first process model of speaking based on the analysis of spontaneous speech errors.

Mental chronometry: Franciscus Donders

Importantly, the experiment by Donders and de Jaager involved a second condition. Subject *A* would shout any of six syllables, involving six different Dutch vowels: *ki*, *ko*, *ka*, *ke*, *ku*, *keu*, or *koe*. *B* would immediately respond with the same syllable. Now Donders' reaction time was 268 ms and de Jaager's 338 ms. These reaction times happened both to be 88 ms longer than the earlier simple *ki* reaction times. How does this come about? The experiment had been designed by Donders[2] to determine the speed of the psychological processes involved in forming a mental image of the stimulus and the following exercise of the will to perform a particular response. By subtracting the reaction time in the former *simple* condition— the a-condition—from the latter *choice* reaction time—the

[1] Donders (1865), de Jaager (1865).

[2] "The idea of these experiments and the methods used were provided to me by my respected Supervisor, who also participated himself directly in the experiments" ("Het denkbeeld tot deze proeven en de gevolgden methoden werden mij aan de hand gedaan door mijne geachte Promotor, die ook zelf aan de proeven direct deelnam."). De Jaager (1865, Preface). Donders (1868a, p. 95) cites this almost literally.

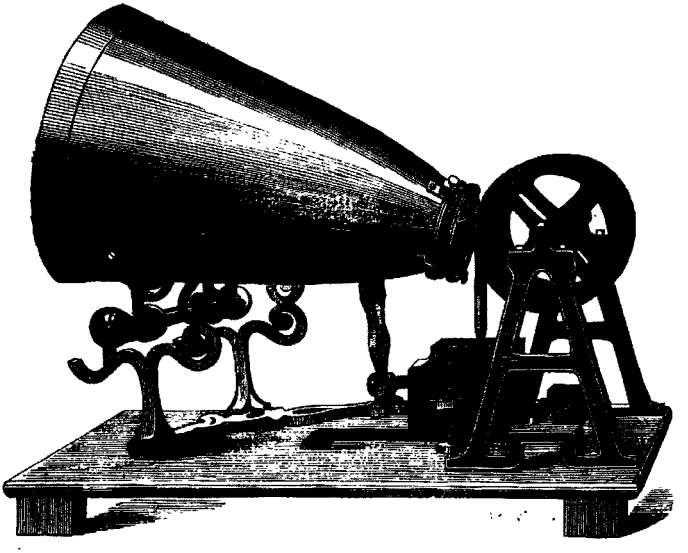


Fig. 5.1 The Koenig–Scott phonautograph, used by Donders.[3] The subject speaks into the open wide end of the paraboloid. This causes vibrations in the membrane stretched over the other, focal end of the instrument. A light spring mounted on the membrane transmits its vibrations to the soot-covered paper on the turning cylinder. Donders simultaneously recorded on the same cylinder the vibrations of a 261 cps tuning fork.

b-condition—the psychological duration could be determined exactly. In order to prepare the choice reaction, the subject had to discriminate the syllable presented from all other possible stimuli in the experiment; something that was not required in the simple condition (*ki-ki*). Also, the subject had to plan the right response: one out of seven, again something that was not required in the simple condition, where, as Donders put it, because there the subject "keeps the position of the mouth parts and the mechanism therefore in readiness, so as to only having to emit the breath upon recognizing [k]i, in order to produce the corresponding sound."[4]

Would it be possible to determine the durations of each of these two psychological processes separately, i.e. the syllable discrimination duration and the response choice duration? To achieve this, Donders (1868a) introduced a third experimental condition, which he called the c-condition, and which we will call the *discrimination* condition. Here different syllables were shouted, exactly as in the choice condition, but now the subject had to respond with *ki* when *ki* had been shouted, and *not* respond in all the other cases. In this condition, Donders reasoned, there was no choice to be made between overt

[3] From Pisko (1865, p. 72).

[4] "... en houdt de positie der monddeelen en het mechanisme daartoe gereed, zodat men bij het herkennen van i slechts de adem hoeft uit te stooten, om den correspondeerenden klank voort te brengen" (Donders 1868a, p. 109).

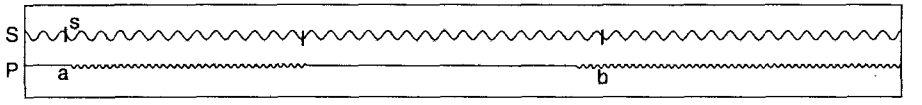


Fig. 5.2 Traces on Donders' rotating cylinder. S: trace of the 261 cps tuning fork. P: trace of voice vibrations recorded on the phonautograph (Fig. 5.1); *a* marks beginning of *A*'s shout, *b* the beginning of *B*'s shout. Reproduced from Donders, F.C. Over de snelheid van psychische processen. *Onderzoekingen gedaan in het Physiologisch Laboratorium der Utrechtsche Hoogeschool* (1868–1869), 2, 92–120 © BiblioBazaar, 1868 with permission.

responses: it could only be *ki*, as in the simple condition.[5] The subject could therefore have his mouth ready in the correct shape, but it would still be necessary to auditorily distinguish, or *discriminate*, *ki* from the other possible syllables. Because that was not necessary in the simple condition, the duration of the process of perceptual distinction could be estimated by subtracting the simple-condition reaction time from the new discrimination-condition reaction time. For subject Donders this difference was 37 ms in the new experiment. The difference between the choice and the simple condition turned out to be 83 ms (i.e. only 5 ms less than the figure Donders and de Jaager had measured three years earlier). Assuming that these 83 ms were the sum of the discrimination and choice-planning durations, the response choice planning must have taken $83 - 37 = 46$ ms.

Franciscus C. Donders (1818–1889, Fig. 5.3), born in Tilburg in the Netherlands, was trained as a military doctor.[6] In 1847 he became a professor of medicine at Utrecht University, increasingly specializing in ophthalmology. He founded, in 1858, a completely modern ophthalmological clinic in the university hospital, the leading such clinic in the Netherlands to the present day. There Donders supervised a stream of dissertations, most of them in ophthalmology.

De Jaager's dissertation on physiological time was somewhat of an exception. Still, it was not a surprising departure. Donders had been friends with Hermann von Helmholtz (1821–1894) in Heidelberg from at least the early 1850s.[7] Helmholtz was (among many other things) the inventor of the ophthalmoscope. For that reason, Helmholtz received a Dutch royal decoration[8] on the occasion of the 1858 opening of Donders' eye clinic in Utrecht. Helmholtz had also, in 1850, been the first to measure the speed of nerve transmission in the frog and, although his measures were less accurate, he also measured the equivalent figure in man. Helmholtz surmised in the latter paper that it would now become possible to determine the time "which expires in the brain to deliver the telegram received from the sensory nerves to the motor nerves." You would have to measure the time from an electrical skin pulse to the subject's hand response (total time) and subtract the

[5] Wundt (1883, p. 32) argued that, in reality, this is a choice between a response and a non-response. See also Wundt (1874, pp. 744–745).

[6] An excellent source on Donders and his measurement of mind is Draaisma (2002).

[7] They had mutual visits and a lifelong exchange of letters. Donders began writing in French, used German occasionally, but mainly corresponded in Dutch, which Helmholtz could apparently read.

[8] Ridder in de Orde van de Nederlandsche Leeuw.

Fig. 5.3 Franciscus C. Donders. © Collectie Regionaal Archief Tilburg.

computed conduction times for the sensory and motor nerves, both of known length. You would then subtract the (measurable) time from muscular innervation to contraction. What remained should be the brain transmission time. Here were the seeds for Donders' major discovery, the measurability of mental duration. Still, Donders' entirely new idea—of inserting an additional mental operation in the circuit and, by subtraction, determining its duration—was crucial. This subtraction method was hierarchical in nature. We saw that by subtracting the reaction time in the simple condition from the (longer) reaction time in the choice condition, an estimate was obtained of syllable distinction plus the time required for making a response choice. By subtracting the reaction time in the discrimination condition from the (longer) reaction time in the choice condition, the response-choice duration was estimated. Finally, by subtracting the latter duration from the former, an estimate was obtained for the duration of making the syllable distinction.

In his paper, Donders applied the complete simple–choice–discrimination paradigm not only to the spoken syllables we considered, but also to touch, light, and letter stimuli. The latter are also relevant here, because they are the very first reading-reaction-time experiments in the literature. In these studies, the subject saw a briefly presented letter and had to name it. The main result was that choice reaction time minus the simple reaction time was about twice as long as what had been observed for responding to spoken syllables. But choice reaction times minus discrimination reaction times were just about the same for both kinds of stimuli. Donders' conclusion was, of course, that distinguishing letters was much harder than distinguishing spoken syllables, whereas response preparation did not differ much in the two cases.

Donders' paradigm was quickly recognized as fundamental. Donders himself recognized that it was important; he took care that his Dutch 1868 paper appeared the same year in German and in French (Donders 1868b,c). Wundt (1874) treated it extensively in his *Physiological psychology*. It became, as Erdmann and Dodge (1898) put

it, "the foundation for all derivations of durations of composite reactions." The paradigm survives to the present day, with many useful modifications.[9] Mental chronometry became the dominant experimental technique in modern experimental psycholinguistics. Its initial use was largely limited to analyzing "the time course of the simplest psychological events,"[10] such as the detection or discrimination of simple visual, acoustic, or touch stimuli, but soon there were also applications to reading and picture naming, as we will discuss shortly.

Phonetics and Wolfgang von Kempelen's speaking machine

Donders also contributed to phonetics. Using the same phonautograph (Fig. 5.1), Donders recorded on the cylinder the sound traces of 14 Dutch vowels, sung at different pitches. A main conclusion from these recordings was that for vowels "their timbre is not determined by overtones with a specific relationship to the fundamental, but rather by overtones of almost absolute frequency,"[11] i.e., independent of the fundamental. They later became known as the vowel's formants.

The study of how vowels and consonants are generated is, however, much older. In fact, Donders' work on vowels fits a tradition in phonetics going back to the Flemish scholar Franciscus van Helmont (1614–1699), who had argued that Hebrew letters actually depict the relevant tongue positions for producing the corresponding speech sounds.[12] He thought this might be helpful for teaching speech to deaf people. Bishop John Wilkins (1668) carefully depicted the vocal tract positions for the articulation of 34 speech sounds. He also created a syllabary notation system.

But what really hit the imagination was Kempelen's (1791) construction of a speaking machine. Wolfgang von Kempelen (1734–1804) was a Hungarian scientist and engineer in the service of the Habsburg and Habsburg–Lorraine monarchy in Vienna. His work on the speaking mechanism began in 1769 and lasted for 22 years. Constructing the machine went hand in hand with developing a theory of speech sound production by the human voice. The larger part of Kempelen's extensive and beautifully written book discusses all components of the vocal tract from lungs to lips and their functions in the generation of speech. It then covers the production details of 12 vowels (*Selbstlaute*), with some remarks on diphthongs, and of 19 consonants (*Mitlaute*), with remarks on a host of consonant clusters.

[9] On the occasion of the 100th anniversary of Donders' 1868 publication, Saul Sternberg (1969) presented his "additive factor method," a sophisticated innovation of Donders' method, which found wide application in modern reaction-time research generally, as well as in psycholinguistics.

[10] This was the title of an important Donders-based paper by Kries and Auerbach (1877).

[11] "... dass ihre Klangfarbe nicht durch Obertöne einer bestimmten Ordnung zum Grundton, sondern vielmehr durch Obertöne von nahezu absoluter Schwingungszahl bedingt wird" Donders (1864, p. 528). Donders (1870) is a small didactical essay on the Dutch vowels and consonants.

[12] The only script that does is Korean.

The last chapter of his book describes the successive versions of his design. It is a fascinating story of trial and error, beginning with Kempelen strolling through a nearby village and seeing a group of country people dance to the sound of an instrument that sounded very much like a human voice. It turned out to be a set of bagpipes, which the musician was not willing to sell. After much negotiation Kempelen acquired a spare reed piece which, back home, he connected to his fireplace bellows and on the other side to the funnel piece of an oboe. With this contraption he produced his first acceptable *a*. The final design, dating to 20 years later, is shown in Figure 5.4.

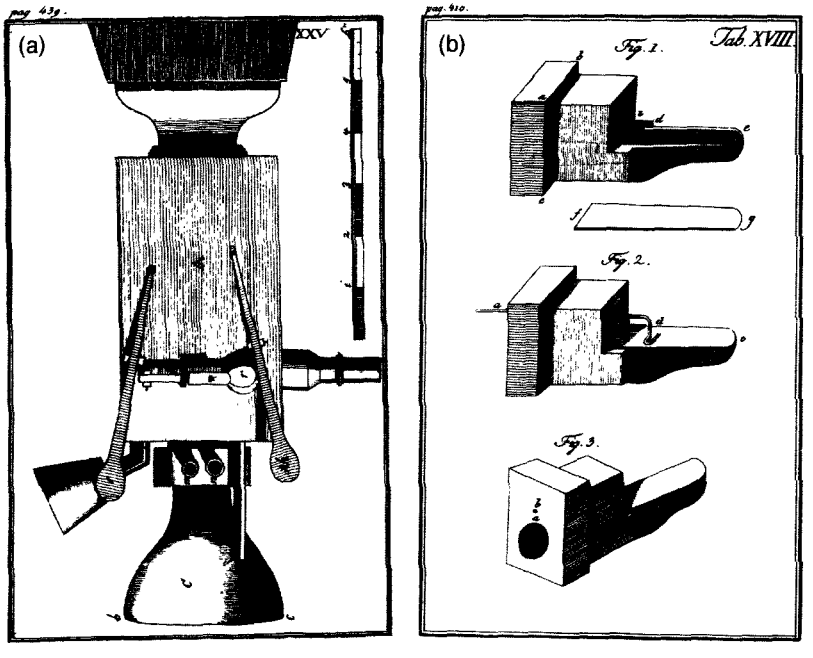


Fig. 5.4 Wolfgang Von Kempelen's speaking machine (1791). (a) When the bellow X (only partly displayed) is pressed, the air flows into the wind box A. From there it passes over the reed stop (see (b)), somewhat like a clarinet reed, but made of very thin ivory, setting it to vibrate. The reed stop resides in the opening connecting the wind box to bell C, the soft rubber resonator. Covering this "mouth" by hand produces a particular timbre. This can be varied by moving the hand closer or farther, make it hollow or flat, etc. Different vowels are produced doing this. Consonants are produced in several ways. An *f* results from closing all openings and pressing the bellow very hard. A *p* is produced by pressing the bellow, then suddenly removing the hand from the mouth piece. Here a by-pass pipe (in view) directly connecting the wind box and the "mouth" ensures that pressing the bellow will not push air through the reed stop; its vibration should not start before the opening of the "mouth." There are levers for producing *sh* and *s* (in view), which let the air flow out of the wind box through swishing channels (in view, sticking out to the left and the right). The nasals *m* and *n* are produced by opening either one of two resonators sticking out (to the front) from the "foot" of the mouth piece. And so on. A well-trained operator could produce whole stretches of speech, such as *Leopoldus secundus*. From Kempelen (1791).

Kempelen pioneered the speech-synthesis approach to understanding the production of speech, an approach which is still with us.[13] Speech machines were built by Joseph Faber of Vienna, Charles Wheatstone of London, and others. Helmholtz, Donders' friend, generated vowels by combinations of tuning forks, and the siren would soon open new possibilities (see Chapter 12). The final, most sophisticated equipment during the period covered by this book was the Haskins pattern playback machine, described in Chapter 15 (see Fig. 15.2).

Reading and naming

Hubert von Grashey

Carl Wernicke, as we saw in Chapter 3, had been much impressed by Grashey's (1885) paper. Hubert von Grashey (1839–1914) was director of the Würzburg psychiatric clinic when he published his influential paper. This presented the case of a patient, Mr. Voit, 27 years old, who, after a head injury, had largely lost the ability to name visually presented objects. But the patient had kept the ability to (slowly) *write* the names of the same objects. This provided him with a roundabout way of naming objects, namely by writing down the name of the object and then reading it off. To deal with the case, Grashey constructed a diagram, similar to Lichtheim's (the right-hand diagram in Fig. 3.8). However, in contrast to Lichtheim and Wernicke, he did not claim any disturbance of centers or connections for this patient. Instead, he suggested that the patient's problem was a short-term memory defect, i.e., the inability to maintain the image of an observed object or of a spoken or written word. According to Grashey, naming an object consists in sequentially activating the speech sounds of the corresponding word image. But the patient cannot maintain the image of the object and the activated speech sounds long enough to compose the whole motor image for the word. One solution for the patient is then to focus attention on the object, while rehearsing the activated speech sounds again and again, each time adding a further speech sound. Alternatively, and more simply, the subject can, letter by letter, activate the visual word image, writing down each letter as it comes. The patient can then read off the word. This is where Grashey's theory of reading comes in: the patient quickly scans the written word, letter by letter, immediately transforming each letter into the corresponding sound image. In this way, the natural sequential sound image is generated fast enough to trigger the corresponding motor commands. Grashey argued that this is the normal course of action in reading. As the gaze of the reader flows over the line, all letters are sequentially recognized as they pass the macula of the retina. One by one they activate the relevant speech sounds of the word image. In normal reading this happens at a speed of about 30 ms per letter. This became known as the "spelling theory" of reading.

[13] Dudley and Tarnoczy (1950) review this history from van Helmont on. See also Paget (1930).

Fig. 5.5 Young James McKeen Cattell, the first experimental psycholinguist. © Lafayette College Archives.

James McKeen Cattell

The spelling theory was short-lived. Robert Sommer (1864–1937) of Würzburg re-tested Grashey's patient in several ways. One of them was to hold Voit's hands and feet tightly and have him stick out his tongue when watching the object to be named. This made it impossible for the patient to make any kind of writing movements. Under those circumstances Voit could not name any object. (The tongue could be retracted after the object was removed). This seemed to confirm Grashey's theory. However, Sommer (1891)[14] discovered that, in the same shackled state, and presented with the object plus the first few written letters of the object's name, Voit could neither confirm nor deny that these letters belonged to the name. That should have been possible under Grashey's hypothesis. Sommer, however, could not provide an alternative theory and he was apparently not aware of Cattell's innovative experiments, in spite of the fact that he, Sommer, had spent two years (1887–1889) as an assistant in Wilhelm Wundt's laboratory.

The experimental approach to reading, introduced by Donders, was soon to show that Wernicke, in his earlier work, had been right to suppose that a word image can, as a whole, activate the corresponding sound image. In the year of Grashey's paper, 1885, James McKeen Cattell (1860–1944, Fig. 5.5) published his first paper on the naming of letters, words, colors, and pictures. This work was conducted in Wilhelm Wundt's psychology laboratory in Leipzig as part of a dissertation project on mental chronometry under Wundt's supervision. Cattell, who had a master's degree from Lafayette College in Pennsylvania, stayed in Wundt's laboratory from 1883 to 1887. He was the first American

[14] Sommer's (1891) paper was entitled *Zur Psychologie der Sprache*. It was probably the first journal paper with "Psychologie der Sprache" in the title.

to obtain a PhD in psychology (in 1886) and became one of the pioneers of American psychology, spending most of his professional life at Columbia University.

The device used in that first experimental paper was quite simple. It was, in essence, an upright cylinder that could turn clockwise. Around the cylinder Cattell would attach a string of letters, words, color patches, or pictures, one per centimeter. The string was covered by a screen with a slot of variable width. As the cylinder turned, the subject would see the stimuli through the slot, passing by from right to left. If, for instance, the stimuli were letters and the slot had a width of 1 cm, there would always be one letter in view. With a slot width of 2 cm it would be two letters, etc. The subject's task was to name the letters, words, colors, or pictures as they passed by. Cattell would then increase the turning speed of the cylinder until the subject started to make errors. The maximum speed without errors provided Cattell with the minimum presentation time per stimulus needed for the subject to perform the task. A typical result was that correctly reading letters in a 1-cm slot required some 250 ms per letter. With wider slots this speeded up to an asymptote of around 200 ms per letter when a slot contained four to six letters. Apparently you can detect upcoming letters while pronouncing the current ones. Reading short words took hardly more time than reading letters. Cattell took this to mean that a word's letters are "not perceived one after another, but rather in one mental process as a whole group." Also, words are not read one after another, but as whole groups.[15] Still, this result does not yet undermine Grashey's spelling theory. As Cattell correctly remarked, the maximum reading speed is only limited by the speed of articulation. When a text from *Gulliver's travels* is read at an average speed of 200 ms per word, with an average word length of, say, five letters, you still have 40 ms to scan each successive letter, enough to read Grashey-wise.

The issue was more definitively settled in a subsequent study, which was the core of Cattell's dissertation and which appeared in three subsequent *Mind* papers.[16] Here, Cattell used and extended Donders' subtraction method to determine the mental durations of perceptual discrimination and response choice (or "will"). He measured individual reaction times to stimuli, as Donders had done, but with a much improved device, the so-called "gravity tachistoscope." Responses could either be hand movements or naming responses. In two sets of experiments,[17] Cattell used Donders' discrimination method for the detection of letters and words. The subject was instructed to *only* respond when one particular letter appeared (for instance *V*) out of 26 different letters, or a particular word (for instance *life*), out of 26 different words. On average the response times were about 150 ms longer than a response in the simple condition. This extra time is spent perceptually distinguishing the stimulus from all other 25 stimuli in the experiment. Cattell also ran Donders' choice condition, with the subject naming each

[15] "Die Wörter und Buchstaben werden nämlich nicht einzeln einer nach dem andern appercipiert, sondern mit *einem* geistigen Process gleich eine ganze Gruppe" (Cattell 1885, p. 648).

[16] Cattell (1886b, 1886c, 1887a).

[17] Cattell (1886c).

letter or word. This took roughly an additional 150 ms. Hence, perception and choice times were roughly the same.

Response times (whether hand or voice) hardly differed for letter and word discrimination. For Cattell himself, they were all around 325 ms. If reading a word involves the successive recognition of its letters (at a rate of around 30 ms per letter), you would not expect these results. Cattell's conclusion was, "we do not therefore perceive separately the letters of which a word is composed, but the word as a whole," and Cattell was practical enough to add: "The application of this to teaching children to read is evident; I have already in connexion with other experiments called attention to it."[18] It was the beginning of a never-ending pedagogical dispute.

Cattell's four papers also represented the very first reaction-time data on color and picture naming. The 1885 paper was the first to report the classical finding that naming a printed word is much faster than naming a picture or color. Still, *recognizing* a printed word is usually slower than recognizing a picture or color,[19] and Cattell concluded that the longer naming latencies for pictures and colors must be due to the difficulty of finding the correct name. Cattell pleaded for the introduction of this kind of naming chronometry in the study of aphasia. The results were replicated in the second and third *Mind* papers, using a better experimental technique for measuring the reaction time for each stimulus presented. The third *Mind* paper adds the naming of one-, two- and three-digit numbers, again a first in psycholinguistics. The naming latencies for one-digit numbers was about 360 ms, only slightly faster than naming a monosyllabic word. Adding a digit to the number added some 40–50 ms to the naming-response latency. Cattell left undecided whether this lengthening was due to perceptual discrimination or to response preparation, but he did obtain a corresponding result for reading short versus long words. Monosyllabic words, such as *child*, *earth* or *house* were named some 40–50 ms faster than polysyllabic ones, such as *education*, *knowledge* or *architecture*. In this case Cattell did test (by using Donders' discrimination method) whether discrimination times differed. They did, by some 20 ms. By implication, the extra response-preparation duration for long words must be around 30 ms.

Finally, Cattell was careful to always measure response latencies for native and non-native language stimuli and responses. These experiments were the first to show a general latency advantage when you use your native language. Cattell immediately suggested that reaction times could be used as a method for testing language ability in school examinations.[20] Cattell was an optimist about the evolutionary progress of the mind in the human species: "If in the course of evolution, as is probable, the molecular arrangement of the nervous system becomes more sensitive and delicately balanced, we may suppose that the times taken up by our mental processes become shorter, and we live so much the

[18] Cattell (1886c), p. 387.

[19] Cattell used Donders' c-method with hand or voice response in these recognition experiments.

[20] "Diese einfache Methode, die Bekanntschaft Jemandes mit einer Sprache zu constatiren, könnte vielleicht bei Schulprüfungen verwandt werden" (Cattell 1885, p. 646).

longer in the same number of years." The research challenges were obvious: "It will therefore be of great interest to make experiments such as these on the lower races, as well as on persons of different age, sex, occupation, &c."[21] We will return to Cattell for his highly original work on word association.

Benno Erdmann and Raymond Dodge

Six years after Cattell's final publication on reading, Benno Erdmann (1852–1922), a professor of philosophy and Kant scholar, began a major new project on reading at the University of Halle, not far from Leipzig. There was no psychology lab there and the equipment for tachistoscopic presentation of letters, words, and sentences, as well as for recording the registration of response latencies, had to be constructed from scratch. Here, in 1894, Erdmann's American PhD student Raymond Dodge (1871–1942), came into the picture. Dodge had obtained a BA at Williams College and was deeply interested in Kant's philosophy. After his application to Harvard had failed, Erdmann accepted Dodge as a graduate student. Dodge's only funds were the five hundred dollars he had been able to save as a student, but he managed to survive on these funds. Dodge masterminded the construction of the tachistoscopic equipment and then, as Erdmann put it, "the student became the factual co-worker" on the project. Their co-authored book, the first experimental monograph on reading, appeared in 1898.[22]

The book opens with rejecting the still widespread idea that reading consists of letters flowing over the fovea for successive recognition and successive articulation,[23] as in Grashey's theory. Rather, the authors claim, reading consists in an alternation of fixations ("resting pauses") and saccades ("eye movements"). This had already been demonstrated in 1893 by Lamare in Javal's laboratory,[24] but the authors seem to have missed this. It was Lamare who had introduced the term "saccade." Erdmann and Dodge demonstrated that in reading a "light, accessible" text, you make some five to seven successive fixations on a printed line. Dodge, for instance, would read aloud a text by John Locke. Erdmann would at the same time watch one of Dodge's eyes by looking from the side into an oblique mirror positioned next to the text. He would count the number of fixations Dodge made on a line. A more difficult text or a text in a foreign language (Dodge reading Helmholtz's *Optik*) produced more fixations per line. From the number of fixations the authors could compute the average angle of a saccadic jump. Earlier measurements by Helmholtz's student Lamansky had produced estimates of jump durations for different angles.

[21] Cattell (1887a, p. 534).

[22] Meanwhile, Dodge also wrote his dissertation on motor word images (Dodge, 1896), an introspective analysis of conscious content during the production of a spoken word.

[23] Later in the book the authors also do away with the naive tacit assumption that quick articulation of a word's successive letters will produce the spoken word (p. 192).

[24] See Javal (1878) and Lamare (1893). The latter used a clever contraption in which a blunt point contacted the reader's upper eyelid. When the eye jerked, it caused a current to pass through the point, activating a microphone which transmitted the click sound through a rubber tube to the ear of the experimenter. This made it possible for him to count the number of saccades.

This made it possible to compute the average duration of a saccade and, taking the total reading time for the text into account, the average duration of a fixation. For Dodge reading Locke, the average fixation time was 0.5 s and the average movement duration 0.02 s. These measurements formed the basis for an important and entirely new conclusion, namely that you do not read during a saccade. On average, some 25 letters will pass the fovea during a single saccade, which is less than 1 ms per letter. When you fixate, in a small window, such a fast-gliding string of letters, you see a gray band; recognition of letters is impossible. The flip side of this conclusion was of course this: you only read during fixation pauses.

The authors' next experiment determined how much a reader could recognize during a single fixation. A fixation was now simulated by tachistoscopic presentation, in a manner quite similar to that of Cattell. The subject would see a letter or word combination in a flash and then name it. The chosen presentation time was 0.1 s, definitely short enough to prevent eye movements during presentation. Like Cattell, they observed that participants could correctly report on a group of some four unrelated letters. But words as long as 12 letters, occasionally even more, could be recognized. The question was "Why?" One hypothesis is that a word's sound image, which is a strong linear association of speech sounds,[25] can be triggered by a subset of the word's letters (each of them related to one of the speech sounds). This may be true, but it necessitates that at least these few critical "determining" letters of a word are actually recognized. In order to test this, the authors ran experiments with words whose individual letters could *not* be recognized. They would position themselves so far away from the daylight-illuminated display screen that individually presented letters could no longer be named correctly. There was no time limit; the letters stayed on view, as in an optician's clinic. But then, when they put the letters together to form a word, recognition often succeeded. This decisively showed that Grashey's spelling theory was wrong. There are, apparently, optical properties of the word, larger than individual letters, that contribute to its recognition. This could be the word form as a whole, but also the shape of frequently occurring parts, letter groups of a printed word, such as syllables. In fact, this whole-word perception worked *better* for longer words than for short ones, the former apparently containing more such global optical cues than the latter.

Further tachistoscopic experiments showed that the authors could often read a whole short sentence when fixating the middle of the screen on which the sentence was flashed. Although sentence coherence played a role here, it would not have worked without global optical recognition of words in the periphery.

So far, none of the many experiments had been reaction time experiments, like Cattell's. However, the second half of the book addressed the issue of a Donders subtraction approach to reading head on. Was it possible to decompose the process of naming a letter or a word in its successive components, such as recognition and response choice? To test this question, they first reanalyzed Cattell's published reaction-time data.

[25] By Goldschneider and Müller (1893).

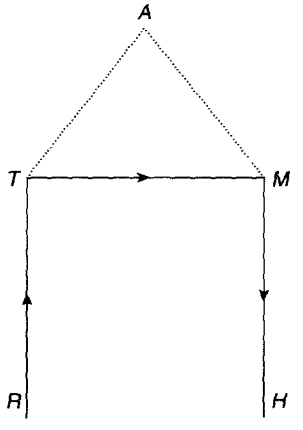


Fig. 5.6 Component processes in word reading. Here *R* represents the reader's retina, *T* is the end- and distribution point of the sensory "wire," and *M* is the motor center for the initiation of hand or speech movements executed by *H* (hand or speech organs). Finally, *A* is the "apperceptive center" where recognition and choice take place. From Erdmann and Dodge (1898, p. 211).

Without reference to Lichtheim they used his "house" to represent the component processes that Cattell had distinguished (Fig. 5.6).

In this schema, Donders' simple reaction, such as pushing a button in response to a light or a fixed letter, is running the reflex arc *R–T–M–H*. However, in a choice reaction, such as naming different letters or words, center *A* gets involved. It causes you to recognize the specific letter or word and choose the corresponding name. Cattell had assumed that conduction times (*TA* and *AM* in the figure) were equal and that roughly the same holds for discrimination and response-choice durations (central processing within *A*). However, he had not gone into the detailed subtraction computations his extensive data tables allowed for. Erdmann and Dodge made up for this in a full, 76-page analysis of Cattell's data for letter and word processing.

This analysis produced many surprises, such as quite unstable and occasionally even negative component durations. We will not follow the authors in the various refinements they tried out,[26] but we will mention their main conclusion, namely "that it is, on principle, not well-advised to derive recognition times or innervation times for naming or reading aloud from reaction times for the recognition of written stimuli and the corresponding speech production."[27] Or, in other words, do not apply the subtraction method to such a componential analysis of the reading process. This does not work, according to the authors, because the components are not well understood. In a detailed introspective analysis, they argued in particular that recognizing a letter or word is not, as Cattell, following Donders, supposed, an act of *discrimination*. When you recognize a particular letter in the experiment, you are not aware of the other possible stimuli. There are no phantom images of the other letters around. More importantly, they do not even

[26] We would now conceive of many of these surprises as mere statistical accidents.

[27] "Die Erörterungen des vorigen Kapitels sollten darlegen, daß es aus prinzipiellen Gründen nicht angezeigt erscheint, aus den Reaktionszeiten für die Erkenntnis der Schriftzeichen und die ihnen entsprechenden Lautreproduktionen Zeitdaten für das Erkennen sowie für die Innervation des Benennens und lauten Lesens abzuleiten" (Erdmann and Dodge 1898, p. 249).

work sub-threshold, because then the response time to a letter such as *V*, which is quite similar to other letters, such as *U*, should be systematically longer since it is harder to discriminate. But the data did not show this systematically. Similarly, there is no conscious experience of *choice*. When you name a presented letter, you simply do not consciously consider the other possible letter names. Also, the data showed no effect on naming latencies if other similar names were around. Taking these two observations together, the authors conclude (on p. 342) that in naming a letter or word you run a direct associative connection between the visual sign and its spoken name.

This, of course, does not argue against reaction-time experiments in reading, only against this particular componential analysis. The finishing touch of Erdmann and Dodge's experimental project was the collection of extensive speech reaction-time data for letter and word naming. Interestingly, these data largely confirmed Cattell's findings. The experiments were mostly of the choice reaction type, naming one of 26 possible letters or, as a comparison, 26 different words. Like Cattell, the authors found that naming a single letter took as much time as naming a monosyllabic four-letter word. And they drew the same conclusion: Grashey's spelling theory was wrong. They also confirmed Cattell's finding that naming latencies increase with word length.[28]

In retrospect, it was a great victory for two Kant scholars to produce this monumental first experimental monograph on reading. However, this was certainly not the view of Wilhelm Wundt, who was working just 15 miles down the road from Halle. In 1900 he published a 30-page critical commentary on the book, or rather on just one aspect of it. Wundt's bone of contention was the tachistoscope Erdmann and Dodge had used. Dodge's design had been to project the stimulus, which is illuminated by an electrical lamp, onto a small ground glass screen. This was a clever contraption that gave full control over the exposition durations: when the word or letter stimulus is not on display, the frosted glass continues to be illuminated. This small strip of light was mounted in a black cone-shaped contraption, which ended in a black cloth, to be drawn over the observer's head. This arrangement incurred Wundt's wrath. Under these conditions, he argued, the subject's eyes would be fully dark adapted, which would mean losing control over the duration of the visual image. For the default exposure time of 0.1 s, the afterimage would persist for a multiple of this time. During that longish interval, attention could freely move over the visual image. The only way to keep control over the image duration is to keep the subject's eyes daylight adapted, argued Wundt. This is what Cattell had achieved by using his gravity tachistoscope. In the latter case, the subject is in daylight and the screen is surface illuminated; afterimages hardly play a role. With some difficulty, Wundt found a difference in the experimental data: Erdmann and Dodge could recognize slightly longer words than Cattell had reported. That is what you get, Wundt declared, when your image duration is much longer than your exposition time.

[28] In these experiments word length is certainly confounded with word frequency. Although the authors do discuss the possible role of familiarity, there is no beginning of de-confounding all these factors.

The authors were understandably offended by Wundt's pedantic treatment of a marginal issue. After all, their stimulus conditions had been superbly constant over the experiments and, unlike Cattell, their aim had not been to measure mental durations of component processes. Their immediate response, the same year, opened with one line of irritation: "The tone by which Wundt, as in similar cases, addresses us might have occasioned us not to react: the form of his polemic characterizes him, not us."[29] But then, the prestige of the critic made them, after all, decide to clarify the issues and they then went into a 25-page detailed, polite and business-like response. But Wundt had to have the last word. His immediate, second critical ten-page paper repeated the same issues, castigated Erdmann and Dodge for their personal remarks, and concluded "I deny that the authors have, in any essential point, surpassed Cattell in terms of firmly established facts."[30] Wundt was a master in many things, but not in making friends.

Walter Pillsbury and Oscar Quanz

Another remarkable contribution to reading research was Pillsbury's 1897 dissertation, supervised by former Wundt student Edward Titchener at Cornell University. Walter Bowers Pillsbury (1872–1960) had received a BA from Nebraska University, with Harry Wolfe, another former Wundt student, as his main teacher. The first half of the dissertation is an extensive discussion of Wundt's notions of apperception and association. It provides the theoretical framework for addressing a phenomenon that had struck Pillsbury's imagination: why do we often misread words? And why do we often not notice spelling errors? Apparently, two different letter patterns, for instance *sufdenly* and *suddenly*, can occasionally give rise to the same perception (here the word "suddenly"). Apart from *objective* factors—the presented visual stimulus—other *subjective* factors must be at work according to Pillsbury, and he sat out to study the role of both of these factors in a series of tachistoscopic experiments. He would present misprinted words on a ground-glass screen in a tachistoscope quite similar to Erdmann and Dodge's. The stimulus would be displayed for up to half a second, depending on the subject and experimental conditions. Pillsbury introduced three types of distortion: letter blurring, substitution, or omission. For example, the target word "whenever" could be presented in any of three distorted shapes (see Fig. 5.7).

In all cases, the subjects' task was to report exactly what they had seen. Would the subject detect the distortion? Subjects noticed it least often in the case of blurring and most often for deletion. Pillsbury then checked the probability of detection in different letter positions of a word. Would it be easier to detect *x* in *xhenever* than *whenexer*? Yes, it was. The later the distorted letter, the harder it was to detect it. This already shows the work of "subjective factors." The reader has the tendency "to read the word from left to

[29] "Der Ton, in dem Wundt, wie in ähnlichen Fällen, so auch uns gegenüber redet, hätte uns einen Anlass gegeben, *nicht* zu antworten: die Form seiner Polemik charakterisiert ihn, nicht uns" (Erdmann and Dodge 1898, p. 241).

[30] "Ich leugne, dass die Verff. an sicherstehenden Thatsachen in irgend einem wesentlichen Punkte über Cattell hinausgekommen seien" (Wundt 1900c, p. 70).

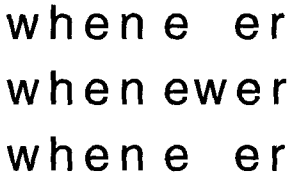


Fig. 5.7 Three different kinds of misprint: blurring, substitution, and omission, as used by Pillsbury (1897).

right" (p. 350). An early letter disfigurement is easier to recognize since there is only "slight expectation of the word to come." But later in the word the expectation is strong and affects the perception. This is a clear case of "apperception" at work. Word length itself, however, is irrelevant.

To study subjective factors, Pillsbury used a priming paradigm.[31] Just before the stimulus appeared on the screen, Pillsbury called out a word associated with the stimulus. So, when the stimulus was going to be the distorted word *winxow* (i.e. suggesting "window"), the priming word might be *pane*. This manipulation had a strong effect. Far fewer distortions were detected when such a priming was used than in the no-priming condition. According to Pillsbury, the priming gave the subject "greater certainty that the word suggested should be the correct word, and thus strengthened the effect of the word suggested upon the separate letters so that the misprints were overlooked more frequently" (pp. 374–375). Again, Wundt's notion of active apperception is applicable here, according to Pillsbury. Recognizing individual letters *follows* recognition of the whole word. The author disagreed with Wundt, however, on the point of association. According to Wundt a word's individual letters also call one another by mere association. So in *winxow*, the *n* would call a following *d*. But how could this work before the whole word had been recognized? After all, in most other words an *n* is not followed by a *d*.

Another pioneer of experimental reading research to be mentioned was John Oscar Quanz[32] of the University of Wisconsin. He was the first to measure what we now call the "eye–voice span." In reading, the eye is ahead of the voice. Quanz measured this span at different positions on the line by suddenly cutting off the view of the text when a particular word was spoken and then recording how many additional words the reader could still utter. The eye must already have caught these later words while the reader was uttering the word at the break-off point. The eye-voice span varied with such factors as the familiarity of words and the reading skill of the subject. Individual differences in reading skill were a main interest of Quanz, and became a core issue in the teaching of reading, which was to become an industry in itself.

Edmund Huey

The ultimate development, however, of *fin-de-siècle* reading research was Edmund Huey's invention of an eye-tracking device.[33] Huey (1870–1913) designed this as part of his

[31] First used by Münsterberg (1892, p. 21).

[32] Quanz (1897).

[33] Huey (1898).

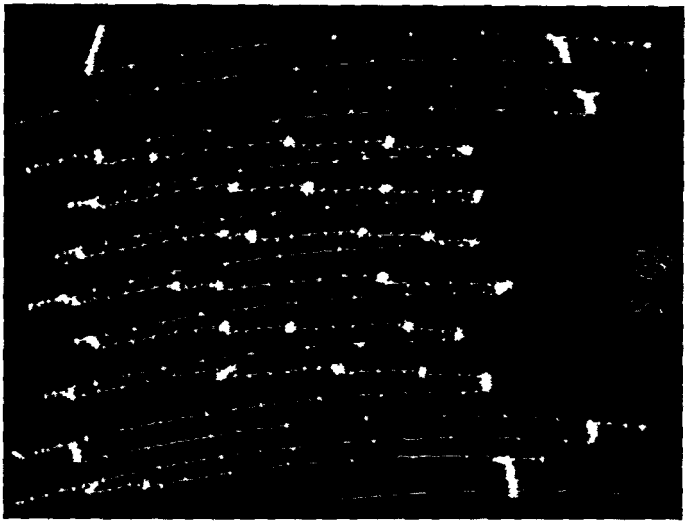


Fig. 5.8 Huey's specimen curve of recorded eye movements during the reading of six lines of text. From Huey (1900).

dissertation research on the topic of reading at Clark University. He managed to attach a cup, or rather a ring leaving the pupil uncovered, to the reader's cocaine-anaesthetized cornea. An exceedingly light lever attached to the cup would move a pointer over a moving cylinder covered in soot, tracing the reader's eye movements. Figure 5.8 shows the first such tracing ever recorded. The device made it possible for Huey to determine more exactly what Erdman and Dodge had done by means of saccade counting and speed computations. Saccades, Huey measured, take some 40–50 ms, almost independently of the arc traversed. The duration of reading pauses varied with the type of passage, but ranged between 100 and 200 ms in Huey's measurements. Huey was going to write the first textbook on reading,[34] firmly based in experimental research. We will return to it in Chapter 12.

Speech perception and William Bagley

The nineteenth-century psychology of language was dominantly, though not exclusively, a psychology of the speaker. It was the emergence or genesis of spoken language, whether phylogenesis, ontogenesis, microgenesis, or neurogenesis, which set the research agenda. Rarely was the listener, or the comprehension of speech, the object of empirical study. Steinthal's grand project, we saw in Chapter 2, was to explain how our ability to speak meaningfully had evolved in the human mind. The aphasiologists' core issue was localizing the faculty of articulated speech in the brain. Diaries of children's speech and language development were records of early speech, with at best marginal observations

[34] Huey (1908).

on the child's comprehension of language. It was largely through reading research that a genuine interest developed in the receptive side of language processing. In particular, Pillsbury's work on the visual perception of distorted words inspired the first large-scale study of spoken-word perception in context, a project which was run by William Bagley at Cornell University during the last two years of the century.

Before turning to that classic study, however, we must mention a few other early studies on the perception of speech sounds. Ear doctors and teachers had been testing hard-of-hearing children with tuning forks or ticking clocks since the 1860s. But increasingly speech stimuli were used to clinically test children's and adult's auditory acuity. Oscar Wolf (1873) was probably the first to measure the relative intelligibility of speech sounds in open space. He would utter German consonants and vowels as loudly as possible and his colleague would, for each speech sound, walk closer or further away to the distance at which the sound could just be distinguished. The published table shows that the clearest sound *a* can still be heard at a distance of 360 steps, whereas *h* needs the smallest distance, of 12 steps. All vowels carry long distances, whereas *s*, *sch*, *g*, and *ch* are in the mid range. The nasals were not tested. Wolf's method would later be taken up by Ruederer (1916), see Chapter 12. Sara Wiltse (1888) tested large groups of children by means of a standard list of monosyllabic words. In her paper she reports the mishearings of 530 pupils between 8 and 14 years of age. The *p* of *pen* is most frequently misheard as *h* of *hen*. The *f* of *fan* is mostly misheard as *th* of *then*, and so on. Also vowels have their preferred mishearings; *pen*'s *e* is most often misheard as *a* in *pan*, etc. Boas (1889) referred to this paper and correctly remarked that mishearings were a function of two factors: speech sound similarity (as *f* versus *th*) and word similarity (as in *pen* versus *pan*). The hearer checks his memory for existing words that are similar to what was heard.

A rather curious study was performed by Hansen and Lehmann in Copenhagen. The authors had set out to run an experimental test of telepathy, the transmission of thought by some unknown medium. Their idea was to maximize the probability of such transmission by relaying the "energy" of the sender's thought to the receiver by a system of two concave mirrors, one at the sender's and one at the receiver's side. The sender would put his head in his mirror's focus and "think" a two-digit number. The "thought beam" would then be captured by the receiver's mirror and projected onto the receiver's head which was in the mirror's focus. The two heads were 2 meters apart. Would the receiver be able to reproduce (write, draw) the image of the thought numeral? Results: random. But what if the sender silently *named* the numeral? There had been previous reports that this procedure worked.[35] Here, however, Hansen and Lehmann argued, it must have been acoustic transmission of whispered speech, involuntarily and unconsciously produced. Even if the experimenter honestly hears nothing, the two-mirror channel may effectively transmit near-threshold whispering. To check this, they ran an experiment between them where they did not inhibit silent whispering.

[35] The paper was a response to the work by Sidgwick in Cambridge, who claimed to have found thought transfer. Sidgwick (1896) responded to the Copenhagen paper, stating that his results were obtained with the two subjects in different rooms.

Now, one third of the two-digit numerals were correctly transmitted.[36] The authors analyzed the digit confusion in this and further experiments. It turned out that, in all cases, the statistics of these confusions could be explained from characteristic similarities (under whispering conditions) of vowels, but in particular, consonants. The authors were both Danish. The numeral 80 (*otti*) would be heard as 8 (*otte*) or the numeral 1 (*én*) as 5 (*fem*), i.e., *f* got lost and *n* was heard as the closely related *m*. Although the pages-long analysis makes sense, the drawback of this experiment is, of course, the use of numerals. You cannot systematically compare all speech sounds when you are limited to numerals.

William Chandler Bagley (1874–1946) did his PhD research under supervision of Edward Titchener at Cornell University. Later in life he made a reputation for himself as a professor of education, first at the University of Illinois, then at Columbia. Relevant here is that he was the first scientist to perform and publish an experimental study of spoken word perception in context.[37] His experiments were not only theoretically well-conceived, but also well-designed and high-tech. When you perceive a spoken word, how much of your perceptual image is determined by the stimulus, the perceptual input, and how much by the context in which the word is produced? In the terminology of the day, how much is due to objective perception, how much to subjective apperception? Bagley designed his experiments after Pillsbury's model, but he did it much more systematically. Like Pillsbury, he presented distorted words to his subjects, but spoken ones this time. His "mutilations," as he called them, were in all cases elision of a consonant. But he systematically varied the consonant's position in the word. It could be the *initial* or the *final* consonant of a monosyllabic word (such as *joy* mutilated to *oy*, or *sleep* mutilated to *slee*), or it could be a middle consonant in a polysyllabic word (such as *using* mutilated to *uing* or *nobler* to *noler*). Hence, the context varied was the within-word relations between the "symbolic elements" (the speech sounds). The choice of words was such that, for each of the three locations *all* possible consonants in that location were represented. Also each consonant would appear with as many different vowels as possible, both preceding and following it. For instance, *m* would be the deleted element in *amiable, emotional, glimmering, rhyming, looming*, etc.

The high-tech feature of this study was Bagley's use of the Edison phonograph. The mutilated words (plus non-mutilated fillers) were recorded on the phonograph's cylinder and presented to eight experimental subjects under two context conditions. In the isolated condition the subject would hear a (mutilated) word and repeat to the operator what he or she had understood. In the primed condition, also modeled after Pillsbury's experiment, the operator would first speak two words that were related to the target word, immediately followed by the recorded word, so for instance: "men, fear" → *bra(v)est*. The outcome of this first set of experiments was that elision of a word's first consonant was far more disruptive than elision of its last consonant. The initial consonant is, as a

[36] The authors alternated as senders/receivers.

[37] Bagley (1900). It is the first journal paper with "psychology of language" in the title.

rule, more "determinant" for a word than its final consonant. This confirmed Pillsbury's findings for the reading of mutilated words. When priming words were presented, the subjects far more often reported having heard the non-mutilated word. They detected the elision less often than in the isolated condition.

In a second set of experiments, Bagley put the same (mutilated) words in sentence context. They could appear in sentence-initial, -middle, or -final position. These conditions could then be compared to the isolated condition, which we have just discussed. A main finding was that detecting the mutilation, i.e., correctly reconstructing the target word, increased from beginning to middle to end of sentence and all context conditions were better than when listening to the stimulus in isolation. Meaningful context builds up over the sentence, helping to apperceive the intended target. These effects of context are less pronounced for the polysyllabic words, i.e., those with a "middle" consonant elision. On the other hand, the (undistorted) target is more easily recognized for these longer words.

Because Bagley had systematically elided all possible consonants, he could compare them. Eliding one consonant might be more disruptive than eliding another one. This indeed was the case. The statistics showed that the plosives (*p*, *b*, *d*, *t*, *g*, and *k*) are the most critical in spoken word perception. Eliding them is most disruptive. Least critical were the semi-vowels (*w*, *r*, and *l*). Here Bagley stressed the relation to early speech recognition, citing Tracy's data on plosives being the first consonants to appear in the child's speech. But the semi-vowels are special in that they are most often apperceptively inserted by the hearer in the position of elision. That is, Bagley supposed, because they are intermediate between the explicitly voiced and unvoiced consonants.

Like Titchener (and Wundt, and James), Bagley wanted to establish the stream of consciousness leading from the stimulus to the response. In a further, extensive experiment he collected the introspective reports of his subjects for all experimental materials. We will not follow his detailed analysis of these reports. One issue, however, was the question raised by Stout (1899): was there such a thing as "imageless consciousness," a mode of consciousness that is neither visual, nor auditory, nor otherwise "sensory," but rather "pure thought." Bagley did not like that idea. But it was about to become a hot issue in Oswald Külpe's Würzburg laboratory, where "angry young men," such as Karl Bühler were about to break with Wundtian psychology (see Chapter 7).

Verbal learning, memory, and habits

Hermann Ebbinghaus

A single all-important study set the scene and experimental standards for decades of verbal memory research. In 1885 Hermann Ebbinghaus (1850–1909, Fig. 5.9) published his 169-page monograph *Über das Gedächtniss. Untersuchungen zur experimentellen Psychologie* ("On memory. Studies in experimental psychology"). It was the result of several years of solitary experimentation, cumulating in two (non-consecutive) years of precisely planned, systematic tests on a single human subject: Hermann Ebbinghaus himself. Ebbinghaus had studied philosophy in Bonn, Halle, and Berlin, receiving his

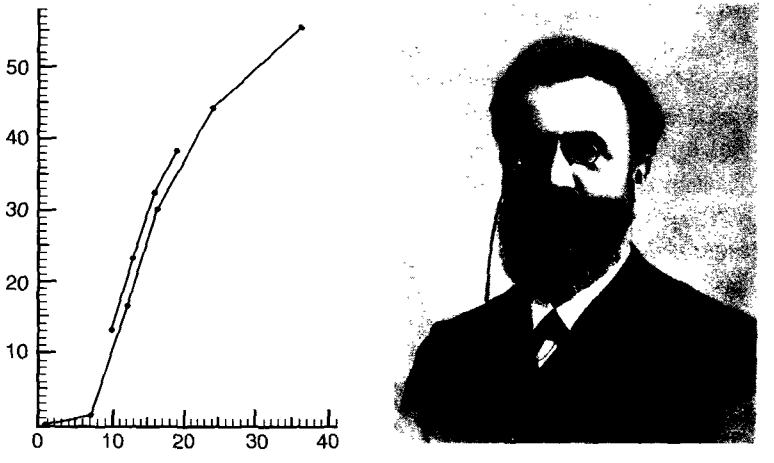


Fig. 5.9 Hermann von Ebbinghaus and his "learning curve" (see text).

doctorate back in Bonn in 1873. But in psychology Ebbinghaus was, by and large, a self-made man. On his own account, reading Gustav Fechner's *Elements of psychophysics* put him on the track of experimental psychology; "*I have it only from you*" is his dedication of a later book to Fechner.[38] Fechner had pioneered the quantitative measurement of sensation, such as brightness or loudness. Ebbinghaus was the first to conceive of a way to measure higher mental processes: memory. A scientific approach would require that measurements were numerical and that they were repeatable under constant conditions. Of course, there will be variability, but that in itself can be quantified by way of Gaussian statistics. To best meet these conditions, Ebbinghaus introduced the "nonsense syllable" as his experimental material. It consists of a vowel flanked by two consonants, such as *tif* or *wap*. By using all German vowels (including diphthongs) and consonants, Ebbinghaus constructed a corpus of about 2300 possible German syllables. The basic experiment was exceedingly simple. Ebbinghaus would present himself with an arbitrary string of such syllables. He would then imprint the ordered string by reading/reproducing the syllables aloud, time and again, till reaching the state where, presented with the first syllable, the whole string could be produced in a fixed tempo and without hesitation.

A first quantitative measure was, of course, the number of repetitions you need to learn a string of a given length. Figure 5.9 shows the experimental curve. Ebbinghaus needed some 55 repetitions to learn a 35-syllable string, but for strings up to seven syllables only one repetition or less was required.

Ebbinghaus' next issue was to quantify forgetting. How strong is a memory trace at a given moment, for instance after 24 h? To measure this, Ebbinghaus counted the number of repetitions required the next day, at the same time, to again correctly reproduce the string. The stronger the memory trace, the fewer the number of repetitions needed.

[38] "Ich hab'es nur von Euch." *Grundzüge der Psychologie* (1897).

Ebbinghaus demonstrated that you can strengthen the trace at first imprinting by "overlearning" it, i.e. by correctly repeating the string one or more times after the first correct reproduction. Overlearning helps. The more you overlearn, the fewer repetitions you need, proportionally, the next day. There is a limit though; you cannot overlearn so much that you do not forget.

Ebbinghaus then, famously, measured the "forgetting curve" over a stretch of time, up to 31 days. The curve characteristically shows that forgetting is initially fast, but then levels off. You need almost as much extra learning after a week as after a month. Ebbinghaus demonstrated that the curve is logarithmic.

Ebbinghaus successfully used his paradigm to test a host of other memory phenomena. All of us, for instance, have heavily "overlearned" the alphabet. Is that of help when you have to reproduce it in inverse order? Somewhat, but actually very little when you measure this experimentally. The forward association between successive nonsense syllables is much stronger than their backward association. Similarly, the association between *non*-successive syllables in a string is measurable, but also quite weak. What about the learning of meaningful materials? Ebbinghaus reported one experiment where he imprinted 80-syllabic strophes from Byron's Don Juan. They needed some eight repetitions. This is a tenth of what Ebbinghaus needed to learn a string of 80 nonsense syllables, some 80 repetitions. Meaning is, measurably, a powerful factor in memory.

The new paradigm was to have large-scale applications in behavioristic psycholinguistics, but Ebbinghaus himself was a mentalist, like Wundt, James, and all of his contemporaries in psychology.

Benjamin Bourdon

The doctoral thesis of Bourdon, *L'Expression des émotions & des tendances dans le langage* (1892) ("The expression of emotions and tendencies in language") was a remarkable dissertation by an independent mind. Bourdon (1860–1934) had studied in Paris, where "biological" psychologist Ribot had been his major source of inspiration. The British associationists provided his theoretical perspective. He then made a study tour through Germany, in particular to Leipzig. There he visited Wundt, his laboratory, and Cattell among others. He also met the neo-grammarian Brugmann. He then returned to France and accepted a position at the Lycée de Rennes, where he taught philosophy and experimental psychology and wrote his dissertation.

The dissertation begins by raising the issue of how emotions are expressed in speech. Emotions primarily affect muscular force and in particular respiratory force. As long as it is kept in bounds, emotion will modulate the intensity of voice. That is observable in speech as variation in stress and pitch, in vowel duration, in speaking rate and interruptions. But then Bourdon turns to the *tendances*, the habits of speech. Bourdon analyzes speech as a system of social habits. The real units of speech are its high-frequency, habitual elements, such as intensities, speech sounds and clusters, syllables, words with their affixes and clitics, stock phrases, etc. The core process of speaking is the successive expression of ideas by such units of speech, often but not necessarily always, words. Ideas are expressed by their *associated* speech units.

Bourdon systematically discussed composition, the successive arrangement of units of speech. Let us consider some of these chapters. There is a chapter on the arrangement of "elements of articulation," phonemes. Here, Bourdon presented the first diphone statistics ever, based on texts by Balzac and Rimbaud. He analyzed these texts in the way they should be spoken, i.e., in terms of the speech sounds that would result. Diphones are successions of two speech sounds, such as *da* or *fr* (not *digrams*, i.e., successions of letters in the printed text). Bourdon's first table was a huge list of diphones that never occur in these spoken texts. Examples are *wp* and *oo*. Then followed lists of diphones occurring once, twice, etc., up to 48 for one text (the diphone *ar*) and 54 in the other text (the diphone *wa*, as in *quoi*). Bourdon then discussed various regularities in these statistics, such as the suppression of sequencing same or like speech sounds, tendencies of assimilation, etc.

Other chapters dealt with the arrangement of words and intensities in speech. The strongest, most important ideas are the first to be expressed in words or other units of speech and with the highest intensity. The natural arrangement over an utterance then is from high to low intensity, i.e. from higher to lower pitch and stress. That is what one can observe in the speech of children and of the "common man."[39] "The order of words tends itself to imitate the order of ideas,"[40] Bourdon wrote. It is the "natural syntax." There are, of course, grammatical habits that can counteract such an arrangement, especially in the educated. But more often syntax will be adapted to the order of importance (*You have been in Paris?* versus *Is it in Paris that you have been?* p. 207).

As the book proceeded, Bourdon increasingly addressed current misconceptions in linguistics. Linguists tend to forget that words are not *printed* words. Words are rather cohesive ideas that are expressed by stretches of articulation. So (in French) *tu pars* ("you leave") is a word and so is *il vient d'entrer* ("he has just come in"). From the perspective of spoken language the whole distinction between synthetic and analytic languages makes no sense (p. 273). The linguistic literature keeps expressing the superiority of inflectional languages, but psychologically this claim is not based on anything. Bourdon added a chapter on "useless parts" (*parties inutiles*). The upshot is that inflections and function words are almost totally useless. Take the expression of person by verb inflection. It hardly ever adds to meaning. In English one would do just as well, Bourdon wrote (p. 278), with *I sing, thou sing, he sing, we sing*, and similarly for German *ich sing, du sing*, etc. But worse, inflections are plainly misleading. In French, the conditional tense inflected *chanterais* ("will sing") has the same ending as *jamais* ("never") and *chante* ("sings") has the same ending as *méchante* ("naughty"). In other words, most of this inflectional machinery does not serve any symbolic function. Only content words can perform such functions, as we can do quite economically in telegram style. There exists a simple regularity: the shorter a word, the less specific. The word *is* hardly means anything, but *hippopotamus* (not Bourdon's example) is quite specific. Here Bourdon anticipates Zipf's (1949) analysis

[39] "L'homme du peuple." (Bourdon 1892, p. 196).

[40] "L'ordre des mots tends lui-même à imiter l'ordre des idées" (ibid. p. 197).

but does this in a more general and methodological way. "To study with exactness the habits of speech that are dominant in a society, it suffices to carefully observe and, in case of doubt, to perform statistics as we have done in the present work."[41]

In 1896 Bourdon was appointed to the chair of philosophy at the University of Rennes. There he was to establish a "laboratory for psychology and experimental linguistics." That was achieved in close cooperation with the chair of linguistics, Loth, and then his successor Dottin. This was the world's first laboratory of its kind. There were psychological laboratories, there were phonetic laboratories, but only here did experimental linguists and psychologists join forces.

Association and analogy

Francis Galton

One day Francis Galton (1822–1911), based in London, wrote down 75 different words each on a small sheet of paper. A few days later he put the sheet out of view under a book, but slightly sticking out, so that if he bent forward he would see a word. He then took his stopwatch, bent forward and pushed it when the word caught his eye. As soon as two associated ideas had come to his mind, he stopped the stopwatch, noted down the two associations and the elapsed time. This he did for all 75 words. He repeated the exercise three times, separated in space and time. In 1879 he reported the findings in the journal *Brain*. It was to be the mother of word-association experiments, with numerous offspring. On average, it took Galton just over 2 s to form the two associated ideas. He found that quite slow, but what surprised him more was that 42% of the associations of a word occurred more than once, i.e., on two, three, or all four occasions; "the roadways of our minds are worn into very deep ruts," Galton sighed. Our memory "is apparently always engaged in mumbling over its old stores."[42] And indeed, old stores they are. For a large subset of his associations, Galton could estimate at what stage of life he must have acquired the association. It turned out that one half of them date from the period of life before leaving college. Galton decided not to print his associations, because they "lay bare the foundations of a man's thoughts with curious distinctness, and exhibit his mental anatomy with more vividness and truth than he would probably care to publish to the world" (p. 162).

Martin Trautscholdt

The idea of association was soon picked up in Wundt's psychology laboratory in Leipzig, where Martin Trautscholdt ran the first Donders-paradigm experiments on word association as part of his dissertation project. It was clear to Trautscholdt that a substantial part of Galton's association duration had gone into reading and understanding the

[41] "Pour savoir exactement quels sont à moment donné les habitudes de parole prédominantes dans une societé, il suffit d'ailleurs d'observer avec soin et, dans le cas douteux, de faire des statistiques analogues à celles que nous avons données dans le présent ouvrage" (Bourdon 1892, p. 371).

[42] Galton (1879), p.155.

stimulus word. Trautscholdt's (1883) simple solution was to pretest the stimulus words. He would simultaneously speak the stimulus word (always monosyllabic) and push a button. The subject had to move a lever as soon as he understood the word. This took slightly more time than responding in the same way to a tone stimulus (the "simple" reaction time). The extra time must be due to recognizing the word. For subject Wundt this time was, on average, 107 ms, for his American student Stanley Hall it was 137 ms. In the main association experiment the subject would move the lever as soon as an association word had entered consciousness as a response to the spoken word. By subtracting from this total reaction time the simple reaction time and the computed word-recognition time, the pure mental association duration was estimated. The average over all subjects and experiments was 727 ms. It takes, on average, some 0.75 s for one clear idea to call up another.

Trautscholdt also analyzed the character of the associations. Here he followed Wundt's classification. There are "outer" associations, where the two ideas are contiguous in space, time, or otherwise (*house* and *garden*, *March* and *April*) and there are "inner" associations, where there is a logical relation such as subordination, co-ordination, or superordination (*dog* and *puppy*, *dog* and *cat*, *dog* and *animal*). For Trautscholdt's four academic subjects 46% of the associations were "inner" ones. The data also offered the possibility of testing one of Steinthal's "armchair" psychological predictions. Steinthal had argued against symmetry in mental association. Temporal associations are stronger in the chronological, "natural" order than inversely and indeed there are no cases of inverse temporal associations (such as April–March) in Trautscholdt's data. It should furthermore be the case, according to Steinthal, that it is easier for the image of a part to call up the image of the whole than inversely. This is because "the mind rests in the thought of the whole, whereas the image of the part pushes it to the whole, without which it cannot be thought at all."[43] And lo and behold, part-whole associations (such as *handle–knife*) are faster than whole-part associations (such as *wheel–spokes*).

James McKeen Cattell

These issues recurred in Cattell's extensive studies of association, which he started in Wundt's laboratory and continued in England and the United States. Galton and Trautscholdt had experimented with *free* association, a method which, till the present day, raises the issue of response classification. Is it useful to talk about inner and outer associations? Are further or other distinctions necessary? This became a controversial matter, as we will see. Cattell enriched the field with a different paradigm: *restricted* association. For example, the stimulus might be a city and the subject would have to name the country in which it was situated: *Paris*–"France." Although free association is, as Galton had argued, what the mind is doing most of the day, judgment and volitional thought involve directed, restricted activation of one idea by another. As we will see in the

[43] "Denn im Gedanken des Ganzen ruht die Seele, während die Vorstellung des Teiles sie zum Ganzen treibt, ohne welches er gar nicht zu denken ist." (Steinthal 1871, p. 161).

next chapter, the latter is *apperception* in Wundt's theory, and is at the basis of human language and thought. Cattell, always matter-of-fact, did not discuss these theoretical issues in much depth in his 1887b paper on restricted association. Rather, he presented a wealth of chronometric data on different kinds of restricted association. In almost all cases he subtracted the naming time for the stimulus (e.g., *Paris*–"Paris") from the naming time for the association (*Paris*–"France"), thus obtaining the mental "association time."

The categories of restriction Cattell chose are quite interesting. The first is picture naming, already discussed. Next was translation. An English word appears and you name the German translation, or the other way round. For Cattell himself, going from English to German was going from native to non-native. For the other subject, Dr. Berger, it was going from non-native to native. Cattell discovered that, for both subjects, going from native to non-native took substantially longer than the other way round (when using short words: 84 ms for Cattell, 38 ms for Berger). This was the first chronometric study of bilingualism. Further categories of restriction used were, aside from city–country, month–season, month–following month, author–language, man–calling (e.g., *Shakespeare*–"author"), and author–language. With these categories, Cattell could test Steinthal's two predictions. To test "natural order" he added month–previous month to month–following month. And indeed, the backward direction took Cattell (431 ms) and Berger (292 ms) longer than the forward association. To test Steinthal's "part–whole" preference, Cattell added season–month to month–season. Clearly, the former involves a choice and, indeed, takes longer (93 and 138 ms for Cattell and Berger, respectively). A choice was also involved in all further categories of restriction Cattell used. Two of them had a picture as stimulus: picture—part of object (such as picture of a ship–"sail") and picture–property (such as a picture of a horse–"trot"). He used the same restrictions when presenting object names instead of pictures.

Cattell argued that directed associative-memory retrieval is also involved in the addition and multiplication of one-digit numbers. So he presented two numbers on the screen and the subject would name their sum or, in another experiment, their product. For Cattell himself, multiplication took, on average, 208 ms longer than addition. For Berger, who was a mathematician, it was 146 ms. These were the very first chronometric experiments in arithmetic, another field that was to see major developments.

Finally,[44] Cattell reported the very first experiments in *syntagmatic* association. In the first one, an adjective was presented and the subject had to find an appropriate substantive, such as *blue*–"sky." In the second, an intransitive verb appeared and an appropriate subject had to be provided, such as *swim*–"fish." In the final one, a transitive verb was presented and a direct object had to be named, such as *write*–"letter." Verb–subject appeared to be harder than verb–object, because of "the mind moving logically in the latter direction" (p. 73).

This is Cattell's most interesting work on association, but not his only work. In 1889 Cattell and Bryant published an extensive set of studies on free association in *Mind*, which we will not discuss in detail. Careful chronological experiments performed

[44] There are two further experiments in the paper, on length and number estimation, not really association experiments.

together with Berger, showed association times twice as long for abstract as for concrete nouns. For large-scale experiments, however, the tachistoscope was of no use and Cattell developed other methods, such as giving a subject 20 seconds to write down as many associations to a spoken stimulus word as possible. This was applied in a host of studies in German, British, and American schools. The authors produced the first association tables for a standard set of ten concrete words (such as *house, tree, ship*) and ten abstract words (such as *time, courage, form*), standardized over 465 subjects. For Cattell, such standards would have applicability in the testing of children of different ages or backgrounds.

Joseph Jastrow and Gustav Aschaffenburg

There were still other students of association. Joseph Jastrow (1863–1944) at the University of Wisconsin introduced the notion "community of ideas." There is much agreement in people's association of ideas. When you present *pen* as a stimulus word, most of us give "ink" as a first associate—or so it was in Jastrow's time. Jastrow (1894) tried to quantify this "community of ideas" (Jastrow 1891) by asking his subjects to respond to each of ten test words with five word associations. He then checked the agreement among his subjects on their first response word. That turned out to be 54%. For the second response word it was less, namely 38% and so on for the third (27%), fourth (20%) and fifth response word (17%). He also counted the number of unique responses, i.e. given by one subject only, for these five word associations. The number of unique responses increased over the five response positions, from 33% to 55%. He could now use this tool to compare different "communities" of subjects. In Jastrow (1896), for instance, he compared male and female students, finding more commonality among the women's responses than among the men's. Women, according to him, have a greater "community of ideas" then men. Jastrow (1894) also analyzed *types* of association. The most frequent type was the "natural kind," i.e., *dog*–"cat." Sound associations, such as *man*–"can" were quite infrequent. He also found that associations are strongly unidirectional. It was much easier to go from whole to part (*tree*–"leaf") than the other way round (as Trautscholdt had shown before).

In an extensive study,[45] Gustav Aschaffenburg (1866–1944), who would become Kraepelin's successor in Munich, collected association data, including response latencies, for use in clinical settings. He extended Wundt's categories of internal and external associations with a major clinical category of sound associations. These tend to dominate in fatigue syndrome, in neurasthenia, but also in associations made by people with learning disabilities, although in different forms. Sound associations, Aschaffenburg argued, involve little mental load because they do not involve attention to the meaning of words.

Albert Thumb and Karl Marbe

So far, the studies of association were rather self-contained psychological studies, but by the very end of the century a move was made to apply the new experimental approach to

[45] Aschaffenburg (1896, 1899).

a classical core issue in historical linguistics: how does language change come about? Steinthal had always argued that the ultimate explanation of language origins and change had to be psychological in nature. This became the accepted view in the so-called *Neogrammarians* movement, of which Steinthal's one-time linguistics student Hermann Paul (1846–1921) had become the leading theorist.[46] Where Steinthal had still paid lip-service to ethnopsychology, Paul did not. The causes of language change are to be sought in the psychology of the individual mind. We will return to Paul in the next chapter because of his controversy with Wundt on precisely this issue, the kind of psychology that should be foundational to linguistics. Paul (1880) had extensively discussed *analogy* as a psychological mechanism in language use and language change. We often produce new utterances in analogy to old ones. Children "regularize" their language by analogy (*go–goed*, like *cry–cried*) and language change is very much under the influence of analogical "proportion groups" in the minds of a language community's speakers (*ride: rode: ridden = rise: rose: risen = shrive: shrove: shriven = stride: strode: stridden = write: wrote: written*, etc.). The question is what a language user is inclined to draw into such a proportion group. One mechanism is association: *hide* and *ride* are rhyme associates, as are *hidden* and *ridden*. This may exert pressure to change *hid* to *hode*, like *ride–rode*. But an important countervailing power, according to Paul, is the strength of the memory trace; *hid* is a frequent, well-established word form in a speaker's mind; it would not easily "give in." That is why children are much more vulnerable to analogical change. Their memory traces are still weak. Can such psychological mechanisms of association leading to analogical change be measured experimentally?

That was the question Albert Thumb (1865–1915) asked himself. Thumb was a specialist in modern Greek, who had studied with the Neogrammarian Brugmann in Leipzig, where he cannot have missed the interactions between Wundt and the Leipzig Neogrammarians, including August Leskien. Thumb was teaching in Freiburg during the last decade of the century, when he became friends with Karl Marbe (1869–1953), a psychologist in Oswald Külpe's Würzburg laboratory, 100 miles north of Freiburg. Marbe had a good background in linguistics as well. He had been a student of Hermann Paul in Freiburg before he opted for psychology, with Ebbinghaus as one of his teachers in Berlin. Thumb and Marbe joined forces in addressing Thumb's question. It became an early case of real psycholinguistic cooperation. Marbe designed and ran the experiments and Thumb evaluated them from a linguistic perspective. Their joint 87-page monograph on the project appeared in 1901 and became a classic. The 1978 edition contains an excellent, detailed historical introduction by David Murray.

Thumb opened the monograph with a chapter stating the problem. Analogical change, whether it concerns grammatical form (like in the hypothetical "hid" case) or adaptations in meaning-related words (such as *October* becoming *Octember* under the influence of *November*),[47] must be based on association between the two elements in the language

[46] Paul's 1880 book *Prinzipien der Sprachgeschichte* went through nine editions before 1975.

[47] Discussed by Thumb and Marbe (1901) on p. 57.

user's mind. Psychologists such as Trautscholdt, Cattell, and Scripture[48] had pioneered the experimental study of association. It was now to be applied to the study of analogical linguistic change.

The next short chapter criticized Wundt's taxonomy of associations. According to Thumb, Wundt tacitly assumes that word associations are always mediated by images of the word meanings involved. That, however, does not correspond to experience. The response word just appears without conscious mediation of meaning. It therefore makes no sense to classify associations according to their meaning, as Wundt did.

Then followed the two experimental chapters, written by Marbe. In most cases analogical change involves the association between same word-class items. Is it indeed the case that word associations tend to be within-word class? In a first experiment, Marbe used 60 words, 10 from 6 different word classes: relational nouns (e.g. *father, mother*), adjectives (e.g. *big, small*), personal pronouns (e.g., *I, you*), local adverbs (*where, where from*), temporal adverbs (e.g., *when, now*), and numerals (*one* to *ten*). Marbe would "call" these stimuli in quasi-random order, with additional fillers, pushing a stopwatch at each call and stopping it on the subject's response. The data correspond to linguistic reality: for all word classes same-class responses dominate—on average in 77% of the cases. But Marbe's data tables showed many other interesting facts, such as the tendency to respond in fixed pairs (such as *father*–"mother"), opposites for adjectives (*big*–"small"), the next number for a numeral (*three*–"four"). But one observation intrigued him in particular. When all four subjects produced the same response to a stimulus (as was, for instance, the case for *this one*–"that one" (*dieser*–"jener"), the response times were relatively short. To further explore this, Marbe ran another experiment with homogeneous materials: 80 infinitival verbs. There were eight subjects and Marbe could now for each stimulus word determine the response agreement, as Jastrow had done: "8" if all subjects produced the same response, "1" if they all produced different responses, and anything in between. Marbe then drew the curve, which would carve his name in stone, representing the relation between response time and response agreement or frequency (Fig. 5.10). According to "Marbe's law," the larger the association agreement in a population, the faster the association response.

Thumb completed the monograph with a discussion of the linguistic relevance of these findings. He argued, with a host of examples, that in the history of language change the strongly associated pairs, such as *father–mother, summer–winter, two–three*, indeed tend to become more similar in form. They are the same strong associations that occasionally turn up in slips of the tongue, such as *where is your father . . . er . . . mother?* Here, Thumb cited Hermann Paul, who had suggested that if a particular slip becomes very frequent in a language community, it may become an accepted change. Thumb then turned to the data from infinitival verbs. They form an exception in that the most frequent response words are nouns (52%), not verbs (42%). But opposites, such as *give–take*, are

[48] Scripture's 1892 dissertation with Wundt was on subjects' free associations to visual, auditory, tactual, and taste stimuli. The only lingustic observation is that subjects occasionally produce word sound associations, such as *Mon*–"Mond," or derivations, such as *Raub*–"Räuber."

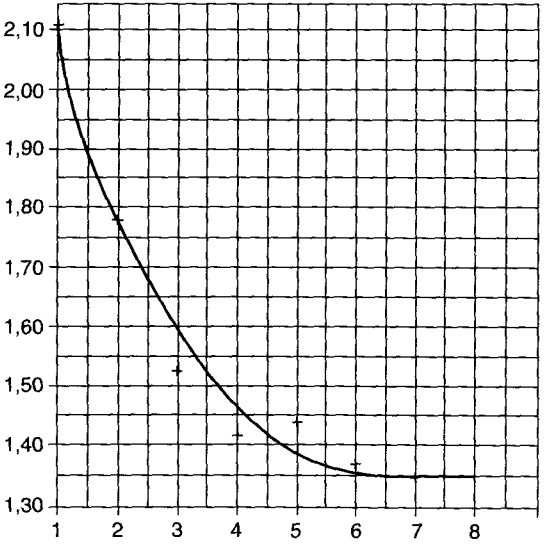


Fig. 5.10 "Marbe's law," the curvilinear relation between response agreement, i.e., number of subjects in agreement out of eight (horizontal axis) and association latency in seconds (vertical axis). From Thumb and Marbe (1901).

also preferred associations here. And indeed, they undergo analogical change in the history of languages. A further observation is that verb responses tend to be of the same "proportion group" as their verb stimuli. Weak verbs trigger weak verbs, strong verbs trigger verbs of the same conjugation class. They are the natural groupings for analogical change.

A main conclusion was that analogical change takes place between items that are strongly associated in the minds of many individuals in a language community. The more associative agreement exists in the community, i.e., the stronger the "community of ideas," the stronger the association in the individual's mind. Still, the handicap for this psychological approach was that you could not do the anachronistic experiment. How strong were the associations in the language users' minds when the analogical change took place? We could never know. Hence, "it is necessary to connect the psychological experiment with the study of living languages and in particular dialects."[49] By studying the "preferred associations" in a "living language community," one would be able to "ascertain, to what degree and range these associations are linguistically effective, i.e., leading to analogical innovations." However, much more experimental material had to be collected before we could know "the conditions under which current association *must* have linguistic effects."[50]

[49] "... dazu ist es nötig, das psychologische Experiment mit der Erforschung der lebenden Sprachen und insbesondere der Mundarten zu verbinden" (Thumb and Marbe 1901, p. 84).

[50] "Das weitere Problem unter welchen Bedingungen eine geläufige Assoziation sprachliche Wirkungen erzielen *muss* ... kann erst dann mit Erfolg bearbeitet werden, wenn einmal ein reiches Material sprachpsychologischer Experimente zu Gebote steht" (ibid. p. 86).

Wundt was, as usual, quick to react. In a short review of the monograph, he argued that Thumb and Marbe's experimental approach to language change was ill-formed. He said that if you want to study the "constellation of consciousness" which underlies analogical change, you would do better to study the change that took place than run A→B association experiments. The experimental "constellation of consciousness" is "as dissimilar as possible"[51] to the one prevailing at the moment of language change. Whereas in the latter a multitude of associations are, without any voluntary effort, unconsciously at work, in the former the speaker's attention is highly focused to purposefully produce a single conscious association. Here Wundt added that Meringer and Mayer, in their speech error research, had succeeded far better in matching what is unconsciously going on in the individual speaker's mind and in language change. It must have been a sore point for Thumb, who as a linguist had been (and kept) risking his neck to base a theory of language change on thorough experimental psychology. Here was the father of experimental psychology telling him that it was ill-conceived. It certainly cannot have helped his slumbering rivalry with Rudolph Meringer, to whom we will now turn.

Speech errors

Rudolf Meringer and Carl Mayer

In their excellent historical introduction to the 1978 facsimile edition of Meringer and Mayer's (1895) *Versprechen und Verlesen* ("Misspeaking and misreading"), Cutler and Fay state that Meringer's collection of speech errors is "modern." It was indeed, setting the standards for a century of speech error research. Rudolf Meringer (1859–1931, Fig. 5.11) was born in Vienna, and studied and taught philology there before accepting, in 1899, a professorship of Indo-European Linguistics at the University of Graz, where he stayed till the end of his productive life. Meringer hated arm-chair linguistics: "one who cannot observe is not a researcher, but a bookworm."[52] His main, lifelong research project was to study language change and language use in context. The only way to study etymology is to find out what words really refer to. "No history of words without history of things."[53] You would have to study the actual objects and entities for which names were coined, what kind of *plow* or *boat*, what kind of *attic*, what kind of *god*, etc.[54] In 1909 Meringer founded the journal *Wörter und Sachen* ("Words and Things"), which became the main platform of his movement. We will not discuss his life's work on Indo-European linguistics here, but rather turn our attention to the work he began in Vienna, together with Carl Mayer.

[51] "Ich möchte glauben, dass sie bei ihren Assoziationsexperimenten eine 'Konstellation des Bewusstseins' hergestellt haben, welche der bei den Analogiebildungen stattfindenden Konstellation so unähnlich wie möglich war" (Wundt 1901a, p.19).

[52] ". . . und wer nicht beobachten kann, ist kein Forscher, sondern ein Bücherwurm" (Meringer 1909, p. 597).

[53] "Ohne Sachwissenschaft keine Sprachwissenschaft" (ibid. p. 594).

[54] In the following I will continue to italicize linguistic materials, in particular speech errors.

Fig. 5.11 Rudolf Meringer. © Archives of Karl-Franzens-University Graz reproduced here with permission.

Meringer was a Humboldtian in that he considered language, in particular spoken language (his main interest), to be a process. "Linguistics is, in first instance, concerned with language in thought, a purely psychological event, which in brain physiology is called 'speech images' or 'inner speech.'"[55] It is Meringer's everlasting contribution to have opened a new, data-based window on the psychological processes of inner speech involved in the preparation of an utterance. The systematic collection and analysis of speech errors has become a major and very fruitful paradigm in the psychology of language.

An initial motivation for studying speech errors was Hermann Paul's suggestion that particular kinds of language change are caused by speakers making systematic speech errors. This, we saw, had Wundt's support. It would in particular work through so-called "dissimilations." These are cases where the phonetics of a word changes, in particular by loss or displacement of consonants or whole syllables. For instance Latin *semimestris* becoming *semestris*; here one of two adjacent, quite similar syllables got elided. Meringer and Mayer were careful to cite Paul (1880) on this point: "I know of no other explanation for these events than that they are based on repeated slips of the tongue, on which a substantial part of the speech community spontaneously converged."[56] And indeed, as Paul had argued, in tongue slips speech sounds get lost or displaced in similar ways. In particular, repeated or adjacent similar elements are vulnerable. That is, Paul observed, our trouble with tongue twisters.[57] But Paul's evidence was no more than anecdotal.

[55] "... [die Sprachwissenschaft] hat es in letzter Linie mit der gedachten Sprache zu thun, einem rein psychologischen Vorgange, mit dem, was die Hirnphysiologie 'Sprachvorstellungen' oder 'innerliche Sprache' heißt" (Meringer and Mayer 1895, p. 1).

[56] "Für diese Vorgänge weiß ich keine andere Erklärung, als daß sie auf wiederholtem Versprechen beruhen, worin ein bedeutender Teil der Sprachgenossen spontan zusammengetroffen ist" (ibid. p. 179).

[57] Such as "Der Kutscher putzt den Postkutschkasten" (ibid. p. 181).

Serious data-based linguistics requires a systematic, unbiased collection of spontaneous speech errors and Meringer set out to create such a collection. Others were on a similar track, although only on a very small scale.[58]

The way he proceeded was "modern," according to Cutler and Fay, for at least the following reasons. He recorded *any* speech error he could note down, given the circumstances, not only the odd or funny cases. He scrupulously wrote down the error immediately upon its occurrence, noting down contextual details and more: in many cases he collected the speaker's introspections on the event and its possible cause. His major error categories were basically the same as those in modern collections in which collection bias was minimized, as noted by Cutler and Fay: "anticipations, perseverations, exchanges or substitutions of sounds, syllables or words, with conflation of words, phrases or sentences."[59] But also his theoretical motivation in analyzing these errors was "modern." He quickly abandoned Paul's idea. Rather than conceiving of language change (or aspects thereof) as being *caused* by frequent speech errors in the community, he stated in his introduction to *Versprechen und Verlesen* "Speech errors and certain kinds of sound change are not inter-dependent, but have in common a higher cause which is to be found in the characteristics of the psychological language mechanism."[60] The explanatory principles of historical linguistics are to be sought "in the current speech functioning."[61] Speech errors are not pathological, but reveal the regularities of the system: "Only attention fails in a speech error, the machine runs without supervisor, is left to its own devices. And what makes speech errors instructive for linguistics, is the circumstance, that the clockwork's cover has been taken off in such moments and a view on the cogs is possible."[62] In other words, you can glimpse the inner speech mechanism at work. Speech errors, according to Meringer, are *regular*; "they must be determined by constant mental forces and thus they become a research domain for natural and language scientists, who from them can expect elucidation of the speech mechanisms."[63] That, indeed, has ever since been the motivation for speech error research.

[58] See Delbrück (1886) analyzing amnestic speech errors, and Grammont (1895) on dissimilation in Indo-European languages with some reference to tongue slips, which received a fairly positive review from Meringer (1897).

[59] Meringer and Mayer (1978), Cutler and Fay, (Introduction, p. xvi).

[60] Cutler and Fay's translation of "Versprechen und einige Arten des Lautwandels sind nicht von einander abhängig, sondern haben eine gemeinsame höhere Ursache, die in der Anlage des psychischen Sprechorganismus liegt" (ibid. p. vii).

[61] "... man muß sie *suchen*, in der jetztigen Sprechtätigkeit suchen" (ibid. p. vi).

[62] "Beim Sprechfehler versagt nur die Aufmerksamkeit, die Machine läuft ohne Wächter, sich selbst überlassen. Und was den Sprechfehler für die Sprachwissenschaft lehrreich macht, ist der Umstand, daß das Uhrwerk in solchen Augenblicken des Mantels entkleidet scheint und ein Blick in die Räder möglich ist" (ibid. p. vii).

[63] "sie müssen durch konstante psychische Kräfte bedingt sein und so werden sie zu einem Untersuchungsgebiet für Naturforscher und Sprachforscher, die von ihnen Licht für den psychischen Sprechmechanismus erwarten dürfen" (ibid. p. 9).

What about co-author Carl Mayer (1862–1936)? In his (not their) foreword to the book, Meringer is very clear. Four years after he had started his systematic collection he discussed it with his medical friend in the Vienna psychiatry clinic, and asked him to also collect data and to check the proposed error rules. That, indeed, became Mayer's contribution. Though none of the writing was done by Mayer, Meringer was keen to have him "co-sign" for the essence of the content. It would signal to scientists that scientific observation was not their prerogative. Mayer participated in the main source of their observations, a regular lunchtime meeting in which the participants had agreed to stick to certain rules, such as one person speaking at a time and halting all conversation as soon as an error occurred, to allow for proper recording.[64] This was all voluntary cooperation. Cutler and Fay rejected ever wilder stories in the literature that Meringer ran his recordings "in Teutonic style" and made himself "very unpopular" among colleagues.[65]

Meringer continued to collect errors even after the book with Mayer had appeared, leading to his single-authored book *Aus dem Leben der Sprache* ("From the life of language"). This 1908 monograph is an even richer source than the original, both in terms of the sheer number of observations and in the subtlety of the theoretical analysis. It substantially enlarged the corpus (the two books together provide over 2500 slip observations). There is an entirely new chapter on children's slips plus an extensive appendix with speech diary data on five children, three of which were his own. The book further refined the theory. It imposed the same main typology of errors: exchanges, anticipations, (extraneous) intrusions, perseverations, and fusions (contaminations). There were subcategories not only on words, syllables[66] and speech sounds, but also on errors of gender, case, person, number, mood, tense, etc. And Meringer co-discovered (with Wells (1906), see p. 427) slips of phonological features, such as umlaut anticipations.[67] The analyses within these categories were detailed and linguistically sophisticated, all aimed at demonstrating that slips of the tongue conform to rules. "Chance is totally excluded in slips of the tongue, slipping is rule-based."[68]

On the basis of this rich data collection, Meringer formulated some of the basic slip regularities and provided a first theoretical account, which is still considered valid today, though much refined and qualified. It should suffice here to mention one pervasive regularity and the basic theoretical account. One of Meringer's most important observations was that for all three categories of exchange, anticipation, and perseveration, the erroneous element is functionally similar to the replaced element. For example, when *senile dementia*

[64] Meringer and Meyer (1895), pp. 11–13 and Meringer (1908, p. 5).

[65] Meringer and Meyer (1978), Cutler and Fay (Introduction, p. xxxiv).

[66] In modern corpora syllable movements are notably rare. Many of Meringer's syllable displacements are, in fact, morpheme errors, but not all. Here is one: "Sie Zeuge *Krax*ner, haben sie die *Has*e noch?" (from *Hasner* and *Kraxe*) (Meringer 1908, p. 19).

[67] Such as "lässen nämlich" for "lassen nämlich" (ibid. p.109).

[68] "Der Zufall ist beim Versprechen vollkommen ausgeschlossen, das Versprechen ist geregelt" (ibid. p. 3).

is erroneously pronounced as *denile sementia*,[69] two consonants are exchanged that are both word-initial. Their syllables, moreover, should both be stressed or both unstressed. Word-final consonants can also exchange[70], but it never occurs, according to Meringer, that a word-initial and a word-final consonant exchange. Similarly, stressed vowels replace stressed vowels.[71] In a word exchange, such as *Die Milo von Venus* the exchanged words are nouns in a similar function.

This similarity-of-roles principle works across the board. For instance, Meringer himself had contaminated the two names, *Zeifel* and *Herzig* to *Zeifig* (Meringer 1908, p. 62). Here *Zeif* remained word-initial and *ig* remained word-final. A contamination such as *Zeiherz* was out.

The core of Meringer's theoretical account is this:[72] speech errors are, essentially, errors of ordering. Elements, whether consonants, vowels, syllables, roots, words, prefixes, suffixes, or phrases, get into positions where they were not intended to be. When elements are displaced, they end up in functionally similar positions. During inner speech preparation, the elements to be ordered, but often also other elements, are *simultaneously* active, just under the "threshold of consciousness." For each target position an element from a functionally similar position can enter consciousness and intrude. It can be an earlier element (as in perseverations) or a later element (as in anticipations). See Figure 5.12 for Meringer's own example.

Target positions differ in *weight*, according to Meringer. Word onsets and root onsets have the highest intensity. When you are trying to remember a word, such as *Denmark*, these elements (*d* in this case) have the best chance of coming to mind.[73] Next down in weight are the stressed vowels, such as *e* and *a* in *Denmark*. Then follow the onsets of unstressed syllables (such as second *t* in *Staate*), followed by all other vowels and all other consonants. Heavily weighted speech sounds have easier access to consciousness and are therefore better intruders than light elements. This simple hierarchy thus predicts the frequency distribution of sound errors that Meringer had observed. It is another modern feature of his work.

There can also be additional semiconscious elements on the speaker's mind, such as a word just heard, something just thought of, a similar word, an associated word, etc. They may also, occasionally, intrude. Meringer was always careful, in the case of such "extraneous" intrusions, to ask the speaker what thought had been available when he made the error. One of his friends, for instance, replaced the term *Vorschein* in *zum Vorschein*

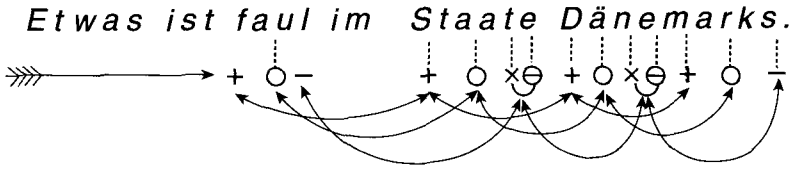
[69] Meringer's example: *Demile Semenz* (Meringer and Mayer 1895, p. 20).

[70] *Ich verganz gaß* for *Ich vergaß ganz.* (ibid. p. 23)

[71] *Granitpflaster* ("granite pavement") becoming *Granatpflaster* (ibid. p. 43).

[72] See especially ibid. p. 97.

[73] *En passant*, Meringer describes some of the core features of the tip-of-the-tongue state: consciousness of word or root onset, consciousness of the stress pattern (*Tonfall*), and hence the number of syllables of the target word. (ibid. p.160).



Etwas ist	f	a u	l	im	St	aa	te	D	ä	ne	m	ar	ks
	st, –	aa, –	te, –		D, f	ä,au	ne, l	m,St	ar,aa	ks,te	–, D	—, ä	–, ne
	D, –	ä, –	ne, –		m, –	ar, –	ks, –	—, f	–, au	—, l	–, st	—, aa	–, te
	m, –	ar, –	ks, –								—, f	—, au	—, l

Fig. 5.12 The top diagram shows possible exchanges of speech sounds in the last three content words of Meringer's example sentence *Etwas ist faul im Staate Dänemarks* ("Something is rotten in the state of Denmark"). They are exchanges between sounds in functionally equivalent positions. The bottom table shows, for each planning position in the sentence, all possibly competing sounds that are still active or already active. From Meringer and Mayer (1895, pp. 53, 164).

gekommen ("appeared") by the non-word *Vorschwein* ("fore-swine").[74] Meringer checked and got the speaker's introspection: he had been thinking of filthy scenes, which he had internally labeled as *Schweinereien* ("smut" or "swine's stuff"). The sound similarity (*schein, schwein*) facilitated the intrusion.

A special type of extraneous intrusion results from visual impressions. Meringer worked this out with a host of examples in his 1908 book. For instance, you misname a person because you see, or just saw, another person around. In a hilarious section, Meringer lists all the occasions when his colleagues mixed up people's names. More generally, Meringer developed the notion of "floating words" (*Wortvaganten* or *schwebende Wortbilder*).[75] In every situation, such as a lecture, a faculty meeting, a home, or some topical conversation, there are words floating around in consciousness that are specifically related to that situation. More generally, there is always a "cloud" of floating words around in the speaker's mind. These words may occasionally intrude, disrupting the intended utterance. Such intrusions can be facilitated by a sound correspondence to a target word. For instance, suppose there is a conversation about ducks (German: *Enten*). Somebody says *they are not good to duck* [*Enten*] . . . *to eat* [*Essen*].[76] Almost always, the speaker is able to reconstruct the meaning, and to spot the intruder.

Though the history of psycholinguistics has confirmed the essential correctness of Meringer's discoveries and theoretical analyses, Sigmund Freud (1901) was of the opinion that Meringer and Mayer had missed the point. In the opening statement of his paper,

[74] "[. . .] dann aber sind Thatsachen zum Vorschwein gekommen . . ." (Meringer and Mayer 1895, p. 62).

[75] Meringer (1908, p. 56 *ff*).

[76] "[. . .] die sind nicht gut zu enten . . . zu essen" (ibid. p. 57)

Freud generously conceded that, "exceptionally, I am in the position that I can acknowledge preliminary work" ("Vorarbeit"), Meringer and Mayer's study, "whose points of view lay far away from my own."[77] After a short summary of the authors' account of internal, within-speech slips, Freud stressed that an explanation from external intrusions would be more revealing. He acknowledged that the authors had recognized such external influences, citing, among others, their *Vorschwein* example. He then argued in two steps. First Freud claimed that there is some external influence in just about all cases of speech error: "I rarely find one in which the speech disturbance can only and exclusively be accounted for by . . . 'contact effects of speech sounds'."[78] Second he claimed that "*In a major set of substitutions slips of the tongue fully ignore such sound laws.*" (Freud's own emphasis).[79] These two claims were backed up with an increasing number of examples from Freud and his psychoanalytic colleagues in successive, book-form re-issues of the paper.

The two claims have different force. The former, the external causation of errors, can be correct under preservation of Meringer and Mayer's sound laws. Intrusions from their "floating words" always respect the laws, in particular the functional equivalence of target and intrusion and the facilitating role of sound similarity. This is, for instance, the case in Freud's example in which some gentleman addresses a lady on the street as follows: "If you allow me, my lady, I would like to accompany/insult [*begleit–digen*] you."[80] Freud suggested that the fear of "insulting" (*beleidigen*) was on the speaker's mind and slipped in to fuse with the target word *begleiten* ("accompany"). This is a normal case of "floating words" and an insertion respecting the rules: it is two verbs fusing, there is a sound similarity between them (*begleiten, beleidigen*) with all corresponding elements preserved (*be–ei–en*). The insertion is in the functionally correct place. The speaker does not say, for instance, "If you allow me, my lady, I would like to accompany [*bedeigliten*] you." The claim of external causation only invites the psychological interpreter to come up with *some* floating word or thought for every speech error. At this point, there were no limits for Freud in what could be postulated. As Meringer would later state, his "analyses are beyond good and evil."[81] Where Meringer, in almost all cases, could get the speaker's introspection of the floating word that had disrupted the speech, Freud and his colleagues would not leave it at that. The obvious "surface" floating word was rarely enough for them; there must be a more distant cause, requiring further interpretation. There had to

[77] "Ich befinde mich hier ausnahmsweise in der Lage, eine Vorarbeit würdigen zu können. Im Jahre 1895 haben Meringer und Mayer eine Studie über 'Versprechen und Verlesen' publiziert, deren Gesichtpunkte fernab von den meinigen liegen" (Freud 1901; I am citing from the 1954 edition, pp. 52–53).

[78] ". . . finde ich kaum eines bei dem ich die Sprechstörung einzig und allein auf das, was Wundt 'Kontaktwirkung der Laute' nennt, zurückführen müßte" (ibid. p. 59).

[79] "*In einer großen Reihe von Substitutionen wird beim Versprechen von solchen Lautgesetzen völlig abgesehen*" (Freud 1901/1954, p. 74).

[80] "Wenn Sie gestatten, mein Fräulein, möchte ich Sie begleit-digen" (ibid. p. 64).

[81] "Diese Analysen sind öfters jenseits von gut und böse" (Meringer 1908, p. 129).

be something suppressed from consciousness, forcing its way out. Here are two examples Freud approvingly cited.

1. Example from Tausk: There had been discussion about Jews. A Jewish speaker then says to a boy (*Jungen*): "go in the garden, Jew [*Jude*]—boy [*Jungen*]." The floating word was *Jew* and it replaced the sound related target word *Jungen*. This follows Meringer's rules. But this does not suffice for Freud. The interpretation proposed is that the speaker had disavowed his Jewish descent, but this pops out in the slip, because "the 'faith of the Fathers' does not let itself be disavowed without punishment."[82]
2. From Reik: A gentleman addresses a young woman, to offer his condolences on the recent death of her husband, saying: "You will find consolation in fully widowing [witwen] yourself to your children," instead of "fully consoling [*widmen*] yourself." This is a clear case of Meringer's intrusion from visual impressions; the speaker is watching the widow; sound similarity facilitates the intrusion. But that does not suffice for Freud: "the suppressed thought indicated a different kind of consolation: a beautiful young widow will soon enjoy new sexual pleasures."[83]

Freud's second claim is more radical: substitutions do *not* follow the sound rules; their mechanism (the popping out of a suppressed thought) is an entirely different one. Errors that are seemingly linguistic are in fact nothing of the sort. As a first example, Freud discussed Meringer and Mayer's noted slip: "*Es war mir auf der* Schwest . . . *Brust so schwer.*" (target: "It was for me on the breast so heavy"), for them a simple case of word onset anticipation. This does not satisfy Freud. There are various associations at work here: *Schwester–Bruder* ("sister–brother"), also *breast* of sister. They are the real cause of the error. Another example was when one of Freud's patients intended to say: "I snap together like a pocketknife" [*Ich klappe zusammen wie ein Taschenmesser*], but instead she said ". . . *wie ein Tassenmescher.*" This is, in Meringer and Mayer's analysis, a regular sound exchange. Not so for Freud: the patient had earlier made a jokey remark, saying "*Ernscht,*" in citing Freud's expression *Jetzt wird es Ernst* ("now it becomes serious"), in this way referring to the seriousness of getting married soon. In this way the patient had distorted the name Ernst, which happened to be the name of a firm in town dealing in contraceptives. The pocketknife had come up, because its shape referred to the posture of the fetus in the mother's womb. Etcetera. This conglomerate of factors caused the slip *Taschenmesser* (pp. 59–60). Hence "slips of the tongue fully ignores the sound laws." If that were indeed the case, one wonders why the patient did not say "*Isch klappe zusammen wie ein Tassenmescher,*" or "*Ich schlappe zusammen wie ein Tassenmescher,*" or "*Ich klappe zu-schammen wie ein Tassenmescher,*" or "*Ich klappe zusammen wie ein Schassenmescher*" and so on. Freud's explanation would predict all of them, but none of them follows Meringer's rules.

[82] ". . . daß der 'Glauben der Väter' sich nicht ungestrafft verleugnen läßt" (Freud 1901/1954, p. 84).

[83] "Sie werden Trost finden, indem Sie sich völlig Ihren Kindern *widwen.*" "Der unterdrückte Gedanke wies auf andersartigen Trost hin: eine junge schöne Witwe wird bald neue Sexualfreuden genießen" (ibid. p. 66).

Meringer was extremely irritated by all this. In his 1908 book he left it at a few critical remarks, concluding: "I most energetically protest against this use of my thoughts, which Mr. Freud has condescended to, against this after-work (*Nacharbeit*) to my 'preliminary work'(*Vorarbeit*)."[84] However, the 1919 revised edition of Freud's monograph triggered him to write an extensive review of the book.[85] The review is a masterpiece: sophisticated, hilarious, and devastating. This charge of quackery in science should be on the shelves of any psycholinguist (and any psychoanalyst).

Heath Bawden

There is one other extensive nineteenth century study of speech errors that must be mentioned. It is the dissertation by H. Heath Bawden (1871–1950), a student of John Dewey and George Herbert Mead at the University of Chicago. The dissertation was published in 1900 as a monograph supplement to the *Psychological Review*. It is a curious piece of work. Bawden diligently collected errors of speech and writing and other lapses from various sources, his own observations, columns in newspapers, even experiments. The latter were certainly an innovation. One of his methods, for instance, was for the subject to copy in writing a visually presented text, but under distracting conditions. The experimenter would be reading to the subject, or some set phrase had to be repeated loudly all the time. Alternatively, the subject would be reading aloud a text while the experimenter was loudly reading a "fascinating story." The monograph contains numerous tables of errors, but in many cases the origins of the errors are not reported. Surprisingly enough, there is no reference to Meringer and Mayer's book. Bawden's supervisors and the editors of *Psychological Review* (Cattell and Baldwin) must have had a joint lapse of attention. By some whim of fate, Meringer (1908), pleading for cross-linguistic research on speech errors and discussing "work by others" (such as Freud), also missed Bawden's monograph.

Bawden's theoretical approach was "psychological;" there is no linguistic sophistication whatsoever. The general cause of a lapse is "the breaking up of an habitual (familiar) association" (p. 72). This occurs in particular if attention is diverted. But as the error occurs, habit quickly reestablishes itself. That is why verbal lapses are mostly familiar words themselves, and there is a table showing examples, such as *conservation* lapsing to *conversation*, and *aghast* slipping to *amazed*, etc. (pp 78–79). This is probably the first observation of "lexical bias" in speech errors. Habit runs where attention lapses, as Baldwin had proposed. It is also "psychological" to relate speech errors to a "psychology of the ridiculous"; another table (pp. 117–119) presents a host of examples that are remarkably unfunny.

[84] "Gegen diese Verwertung meiner Gedanken, wie sie Herr Freud beliebt hat, gegen diese Nacharbeit zu meiner "Vorarbeit," protestiere ich auf das energischeste" (Meringer 1908, p. 131).

[85] Meringer (1923).

Retrospect

To conclude this chapter, the main point is that during the last few decades of the nineteenth century the major empirical paradigms for the study of language production and perception were developed. The invention of mental chronometry, the tachistoscopic study of naming and reading, the eye-tracking approach to reading, the presentation of recorded speech in speech perception experiments, the experimental study of free and restricted word association and of verbal memory, and last but not least, the systematic collection and analysis of speech and reading errors were to form the basic tool kit for a twentieth-century psychology of language. The term "psychology of language" had become generally accepted around the turn of the century, signaling the existence of a loose community of psychologists, linguists, physiologists, and philosophers whose aim it was to study the mind's dealing with language and speech. Only a few of them had the drive and opportunity to sketch a larger picture of language in the mind, but one stood out in pursuing just that: Wilhelm Wundt in Leipzig, to whom we will now turn.

Chapter 6

Wilhelm Wundt's grand synthesis

Centennial years have no special significance in the history of science; landmark events are not choosy. When Wilhelm Wundt (1832–1920, Fig. 6.1) published his two-volume *Die Sprache* ("Language") in 1900, he had not intended to draw a line under nineteenth-century psycholinguistics. During the 1890s Wundt had planned a major new project: a comprehensive, authoritative text on ethnic psychology. The publication would address the "higher" mental processes from the point of view of their cultural products, such as language, myth, and custom. The evolution of these complex cultural phenomena, according to Wundt, reflects the evolution of human consciousness. When Wundt published *Die Sprache*, the first two volumes of this project, he did not foresee that his *Völkerpsychologie* would eventually encompass ten volumes in total. The last volume appeared in 1920, the year of his death.

In retrospect, however, *Die Sprache* does consolidate the nineteenth-century psychology of language. It provides a grand theoretical synthesis of the field by comprehensively reviewing the major issues discussed in the previous four chapters. It is only natural, then, to complete this part of the book, on the early, mostly nineteenth-century roots of psycholinguistics, by considering Wundt's psychology of language and in particular *Die Sprache*, which monumentally marked the recent emergence of a new science.

After a biographical sketch, I will present a short outline of Wundt's psychology, which is indispensable for appreciating his psychology of language. We will then consider the major contributions of his psycholinguistics: expressive movements, sign language, speech sounds, words—their psychophysiology, morphology, and meaning—followed by syntax and sentence production. We come full circle with a discussion of the psychological origins of language, and conclude with Wundt's psycholinguistic legacy.

A productive life

Wilhelm Maximilian Wundt was born in Neckerau near Mannheim in his father's parsonage. The youngest of four children, he grew up as an only child; his two sisters had died and his eldest brother lived with an aunt in Heidelberg so he could go to school there. Wilhelm was educated by his mother and a deputy vicar who assisted his father, who was in poor health. In his (1920) autobiography Wundt remembers loneliness, which increased when he left his parental home to go to a grammar school in Baden. According to his own account, his feeling of solitariness caused him to habitually close off entirely and to lose himself in daydreams, "plays of fantasy," which became ardent, or in his words "passionate." Whether these early experiences prepared him for a life of

Fig. 6.1 Wilhelm Wundt (1832–1920).

concentrated, lonely writing we do not know, but one of his later colleagues in Helmholtz's laboratory observed: "Wundt sat the whole year unfailingly at some books in his own corner, not paying attention to anyone and not saying a word to anyone. I did not once hear his voice."[1]

The family moved to Heidelberg when Wilhelm was 13 years old. After he completed grammar school there, he moved to Tübingen to study medicine. There, the most important thing he learned was brain anatomy, but after a year he realized that medicine was not his vocation. Becoming a physiologist was now his ambition and so he moved on to Heidelberg University. Because of the very limited means his widowed mother could supply, he had to put himself on a strict regime of study and by doing so completed his medical studies within three years (in 1855). He had some excellent teachers in Heidelberg, among them the famous chemist Robert Bunsen, who fostered Wundt's interest in experimentation. To obtain his MD, Wundt served a clinical internship at Heidelberg's women's clinic, where he produced a thesis on nerve infections. After that he was well on his way to becoming a physiologist. He moved to Berlin to spend a semester with the great physiologists Johannes Müller and Emil Du Bois-Reymond. Returning to Heidelberg, Wundt completed his second doctoral degree (*Habilitation*) at the young age of 25. Shortly after that, he was almost killed by a sudden attack of tuberculosis, which took more than a year to overcome. As soon as he was back on his feet he was offered a teaching assistantship by Hermann von Helmholtz, who had just been appointed professor of physiology at Heidelberg.

This became Wundt's first academic position, from which he resigned, however, in 1864. He needed to fully concentrate, as an extra-ordinary (*ausserordentlicher*) professor, on his own experimental research, teaching, and especially writing. And write he did. During his years with Helmholtz he did not assist Helmholtz in his research; he held, after

[1] Sechenov, cited by Diamond (2001, p.28).

all, a teaching assistantship. Rather, Wundt used every available moment to study motor and sensory physiology, Herbertian psychology, ethnic psychology, epistemology, and more. This wide-ranging self-tuition initiated Wundt's life-long practice of writing what he taught and teaching what he wrote, building up his encyclopedic knowledge, which would become legendary. He published paper after paper, book after book, on a broad spectrum of subjects. During his 18 years at Heidelberg, till 1875, he wrote books on motor movement, comparative psychology, human physiology (with second and third completely revised editions), and medical physics. But it was one book in particular that grabbed the imagination, his *Grundzüge der physiologische Psychologie* ("Principles of physiological psychology") of 1874. It was the first comprehensive treatise on experimental psychology and it essentially defined a new discipline. William James, ten years Wundt's junior, was very impressed by this work and its author. James's 1875 review of the book was a song of praise.[2] "To be Helmholtz's colleague as professor of physiology at Heidelberg; to spend years in a laboratory and to publish numerous elaborate experimental researches; to write a large treatise on Physics, and an admirable handbook of Physiology (both of which have had several editions and been translated into French), besides two volumes of lectures on Psychology, an essay on the law of causation, and various fugitive articles: to study each new subject by giving a year's course of lectures upon it – these are *preparations* on a scale rather fitted to cool than to excite the ardor of an American neophyte in philosophy." (In later life James' awe would turn into irritation and ridicule; the chemistry between the two great men was less than optimal). Wundt continued to update and expand the *Principles* till the final three-volume sixth edition of 1911.

In 1875 Wundt was offered and accepted a chair in "inductive philosophy" at Zürich, which was, within a year, followed by the much more attractive offer of a philosophy professorship at Leipzig University. It was Germany's largest university at the time, with some 3000 students. Wundt moved the same year and would stay there for the rest of his academic life. He taught his last course in 1917, at 84 years of age. Over the years, Wundt's teaching became the most celebrated at Leipzig University. Not only did he cover a wide variety of subjects, ranging from physiological and comparative psychology to neurology, anthropology, philosophy of language, logic, methodology, cosmology, and history of philosophy, but he also taught them to perfection. He lectured in the university's largest auditorium, which was usually crowded with some 300 students, almost one-tenth of the university population. The tall and slender Wundt, clad in black, would deliver his lecture in "an easy and abundant bass, somewhat toneless, at times a little barking," without reference to the notes he always took along. His voice carried well, and there was "certain persuasiveness, a sort of fervour, in the delivery that holds your interest and prevents any feeling of monotony," Titchener remembered.[3] Others remembered his chiseled language and well-prepared experimental demonstrations.[4]

[2] James (1875/1980, pp. 115–116).

[3] Baldwin (1920/1980, p. 288).

[4] See Baldwin's (1920/1980) collection of personal memories.

From 1879 onwards, rooms were established where Wundt's doctoral students could perform psychological experiments. In an 1880 letter of recommendation for Stanley Hall, Wundt called it a "psychophysical laboratory." Over time the laboratory expanded and eventually moved to larger premises in 1897. History has recognized it as the very first academic psychological laboratory.[5] It became highly influential. From 1875 to 1919 Wundt supervised more than 186 doctoral theses.[6] Most of these were based on experimental work performed in the laboratory, such as the studies, reviewed in the previous chapter, by Trautschold and Cattell. Fifty students came from abroad, among them 14 Americans, 10 Britons, and 19 eastern Europeans. Many of these students went on to establish psychological laboratories of their own, both in Germany and abroad, such as Külpe in Würzburg and Titchener in Philadelphia.

In the afternoons Wundt would do his daily round of the laboratory, checking in with his students, often in great detail, and consulting with his assistants. Reports agree that Wundt took meticulous care with the texts students submitted. This editing, as well as his own personal writing, Wundt did at his home during the mornings. Much of the laboratory's work was published in *Philosophische Studien*, a series rather than a regular journal, which Wundt began in 1883 and continued to edit thereafter. Wundt hardly ever conducted an experiment himself; he only supervised others. I found only one first-authored experimental paper in *Philosophische Studien*, which was an acoustic study of a pigeon without a labyrinth in its ear. Although Wundt displayed the usual formality of a German professor, he was held in high esteem by most of his students. They mention his "free-and-easy intercourse" in the laboratory, his general kindness, the Sunday evening dinners in his home and his personal care in shepherding them through exams.[7] A few of his students, such as James McKeen Cattell and Emil Kraepelin, developed cordial relations with Wundt, never quite symmetrical, but definitely mutual.

As we will see, for Wundt psychology exists as much in ethnic as in experimental psychology. The psychology of language was, for him, largely ethnic psychology. From this perspective it is remarkable that hardly any of his PhD students worked in ethnic

[5] For a thorough historical analysis, see Bringmann *et al.* (1980).

[6] 186 according to Tinker (1932/1980). I noticed that two students are missing from Tinker's list: Ernst Meumann, who graduated in 1889 on a study of time and rhythm and who became one of Wundt's assistants and Wundt's only female student, Anna Berliner, whose dissertation on subjective and objective aspects of sense impressions appeared in 1914.

[7] See Baldwin (1920/1980) for many such reports. But I happen to possess a personal communication of a somewhat different kind. When I was 20, I spent five months in the laboratory of Albert Michotte in Louvain. He was 76 years old at the time (it was in 1958/59) and, somewhat like Wundt, daily spending a few hours in the laboratory to discuss work with his students (me included). Michotte had spent a post-doctoral period during 1905/06 in Wundt's laboratory. One day, Michotte told me about his experience there, and I wrote it down from memory the same day. Here is what I jotted down: "He told me that Wundt was so important that his courses were preceded by a small ceremony. When the auditorium was full, somewhere two doors were opened. Through these, two assistants would step in. After that Wundt followed; on his appearance all students had to stamp on the floor. Though Michotte studied there for a full year, he only spoke 3 times with Wundt. Everything was handled through the assistants."

psychology and none on the ethnic psychology of language. It is as if Wundt kept that enterprise entirely for himself.

At some moment during his stay in Leipzig, Cattell brought Wundt a typewriter, which he took to right away. Wundt became very attached to it and ever afterwards practiced his "typewriter sport," producing book after book. It was no doubt due to his daughter Eleonore that Wundt could keep going at his usual rate during the final years of his life, when his eyesight was deteriorating and typing became impossible. We would now call her his executive secretary, or even his co-author in many cases.

During his lifetime, Wundt published 18 "works," altogether an astonishing 38 volumes. Among them were the ten volumes of ethnic psychology. Wundt had the habit of systematically revising every new edition of his work, such as his popular *Grundriss der Psychologie* ("Principles of psychology"), which went through 14 editions during his lifetime. The ultimate testimony to Wundt's fabulous efficiency is his autobiography *Erkanntes und Erlebtes* ("Known and experienced"). Its preface is signed "August 1920," the very month Wundt passed away.[8]

Wundt's psychology

The 38 volumes of Wundt's works reflect his encyclopedic mind. He had an enviable memory and dealt comprehensively with any subject. The reader is spared no detail; Wundt certainly could have used a few pages less. But he was also highly systematic. His books are clearly structured and his writing was lucid and precise. We now consider to be a vice what in Wundt's literary Germany was definitely considered a virtue: the acrobatics of constructing sentences of high, indeed chiseled, complexity. William James' style has gloriously survived; Wilhelm Wundt's has not.

The present chapter is concerned with just three of these 38 volumes, the first volume of *Logik* ("Logic"), first published in 1880, and the two volumes of *Die Sprache*. They contain what Wundt had to say about language and thought. Reviewing Wundt's psychology is, of course, beyond the scope of this chapter, but a few features are indispensable for the understanding of his psycholinguistics. These will be reviewed briefly.

Experimental and ethnic psychology

Psychology, according to Wundt is the study of "immediate experience;" that is, the totality of our images, ideas (*Vorstellungen*) and the feelings, emotions, or affects that go with them. It is an empirical science, which uses two types of method. From the sciences it inherits the experimental approach. That is appropriate for the study of the senses, visual, auditory, tactile perception, and motor action. These are cases of "elementary" immediate experience. From the humanities it inherits the study of *products* of the mind, such as language, myths, customs, rites, and art. Careful historical and especially comparative

[8] Among the many secondary sources I used here I mention in particular Blumenthal (1970), Bringmann and Tweney (1980), Hall (1914), Meischner and Eschler (1979) and Lamberti (1995).

analysis of such products of the mind will reveal their genesis and the complex underlying mental processes that gave rise to them. This is the ethnic psychological approach.

The psychology of language can be experimental in only a limited way. It is applicable to the study of the perception of speech sounds, spoken or printed words, and of motor processes such as articulation and handwriting. But all else should come from ethnic psychology. Ethnic psychology should handle such complex processes as expressing judgments or predications by way of sentences. It will also be the preferred method for the study of language acquisition—what is it the child *produces* at a certain stages of development?

Association and apperception

Wundt's first thorough reading in psychology, while still in Heidelberg, was Herbart's as yet dominant theory of "mental mechanics." It was, as we have seen in Chapter 2, a mathematically formulated account of how ideas or images (*Vorstellungen*) move in and out of consciousness. Ideas mutually associate or repel, and they form conglomerates into which new ideas can be assimilated. The latter was called *apperception* by Herbart and, following him, by Steinthal. Apperception was not a mechanism *sui generis*, but an emerging property of the mass action of conglomerates of ideas. There are no forces at work other than the laws of association and the spreading of energy from idea to idea. Wundt accepted association as a basic mental mechanism. He was particularly inspired by John Locke's theory.[9] But for Wundt, *Vorstellungen* were themselves processes, as much as any other mental experience. He strongly rejected Herbart's "billiard ball mechanics," as it was sometimes called.

Wundt's real innovation, however, was his redefinition of apperception. He used the old term to indicate a mental mechanism that had not figured in earlier theories and that we still consider basic today. In modern terms it is referred to as "attentional" or "executive" control. The term "apperception" that Wundt used did not survive. One should, of course, be careful in extrapolating historical concepts to modern theories. Still, Wundt's notion of apperception certainly foreshadows the modern view of executive control. Let us cite Wundt: "The state, characterized by a specific sensation, which accompanies the clearer focusing of some mental content, we call *attention*; the specific process by which some mental content is brought into clear focus [we call] apperception."[10]

Whatever is in consciousness at some moment, Wundt calls the "field of view" (*Blickfeld*). This is not limited to visual perception. What is specifically attended-to by active apperception he calls the "point of view" (*Blickpunkt*).[11] Whereas associative

[9] In his courses referring to him as "Schon Locker" (Baldwin 1920/1980, p. 292).

[10] "Den durch eigentümliche Gefühle charakterisierten Zustand, der die klarere Auffassung eines psychischen Inhalts begleitet, nennen wir die Aufmerksamkeit; den einzelnen Vorgang, durch den irgendein psychischer Inhalt zu klarer Auffassung gebracht wird, die Apperzeption" (Wundt 1914, p. 252).

[11] "Die Inhalte, denen die Aufmerksamkeit zugewandt ist, bezeichnen wir, nach Analogie des äußeren optischen Blickpunktes, als den Blickpunkt des Bewußtseins oder den inneren Blickpunkt, die

connections arise passively, establishing apperceptive connections involves a "sensation of activity" (*Tätigkeitsgefühl*), they are "experienced as being established under attentional involvement."[12] As we will see, apperceptive focusing is involved in the speaker's incremental "unpacking" of the global notion he is to express. This apperceptive focusing is, according to Wundt, a case of volitional activity.

Voluntarism

Whereas Herbart saw the growth of mind as an ever expanding associative conglomerate, reflex psychologists such as Lotze saw a role for volition. According to Lotze, initially all action and movement is reflexive. Nevertheless there is the immediate sensation of these movements and an immediate perception of their outcomes. This causes an increasing awareness of the relation of some motor action to its outcome. Eventually this makes it possible for the subject to consciously execute the movement in order to produce the outcome.

Wundt rejected this view. Instead, Wundt proposed that the origin of volition is not to be sought in reflexes, but in "drive action" (*Triebhandlung*). The simplest drive actions are the instinctive behaviors of animals (and man), such as procreation and nourishment. They are triggered by internal or external stimuli and are always preceded by feelings/sensations that can be called their "motives." The outcome of the drive action is the resolution of these feelings. In humans, volitional acts are usually more complicated. There tends to be a battle of motives, a variety of emotions and images that develop into "affects." To relieve these, the subject must make a choice of action, a voluntary act. Wundt does not push the notion of free will here; purely rational decision making does not exist for him. All decision making is in essence affective. In ontogenesis the need for immediate overt action diminishes as the possibilities of *internal* decision making develop. Apperceptive focusing of competing simultaneous motives at work can diminish their affects without causing immediate overt action. This may, for instance, lead to a *delayed* sequence of actions, each satisfying a single motive. This is executive control in volitional action.

If such an apperceptive volitional action is repeated often, it may itself become a drive action, i.e., without further conflict of motives, and ultimately an automatic reflex action or movement. The result of practice is that less or no attention is required for a functional action. There is, for instance, much in the action of speaking that has become fully automatic. Inherited reflexes, such as the sucking reflex in neonates, also evolved from original drive actions, such as searching and finding food, but in this case over evolutionary time. To summarize, it is not reflexes that are primary, but drives. This view clearly reflects Darwin's position.[13]

Gesamtheit der in einem gegebenen Moment vorhandenen Inhalte dagegen als das Blickfeld des Bewußtseins oder das innere Blickfeld" (Wundt 1914, p. 252).

[12] ". . . daher die Verbindungen selbst unmittelbar als unter der Mitwirkung der Aufmerksamkeit zustande kommend aufgefaßt werden" (ibid. p. 307).

[13] "Moreover, an action repeatedly performed by us will at last be done without deliberation or hesitation, and can then hardly be distinguished from an instinct" (Darwin 1871, Vol. I, p. 88).

The feelings and affects preceding drive and volitional action often cause expressive movements of face, voice, posture, or hands. It is from these primary expressive movements that languages emerge, according to Wundt. Hence, Chapter 1 of *Die Sprache* deals with expressive movements.

Expressive movements

Language, whether spoken, gestural, or written, is an expressive movement, which can serve the intention of communicating thoughts, feelings, and inner states, or to assist tacit thinking. Wundt begins his chapter on expressive movements with an extensive analysis of the feelings (*Gefühle*) that underlie all action. They vary along three main dimensions: pleasant–unpleasant (*Lust–Unlust*), straining–relaxing (*erregend–beruhigend*) and exciting-quiescent (*spannend–lösend*). Figure 6.2 is Wundt's own rendering of this space of feelings.[14]

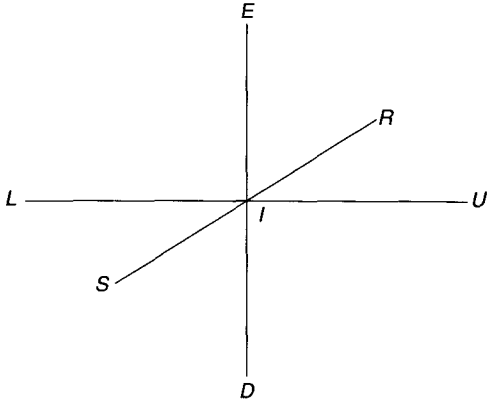


Fig. 6.2 Wundt's three-dimensional space of feelings (1900a, Vol. I), see text.

It is not hard to see that this representation corresponds almost exactly to the three-dimensional representation of "affective meaning," with "evaluation" (pleasant–unpleasant), "potency" (straining–relaxing), and "activity" (excitement–quiescent) as the main dimensions, which Osgood and co-workers proposed more than half a century later.[15] Both Wundt and Osgood recognized that the dimension of pleasantness or evaluation is lexically the most expressive in language; we have a rich vocabulary to denote states of pleasure and an even richer one for states of displeasure. But it is, as we will see, the second, potency dimension which is, according to Wundt, most expressive in the prosody of speech.

For Wundt, an affect is a temporal course of feelings, some trajectory through his three-dimensional space. Feelings are never static, but merely moments in some developing affect. The course of an affect can, to some extent, be studied in the laboratory, as Wundt's student Lehmann had demonstrated by measuring changes in heart beat, blood pressure,

[14] Wundt (1900a), Vol. II, p. 38. The meanings of *D* and *R* are not explained by Wundt.

[15] As recognized by Osgood himself in Osgood *et al.* (1975, p. 394).

and breathing patterns. The evident autonomous causes of such changes suggested to Wundt that there is some "central organ of feelings" in our brain. It represents and regulates the direct relations we entertain to the contents of our consciousness, to the objects of our actions. Wundt makes the interesting move here of stating that this is essentially the very function of apperception: the volitional, affect-driven focusing of attention on relevant content. The central organ of feelings, Wundt claims, is nothing other than the "center of apperception." Wundt's best guess as to the localization of this center was the prefrontal region (*Stirnregion*) of the brain; not a bad guess given our current knowledge about limbic and executive cortical regions.[16]

The specific character of expressive movements depends on two major factors. The first is the quality of the underlying feelings, while the second is the content of the ideas that accompany these feelings. Feeling quality varies according to the three dimensions of feeling space. The potency dimension has a special status here. It varies with the tonic state of the entire vaso-muscular system. Heart rate, blood pressure, and breathing intensity all vary with the potency or intensity of feelings. With increasing intensity, ever larger muscular groups become excited, from small facial mimics to larger pantomimics and complete locomotion. With increasing fear, for instance, the whole scale of movements from widening of the eyes to rushing away can result. All expressive movements, including speech, vary on the potency dimension.

The two other dimensions of feeling involve more specific expression of the facial musculature. The pleasure–displeasure dimension primarily affects the musculature around the three sensory systems of eyes, nose, and mouth. They tend to "open up" in pleasure and to close down in displeasure. This happens, for instance, in laughing versus crying. In pleasure, the mouth musculature responds somewhat as it does in tasting sweetness; in displeasure it responds as it does in tasting bitterness. The exciting–quiescent dimension (Osgood's "activity") finds its expression in the cheek musculature. This dimension involves enduring states of feeling, such as contentment or, at higher intensity, haughtiness. The involvement of the cheek musculature may have its origin in primary feeding reflexes in states of hunger versus satiation.

The second factor, the content of consciousness, finds its primary expression in pantomimics. Here it is the relation to objects and actions in the outer world that matters. There are two basic types of expressive movement here, which will concern us as basic ingredients of linguistic communication. The first is the class of pointing or deictic hand and arm gestures. According to Wundt, these are derived from grasping movements, as is still observable in ontogenesis. Children begin by grasping an object that is within reach and then try to grab an object out of reach. They make the same grasping movement, or a lighter variant of it, to express their wish to obtain an out-of-reach object. Finally they reach a state where they can point to some object that is absent from view, to an imagined object. This ontogeny, according to Wundt, is only observed in humans and not at all

[16] In particular the anterior cingulate cortex and its involvement in attention and executive function.

or only minimally in apes, because only humans have the drive to communicate their inner states.

The second type of expressive movement is imitating movements. We share these with many social animals, who run together, flee together, etc. The perceived movement engenders the corresponding drive and affect in the perceiver, who then executes the same movement.[17] Both basic types of expressive movement figure in the sign languages of deaf people and other signing communities, to which we will turn now.

Sign language

Types of sign language

Sign languages arise in different communities. In his extensive chapter on gestural language, Wundt discussed all that was available in the literature. This included an extensive literature on Deaf sign languages, two publications on American-Indian sign languages,[18] some literature on the signing of Cistercian monks (available since Leibniz), and descriptions of Neapolitan signing. These signing systems differ, according to Wundt, in their degree of conventionalization and in their relationships with spoken/written language. The "spontaneous" sign languages of deaf people are little conventionalized, as opposed to the signing systems of the Rocky Mountain and Dakota Indians. The American-Indians (it was believed) needed their sign languages not only for silent communication at far distances, but also to compensate for the substantial dialectal variation in their spoken language. Conventionalization was also quite apparent in Neapolitan signing, according to Wundt, but so was its inter-relatedness with (in fact complementariness to) spoken language. There was also some tradition in the monks' systems, often with specific relationships to written language. The core of Wundt's discussion focused on Deaf sign languages, which were, in his opinion, often (although not always) non-conventional and also independent of spoken language. That makes them of special relevance to the psychological study of language and its origins. It is in Deaf sign language that we can still see the origins of language at work.

When deaf people who cannot speak (*Taubstumme*) come to live together, a *natural* sign language arises. When the community becomes more permanent, as happens in institutes for deaf people, some tradition is certainly acquired, but the language remains "natural" in two respects that keep sign languages distinct from spoken languages. There is first a continuing creation of new signs. Secondly, there is a high level of iconicity in both conventional and newly created signs. This marked iconicity makes Deaf sign languages mutually understandable, so that one can speak of the "universality of sign language."[19] This holds at least for the domain of universally shared concepts, such as *man*, *animal*, and *water*, according to Wundt. In other words, Deaf sign languages are like dialects, but without common historical roots. Their commonality is purely psychological

[17] Wundt's treatment of this mechanism in Wundt (1990a, Vol. I), p. 127, got a second life in Rizzolatti's notion of mirror neurons.

[18] Mallery (1879–1880), zu Wied (1841).

[19] Wundt (1900a), Vol. I, p. 135.

in nature. "The notion of an original language, a purely hypothetical borderline notion in the domain of spoken language, becomes an immediately observable reality in [deaf] sign language."[20] This very fact shows the "necessity" that, "for any kind of naturally emerged language, there must have been a time, where the relation between sign and what it denotes, was a directly iconic one."[21] In all cases this was based on the drive to communicate and to make oneself understood. The original means of expression were, precisely, pointing and imitating gestures.

Pointing, imitating, and abstract signs

The pointing gestures in sign languages are not just deictic, indicating objects or interlocutors. They can refer to properties, such as "red" by pointing to the lips, to sensory acts, such as pointing from eye to an object, etc. The imitating gestures are of two kinds, according to Wundt. The absolutely dominant type in Deaf sign language is the "drawing" gesture, sketching the object's contour, as in drawing the outline of a house. A frequent special case here is the *pars-pro-toto* sign, for instance referring to a donkey by drawing its two ears. The other kind, frequent in Neapolitan and monk signing, is the "plastic" sign. Here the hands are shaped in the form of the referent. Making a fist, with extended lifted thumb, for instance, is the Neapolitan way of referring to a bottle, with body and neck.

From these two primary and transparent kinds of gesture more abstract symbolic gestures can develop, which are usually the result of conventionalization in a signing community. Here the sign is a symbol for some concept, an indirect non-iconic symbol. The "donkey" sign may lose its reference to a donkey and become the symbol for a fool. A sign of a hand with upwards-pointing index finger often comes to indicate the concept of attention; combined with the facial sign of pressed lips it denotes the concept of silence. A "sowing" hand sign may become the symbol for salt, and so on. These are all cases of meaning change over time, which is a major cause of ambiguities in sign languages (as it is in spoken languages).

Grammatical categories and sign syntax

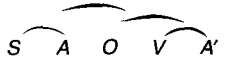
Wundt then addresses the dominant belief in (German) linguistic circles that Deaf sign language has no grammatical categories, i.e., no parts of speech. Steinthal, for example, had reached this conclusion based on the absence of inflections and particles in sign language. But this is misguided, according to Wundt. Sign languages have three *logical* categories of signs: for objects/entities/substance (*Gegenstand*), for properties, and for states/events/actions. These three categories are not marked on the sign, but only "thought" (*mitgedacht*) when the sign is used. But there are no other logical categories or

[20] "... der Begriff einer Ursprache, im Gebiet der Lautsprache ein hypothetischer Grenzbegriff, wird bei der Gebärdensprache zur unmittelbar beobachteten Wirklichkeit" (Wundt (1900a), Vol. I, p. 150).

[21] "... die Notwendigkeit nämlich, daß es für jede Art natürlich entstandener Sprache einmal eine Zeit gegeben haben muß, in der die Beziehung zwischen dem Zeichen und dem, was es bezeichnet, eine unmittelbar anschauliche war" (ibid. p. 150).

parts of speech in sign language; prepositions, conjunctions, and adverbials are "completely lacking."[22] Still, spatial relations among objects and properties of actions are often vividly sketched. By means of a pointing finger it can be suggested that the (signed) cat is on the (signed) roof. The sign for walking can be made with high or low speed to depict a quality of the action, etc. The logical category of a sign is rather flexible. Pointing at a tooth can as easily mean "tooth" (object) as "white" (property). The coffee-grinding gesture can mean "grinding coffee," "drinking coffee" (actions), or just "coffee" (substance). For Wundt, these are all examples of the spontaneity and naturalness of expression in sign language. Whatever category is to be "thought" in a particular use of the sign can usually be understood from the ordering of the signs. That brings us to issue of sign syntax.

Because of the absence of inflexions and particles and the frequently missing verb sign, Steinthal had concluded that sign language has "no sentence and hence no grammar." This, however, is a misconception, according to Wundt. In spoken language there is also frequent use of verbless sentences. Moreover, sign language, like many spoken languages, makes full use of word order as a syntactic means. Wundt turned to a phrase-structural description of basic sign syntax. It is important to mention here that Wundt was the inventor of syntactic phrase diagrams. He introduced them in his *Logic* of 1880,[23] and fully developed them in *Die Sprache*. We will return to them in our discussion of sentence production, but Wundt's representation of basic sign syntax is as follows.[24]



It depicts the structure of a signed sentence such as MAN FURIOUS CHILD HIT HARD. The *SOV* (subject–object–verb) ordering is dominant in Deaf sign languages (at least the German, French, and British varieties thereof). In addition, property signs (*A*, *A'*) follow the signs they modify. Adverbial modification (as in "walking quickly") is often only "depicted," not syntactically expressed; the speed of signing WALK is fully simultaneous with the sign. But where a verb sign is explicitly modified by a property sign, the latter will follow the verb. Hence we have MAN FURIOUS (*S–A*) and HIT HARD (*V–A'*). There is also an explicit genitive ordering, the juxtaposition of two entity signs, with possessor first, as in CHURCH TOWER ("the tower of the church"). This is in no way conventional, but a fully spontaneous ordering.

This basic syntax[25] is the result of universal psychological factors. They involve principles of logical, temporal, and (iconic) spatial dependency. The logical priority of the

[22] Wundt (1900a), Vol. I, p. 188.

[23] Wundt (1880, Vol. II, pp. 53–54).

[24] Wundt (1900a), Vol. I, p. 207.

[25] The spontaneous *S–O–V* ordering described by Wundt was interestingly confirmed by Goldin-Meadow *et al.* (2008), who had speakers of different (spoken) languages *gesture* some event. They all resorted to an actor-patient-act ordering, whatever their spoken syntax. There is also a similarity here to the "basic variety" discovered by Klein and Perdue (1992). It is the syntax arising in the untutored acquisition of a second language, independent of L1 and of L2 syntax.

subject usually puts it in first position, as in spoken languages. An important temporal factor is the laboriousness of gestural movements. The evanescence of the images to be expressed causes the signer to construct very short sentences. Even a sentence such as "the furious man hit the child" easily becomes a sequence of two signed sentences: MAN FURIOUS, CHILD HIT, transliterated (by Wundt) as: "the man the furious one the child he hit."[26] For the same reason, coordination and subordination are completely absent in Deaf sign language. In that connection, the major temporal principle is the following of "natural order." Where spoken languages use subordination, with connectors such as "before" and "after," to describe successive events, Deaf signers arrange simple sentences in the order of events. More generally, the ordering of signs follows the ordering of apperception: as attention focuses on successive images, the expressive signing follows the same order. In modification, expressing a property of some object or event, the sign for the independent item precedes the sign of the dependent item. And finally, affect-loaded images tend to be expressed first. This need not upset the basic order; the sentence can be partitioned into smaller ones (as in CHILD WAS HIT and MAN WAS FURIOUS, where the poor affect-raising child ends up in the subject position of the first sentence).

No match to spoken languages, but a window on the origins of language

Wundt concludes that Deaf sign language is a natural development of human expressive movements. Initially, there is no more than the immediate motor expression of some affect, a pure "drive action" (*Triebshandlung*). But if the affect combines with some image (*Vorstellung*), the movement can also become expressive of the image, first only iconically, later also symbolically. Such expression becomes volitional, due to the specifically human drive or urge to communicate and to share with others affect and inner states. Any volitional act has a motive; in sign language the motive is to evoke the same images and affects in others. With the first response gestures, a real exchange of motives arises, which is the "real birth moment of sign language."[27] This is Wundt's genetic view of the human mind. Each stage is developed from the previous one, although it seems to be an entirely new creation. This is Wundt's principle of "creative synthesis" (*schöpferische Synthese*).

Wundt's views on Deaf sign language are themselves a creative synthesis of the German, French, and British perspectives (as sketched in Chapter 4). Like the French, he considered sign language a naturally emerging and rich system of symbolic and grammatical communication. But he did not share their belief that it could match the expressive power of spoken languages, let alone that it would be the clearer, more distinct kind of language. He was pessimistic about the French efforts to add new, abstract signs to the language, to express adverbials such as *still* (*noch*), conjunctions such as *when*, and abstract verbs such

[26] "... der Mann der zornige das Kind er schlug." Wundt (1900a), Vol. I, p. 214.

[27] "... der eigentliche Geburtsmoment der Geberdensprache" (ibid. p. 241).

as "*to be*" etc. They are completely unnatural to sign language, are extremely hard to learn for the signer and are quickly forgotten. Here Wundt shared the dominant German view that, with respect to abstraction, there is a glass ceiling that cannot be traversed by sign languages. Despite this, he strongly rejected the German practice of harshly suppressing the use of sign language in institutions for deaf people or at home. He preferred the more pragmatic British approach of accepting and conserving the use of sign language by Deaf children. We now know that the French perspective on sign language as a potentially fully-fledged natural language has stood the test of time. But, in Wundt's defense, we know that the available evidence at that time was insufficient to decide the matter either way.

The underlying motive for Wundt to develop his theory of sign language as a naturally and spontaneously emerging system of communication was to provide an answer to the question of the origins of language. Since Herder and Rousseau, the search had been for a psychological explanation of these origins. It had been Steinthal's life's work to provide such a theory. Wundt felt he could improve on that effort. As we will see, sign language was going to play a crucial role in his account of language origins.

Speech sounds

Volume I of *Die Sprache* contains two chapters, a total of 180 pages, on speech sounds. The first chapter concerns their acquisition by the child and their use in sound symbolism. The second deals for the most part with the classical issue of historical sound change from a psychological perspective. There is a literature review, but also a presentation of new insights, which are outlined next.

Evolution of vocal expression

Our speech sounds are, and were in origin, expressive movements. But how did they originate? Comprehensiveness is Wundt's trademark, which leads him to begin by analyzing bird song. Here pitch modulation is the basic characteristic. Birdsong had already become a prominent evolutionary model of sexual selection. Wundt wanted to know how applicable this model was to the human case. The answer is far less than Darwin had suggested, according to him. Wundt criticized Darwin's suggestion that our speech originated from song through sexual selection. Here, Darwin had argued from monkey and songbird evolution. Darwin's two arguments for the sexual selection of song in humans are contradictory, according to Wundt. One of Darwin's arguments is that male monkeys (like male birds) tend to have more developed voices than female ones. That is what the female animals attend to in their sexual selection. Another is that women have more agreeable voices than men, and therefore women required men to put an effort in expressing themselves more musically. So, Wundt asked, why is it that women, but not female monkeys, started out having a better voice? In addition, but no less important, Wundt argued, the prosody of human speech is *not* musical. Song, in both birds and humans, requires the use of fixed pitch intervals and some regular, fixed rhythm. Speech, however, shows neither characteristic. Wundt disliked the song theory, which had been

present in Herder, Spencer, Darwin, Donovan and Jespersen.[28] Human song is an art form, with an independent evolutionary history, according to Wundt.

Children's acquisition of sound patterns

Turning next to the child's acquisition of sounds, Wundt kept close to Preyer's text. We need not review that part of his work, but let us instead focus on one issue which had, apparently, irritated him for a very long time. This was the widespread "prejudice"—among "mothers and midwives," "pedagogues," and "psychologists" alike, who are "spellbound by the miraculous inventive power of the child"[29]—that the child invents its own speech. According to this view, the child starts babbling and talking spontaneously, inventing new words time and again. This is "vulgar/folk psychology,"[30] according to Wundt, because it is not based on careful observation. Wundt dismissed the "invention" of babbling sounds: invention is goal-directed. This, however, does not apply to babbling, which arises by necessity, not by the aim to express oneself. Wundt then turned to the coining of new words, reported in the majority of diary studies, from Taine through to Darwin, Preyer, and Ament. He carefully went over almost every single reported case and concluded that "not a single word could be identified as independently invented by the child."[31] And this should not be a surprise, because the child already "ignorantly" imitates words from his environment before he attaches any meaning to them; it is mere "echo speech." Purposeless sound imitation is already up and running before word meaning enters the picture.

Although this sound imitation is all based on "inherited physiological aptitude,"[32] which provides the child with imitative power and a highly varied articulatory repertoire, much richer than initially needed for expression, Wundt's position on ultimate speech acquisition is an empiricist one: "After all, children's speech is a product of the child's environment, in which the child itself is, essentially, only passively involved."[33] Here Wundt strongly rejected Häckel's "biogenetic law," (as has already been mentioned in Chapter 4). It is the inherited disposition for imitation and use of gesture which the child shares with our speechless ancestors, but it need not "invent" a language, because that is given to him for free. Imitation suffices. But why then are the child's imitations far from perfect? Wundt proposes perceptual and articulatory-motor causes. In particular, he provides a sympathetic discussion of Schultze's theory. Not so much his "law of least effort," which makes children "simplify" articulations: that is obviously wrong, because during the babbling phase children effortlessly produce easy and hard speech sounds alike.

[28] And further to Mithen (2006).

[29] "... die von der wunderbaren Erfindungskraft des Kindes erfüllt sind," Wundt (1900a), Vol. I, p. 273.

[30] "Vulgärpsychologie" (ibid. p. 274).

[31] "... daß [...] nicht ein einziges Wort als von dem Kinde selbständig erfunden nachgewiesen werden konnte." (ibid. p. 280).

[32] "... dies kann aber nur auf einer vererbten physiologischen Anlage beruhen." (ibid. p. 288).

[33] "Nach allem dem ist die kindliche Sprache ein Erzeugnis der Umgebung des Kindes, an dem das Kind selbst wesentlich nur passiv mitwirkt" (ibid. p. 296).

But there is some truth to Schultze's observation that frontal articulations are preferred by the child, often replacing palatal or glottal ones. Here Wundt followed Gutzmann's suggestion that frontal articulations can be lip read by the child. This is a perceptual factor explaining poor imitation of non-frontal sounds. A motor factor largely involves the interactions between sounds, in particular progressive assimilation. The latter causes perseveration of a speech sound in a not yet fully developed articulatory system. This explains deformations such as *bibbe* for *bitte* ("please") or *Nana* for *Nase* ("nose").

For Wundt, acquiring speech is acquiring a highly complex expressive system. There is no mention in *Die Sprache* of the child's receptive or auditory speech abilities. Wundt is in good company with most of his contemporaries. It would be another 70 years before Peter Eimas initiated the experimental study of infants' speech perception.

Natural sounds

The immediate expression of feelings in articulatory movements, observable in the babbling of infants, is not fully lost in adults. Primary sound interjections such as *oh* and *ah* appear in language use as "isolated debris pieces of a pre-linguistic stage."[34] They are, however, largely replaced by secondary interjections such as "my god," a productive class with ever new additions. Ubiquitous, however, is the prosodic expression of affect and the special grammatical variants thereof in the vocative (*Peter!*) and imperative (*come!*) cases.

It is important, according to Wundt, to distinguish primary "feeling sounds" (*Gefühlslaute*), such as *oh* and *ah*, from imitative sound formations (as in *cuckoo* and in *sneezing*). The primary interjections are not depictive of meaning, for a simple reason: the affect is the immediate cause of an articulatory movement, but the sound resulting from that movement need not have any intrinsic relation to the affect.[35] That relation is an indirect one, which needs interpretation. For instance, when the child babbles *mamama* the repetitive articulation is probably caused by some sense of well-being. But the sound pattern *mamama* does not iconically denote a state of well-being. Rather, in many cultures, it is given the almost arbitrary interpretation of "mother" and it takes some time before the child picks that up. The situation is quite different for primary gestures. The child's initial grabbing and pointing gestures are equally affect-driven, but they are also transparently meaningful. This distinction, as we will see, is going to figure in Wundt's discussion of language origins. Because primary interjections are not iconic, they are unlikely to be the source of first words.

However, imitative "sound painting" may play a role. No trace of its origins is left according to Wundt,[36] but the process is still a productive one. We do occasionally

[34] ". . . vereinzelte Trümmer einer vorsprachlichen Stufe" (Wundt 1900a, Vol. I, p. 303).

[35] "Unmittelbar sind es ja nicht die Laute, sondern die Lautbewegungen, die durch den äußeren Eindruck triebartig ausgelöst werden" (ibid. p. 321).

[36] "daß von allem dem, was ursprünglich an Lautgeberden und natürlichen Lautmetaphern in menschlichen Sprachen vorhanden war, heute nichts mehr existiert" (ibid. p. 343). (". . . that from all that was originally available as sound gestures and sound metaphors, nothing at all still exists").

produce new onomatopoeias and we do play with sound metaphors (Wundt's term for phonetic or sound symbolism). There is, however, hardly ever imitation *of* sounds, but only imitation *by* sounds. The word *cuckoo* is by no means a faithful imitation of the bird's call, only a phonetic rendering of some characteristic feature. The relation is even less direct in sound metaphors. Wundt reviews many sound symbolisms reported in the linguistic literature. The most convincing cases are those that arise cross-linguistically in unrelated languages. One of his examples is the terms for spatial and personal deixis. Close-by places or persons are preferably denoted by words with "weak" vowels (*me, this*), more distant ones by words with "strong" vowels (*you, that*). The sound symbol can function because the feeling it evokes has a similarity to the feeling evoked by the image of what it denotes, a purely associative connection.

Folk psychology of sound change

What are the causes of change in a language's sound pattern over time? This had been a core topic in historical linguistics since the early work of Rask and Grimm (discussed in Chapter 2), but Wundt detested the psychology that had been proposed for the explanation of these changes. It was "completely without plan," it was as much the case that "one and the same phenomenon" would be adduced to "two opposite causes" and that "two opposite phenomena" were adduced to "one and the same cause." It was all motivated by some "superficial folk psychology."[37] How, for instance, could one at the same time claim that sound changes serve the function of marking finer meaning distinctions and also that sound changes are the result of speakers' laziness? To make things worse, many linguists believed that psychological causes of sound change would only explain the marginal exceptions to the sound laws, which are carved in stone and of a strictly physical nature. Wundt could not stand such chaos and decided to create order, based on solid psychological principles. The way he proceeded for all cases of sound change was to first consider the phenomena of speech sound variation in the individual speaker and then to apply their principles to sound changes at large in the language community.

Three types of sound change in the individual and in the language community

The speech sounds we utter have no fixed values. They vary in many respects, according to Wundt, such as place of articulation, duration, intensity, and spectral quality. Aside from the normal range of variation, there is an "abnormal" range of variation, the sound disorders in speech pathology and speech errors. Here Wundt presents a review of speech disorders (largely following Kussmaul) and of "normal" sound errors (largely following Meringer and Mayer). He also considered sound changes occurring in

[37] "Es ergab sich, wenn man die Erklärungen der Sprachforscher vom psychologischen Standpunkte aus näher prüfte, unabweislich, daß sie im ganzen völlig planlos entstanden waren, so daß es vorkommen konnte, für eine und dieselbe Erscheinung zwei entgegengesetzte Ursachen oder umgekehrt für entgegengesetzte Erscheinungen eine und dieselbe Ursache angeführt zu finden . . . Motive einer oberflächlichen Popularpsychologie." (Wundt 1920, p. 262).

non-native speakers. Many factors contribute to both normal and disordered variability, but a major factor is the interaction between speech sounds. Sounds influence one another, either by appearing in close contiguity or by analogy to similar sound patterns in memory. Wundt calls the former "contact" effects, the latter "distance" effects (*Fernwirkung*). In addition, there is the "regular" sound change, so-called in the nineteenth-century linguistic tradition because it was seen as a slow, regular change in the pronunciation of consonants that would apply anywhere. These were known as Grimm's laws.

Contact effects: assimilation and dissimilation

The core contact effects are assimilation and dissimilation. In assimilation speech sounds become more similar to one another than they would be in isolation, in dissimilation they become less similar. Assimilation is the most dominant effect in language change. Both anticipations and perseverations in speech sound errors are examples, such as Meringer and Mayer's examples *pree press* (anticipation) and *cumulation of stupends* (perseveration).[38] How do they come about? Wundt followed Meringer's speech-plan mechanics (see Chapter 5), but added an interesting discussion of Steinthal's ideas and the role of consciousness. According to Wundt, the contact effect can only arise when both sounds are simultaneously in consciousness. But remember that Steinthal allowed for only one single idea or image in consciousness, no more. So, how can *free* and *press* interact? Steinthal had come up with the ad-hoc solution of "fluctuating images" (*schwingende Vorstellungen*). On its way into or out of consciousness an image could still exert some effect on a fully conscious image. In this way, upcoming but still unconscious *press* could affect fully conscious *free*. This is cheating, according to Wundt. Instead, one should distinguish a *field* of view (*Blickfeld*) and a *point* of view (*Blickpunkt*) in consciousness. The field of view can contain more than a single item. Only the attended-to item is in the *point* of view. In other words, the inducing or disturbing item (such as *press*) is also conscious, although not in focus, not "noticed." In order to explain the preponderance of anticipations over perseverations, Steinthal had argued that thought is ahead of speech. There is some motor inertia in speech, hence the point-by-point conscious progression of a sentence is ahead of the physical speech mechanism. This may cause anticipatory errors, which is a *psychological* cause. But the same inertia of the speech organs is the *physical* cause of perseveration; some sound (such as the double *u* of *cumulation*) keeps hanging in the inert production mechanism. This dual theory is rejected by Wundt. According to him, both anticipation and perseveration are psychological in nature. Both involve interaction among speech sounds in consciousness, due to "similarity association."

There is, moreover, a good psychological explanation for the fact that perseveration is quite infrequent in adult speech, Wundt argues. Perseveration disappears with the automatization of speech. Perseveration is the more frequent phenomenon in the speech of children and of primitive peoples. Repetition, reduplication, and vowel harmony are

[38] Meringer and Mayer (1895), *preie . . . freie Presse* (p. 36) and *Kumulierung von Stupendien* (p. 49).

more the *original* forms of assimilation, because they arise in actions that are not yet fully automatized. They disappear with increasing mechanical training.[39]

Wundt discusses various cases of dissimilation. One is the elision of a sound that is adjacent to a very similar sound. Speakers do this when they say *substution* for *substitution*, which is deletion (of *sti*) by anticipation (of *stu*). Latin did it by replacing *semimestris* by *semestris*. In both cases the replacement is caused by a similarity association in consciousness. We will not, at this point, follow Wundt's extensive psychological analyses of assimilation and dissimilation in historical sound change, but rather turn to the other source of sound change, analogy.

Distance effects: analogy

In analogy the relation is not between two sounds in a speech plan, but between a word and a similar one in memory. Wundt recognized Herman Paul's leading discussion of analogy's role in sound change and he adopted Paul's distinction between grammatical and conceptual analogy. We discussed Paul's treatment of grammatical analogy in the previous chapter, in particular his notion of "proportion groups" (such as *ride:rode:ridden = rise:rose:risen*, etc.) and their associative force (on *hide:hid:hidden* to become *hide:hode:hidden*). In conceptual analogy, similarity of meaning exerts the associative force. For instance, first, second, and third person pronoun forms assimilate in many languages (such as *mon–ton–son* in French) and kinship terms tend to assimilate their word endings (as in *father–mother–brother–sister*). Word forms interact by similarity of meaning. That is exactly what happens in the individual speaker who blends meaning-related words (*close* and *near* blending into *clear*).[40] Wundt discusses this extensively before turning to the psychology of the analogical process.

Clearly, neither grammatical nor conceptual analogy is the result of a conscious volitional act. They arise mechanically. Hence the psychological process underlying analogy must be one of association. That had been Steinthal's account, but it fails, Wundt argues. For Steinthal, following Herbart, associations are between whole words, whether their meanings or their forms. This, however, cannot explain the phenomena. It predicts that one of two meaning-related words can replace the other one, but not that some *element* of one word changes to become similar to an element in another word. That, however, is what typically happens in grammatical analogy. When somebody produces a so-called "false analogy," for instance saying *hode* instead of *hid*, in analogy to *rose* (from *rise*), only the vowel is changed (from *i* to *o*). There is no replacement by the whole associated word.

Wundt set out to provide a formal account of analogy production. For this it is necessary to distinguish between the fixed-stem elements of a word and its relational or flexional elements. In *rise*, *r* and *s* are fixed-stem elements, but *i* is a flexional element. In analogy it is always the flexional elements that are affected (such as *i* turning into *o*). However, the inducer is different for the grammatical and the conceptual case of analogy.

[39] "... mechanische Einübung." Wundt (1900a), Vol. I, p. 442.

[40] From Fromkin (1973). Wundt presents examples such as *Zwittellaut* from *Mittellaut* and *Zwitterlaut*. (Wundt (1900a), Vol. I, p. 376).

In grammatical analogy the inducer is also a flexional element (the *o* of *rose* in the example). In the conceptual case it is the conceptually related, associated stem that induces the analogical change of the flexional elements. All this is formally worked out in some detail. Wundt is essentially (but tacitly) adopting Meringer's idea of having *elements* of words interact with one another. The disposition (Wundt's term) of elements to interact depends on their connection to other elements. For instance, flexional elements of two words are disposed to interact if they relate to stems that are meaning related. Usually there are multiple induced 'pulls' and 'pushes' at work among sound elements in speech generation. That is what makes us fluently produce the appropriate inflections on nouns and verbs, even on a new noun or verb that we have never inflected before. We carry these paradigms with us as latent forces.[41]

Wundt also treated the special distance effect of adopting a foreign word or idiom in one's language. In such cases there is always accommodation to the native language phonology. One important reason for this is that "the hearer is continuously inclined to perceive in foreign speech the sounds and words he is familiar with."[42] And there is usually meaning change as well, so-called "folk etymology," such as English *hammock* (from Spanish *hamaca*) turning into Dutch *hangmat*, literally a "hanging canvas and/or canvas to hang in."[43]

Regular sound change: Grimm's laws

The classical laws of sound change, "Grimm's laws," as discussed in Chapter 2, concern the historical sound shift from Sanskrit/Greek/Latin to Germanic. The most apparent sound change here is from *p-*, *t-*, *k-* unvoiced plosive (or 'tenuis') sounds to *f*, *dz/th*, *ch/h* Germanic spirant (or 'aspirate') sounds, as in *pater* → *father*. But in addition, voiced plosives (or 'media') *b*, *d*, *g* tend to turn into voiceless plosives *p*, *t*, *k*. And voiced aspirated plosives *bh*, *dh*, *gh* tend to turn into voiced plosives *b*, *d*, *g*. In other words, Grimm's laws involve a circular relation from tenuis to aspirata to media and back to tenuis. A later Germanic sound shift shows a similar, though not identical, pattern and is known as Verner's law.

We do not need to discuss these shifts in the fine detail as Wundt did, to appreciate his approach, which was to explain the causation of sound shift. Wundt considered three potential factors: physical, language contact, and cultural-psychological. It is interesting to read his intellectual dance around physical/physiological/ecological causes. The existing literature suggested that climactic or ecological circumstances affect a society's preference for certain kinds of speech sound and this can also lead to hereditary adaptations in the speech organs. Wundt put question marks everywhere; there is no convincing evidence around he argued. One purported example is the preference for glottal sounds in

[41] Wundt (1900a), I, pp. 463–464.

[42] "In eine in völlig fremder Sprache gehörte Rede ist der Hörende fortwährend geneigt die ihm vertrauten Laute und Worte hineinzuhören" (p. 476), an observation that has been well supported since: see Cutler (2001).

[43] From which, according to Wundt, the German *Hängematte* is derived.

the mountain peoples of the Caucasus and the suggestion that this is a consequence of the high air affecting the functioning of lungs and throat. Wundt did not like that idea; he suggested instead the possibility that the real cause lay in 'the habit of loudly shouting from mountain to mountain',[44] which affects the innate disposition (*Anlage*) of the speech organs. In this case one misses the radical approach Wundt had displayed in his discussion of "invention" in children's speech. He keeps the door open for ecological causes, in spite of a lack of evidence.

Regular sound change can also be the result of enduring language contact. For example, Romance languages, according to Wundt, show no inclination to vowel harmony (assimilating a word's vowels), but Romanian does. This must be due to the intensive long-lasting contact the Romanians have had with their neighboring languages Turkish and Hungarian, which do show that feature.

Wundt's heart, however, was with the cultural-psychological causes, not with cultural factors such as "instinctive acting of a collective mind" (*Volksgeist*),[45] as Grimm had proposed: the medieval sound shifts in German had been due to "barbarism and brutalization," but at the same time (as already cited in Chapter 2) to people's "violent progress and craving for freedom." This is not Wundt's approach. Barely better as an explanation is a "least effort drive," i.e., moving to ever-easier pronounceable speech sounds. But that hypothesis, at least, contains a grain of truth. Wundt qualifies it in two innovative respects. The first was to argue that ease of pronunciation is always a function of a speech sound's context. What are the contacting speech sounds, what is the local prosodic context? Those are the factors determining the ease of an articulation gesture, and these factors we already understand in terms of major aspects of their functioning. They are at work in the contact effects of assimilation and dissimilation discussed earlier. These processes may, in the long run, affect the habitual pronunciation of speech sounds. That, however, requires another factor, a culturally determined change of prosody, of speaking force, rhythm, and rate.

This is the factor Wundt preferred. As a culture develops, the interaction of thought and speech becomes more supple, more fluent. Eighteenth century German style was still heavy and vulgar, whereas the French were ahead in their supple delivery of speech. "A Leibniz and a Frederick the Great certainly spoke and thought faster in French than when they availed themselves of German."[46] At these higher rates there will be increased assimilation among neighboring speech sounds, both progressive and regressive. In addition, thought will be farther ahead of speech, which makes anticipatory assimilation the more frequent variant, a mark of automatization. As a consequence, many changes are likely to occur. Among them are word-final "weakening" of speech sounds, such as the German devoicing of word-final consonants (*Tag*—"day") getting pronounced as

[44] "... die Gewohnheit an lautes, von Berg zu Berg erschallendes Rufen" Wundt (1900a), Vol. I, p. 399.

[45] Ibid. p. 413.

[46] "Ein Leibniz und ein Friedrich der Große haben gewiß französisch schneller gedacht und gesprochen, als wenn sie sich der deutschen Sprache bedienten" (ibid. p. 419).

Tak and elision of inter-consonantal vowels (*Tag^{e}s* becoming pronounced as *Tags*). There will also be loss of aspiration in word-initial plosives. At higher rates, aspiration—as in *p^{h}ater* or *p^{h}unt*—will become too effortful and will be lost, causing a peculiar sound shift. This can still occasionally be observed in children's speech: the aspiration in *p^{h}unt* will first turn into the easier connecting spirant sound *f*, resulting in something like *pfunt*. With a further rate increase the plosive will be lost entirely, with *funt* as the ultimate outcome. Wundt argues that this change automatically occurs in tongue twisters, such as repeatedly pronouncing *fünf pfund* ('five pounds'), which turns into *fünf funt*. This is the type of change in articulation that must have caused Grimm's major sound shift. In addition, analogy will then have done a secondary job in further generalizing the $p \rightarrow f$ sound shift to other occurrences of *p*. This may also have happened in other primary sound shifts.

Contact effects are ubiquitous in fast speech, Wundt argues. A consonant's place of articulation adapts to the preceding and following vowel sound. The fricative *ch*-sound in German *ich* ("I") has a more frontal place of articulation than the one in German *Dach* ("roof"), etc.[47] Such interactive processes of articulation are the basis of historical sound shifts. These processes are entirely psychophysical and non-volitional. There is no drive to simplify sounds, or to sharpen their differences.

This is, in broad strokes, what Wundt had to say about speech sounds. He systematized the field of sound acquisition and sound change by doing away with the haphazard "folk" psychology of pedagogues and historical linguists. Speech sound operates in the domain of mechanical psychophysics, the domain of non-volitional, highly automatic behavior.

Words

Words form another major topic in *Die Sprache*, covering some 40% of the text. There are chapters on the morphology of words, on word categories, and on (word) meaning change. Once again, Wundt is in comprehensive mode. With great systematicity he reviews the linguistic state of the art from a psychological perspective. This perspective always involves the two basic psychological processes of ("mechanical") association and of ("volitional") apperception. We will not adopt Wundt's unmatchable comprehensiveness here, but rather concentrate on some of Wundt's core insights and innovations.

Word formation in brain and mind

Wundt's term is *Wortbildung*, "word formation." Wundt's Chapter 5 sets out to discuss the psycholinguistic structure of spoken and printed words. But somewhat unexpectedly, the chapter starts with a concise review of the Lichtheim model, which was discussed in Chapter 3. Wundt's purpose is to quickly reject the localization theory of words in the brain. It is, in his opinion, a hopeless enterprise to couple the images involved in the

[47] In early child speech a stronger version of Wundt's observation holds: the place of articulation spreading over the whole word (C. Levelt 1994, pp. 60–66).

elementary functions of speech to single nerve cells in particular brain regions. His main arguments for the untenability of this approach are these:

1. Conduction aphasia does not exist. If it did exist, it would, according to Wernicke, arise when the connection (through the insula) between Wernicke's and Broca's regions was disrupted and lead to the inability to repeat a spoken word. But what you get instead is paraphasia, a frequent symptom in central language disorders. Wundt refers to Freud's monograph on this matter, which had apparently impressed him.[48]
2. The Wernicke–Lichtheim model cannot handle the syndrome of Grashey's amnesic patient (discussed in Chapters 3 and 5). Even Wernicke agrees that the syndrome cannot be explained by any disordered center or connection in the model.
3. The Wernicke–Lichtheim model cannot explain why word loss in patients follows a certain hierarchy: personal pronouns and concrete nouns suffer, then concrete adjectives, followed by abstract adjectives, verbs, interjections, and finally particles.
4. Paraphasia, i.e., word mis-selections and neologisms, cannot be localized to any specific brain region because it appears in very different aphasic syndromes.

These four arguments do not strike one as being very convincing. Nature is rarely helpful enough to knock out a single center of connection. For the diagram makers the model had been a theoretical framework for representing *syndromes* in the first place. Also, claims of the *comprehensiveness* of the Lichtheim model are hard to find among the diagram makers. Remember that Wernicke had called Grashey's paper "the most important advance we have scored in explaining the aphasia issue during the last 10 years." In the background, no doubt, is Wundt's deep aversion[49] to the combination of old faculty psychology and phrenology, which he saw returning in the shape of a theory of speech-related functional centers in the brain.

Still, it is surprising that this is all Wundt chooses to say in *Die Sprache* on the neurology of language. Maybe the former brain anatomist and physiologist regretted having missed the opportunity to play a role in the exciting Dax–Broca–Wernicke–Lichtheim voyage of discovery. Anyhow, Wundt's innovation lay in providing an entirely psychological characterization of central speech/language disorders. The basic explanatory principle was the "principle of functional exercise," which later, in the behaviorist tradition, became known as the "law of effect." It says that each function is improved by exercise and diminished by non-use. The nervous substrate and connections in the individual brain are not fixed, but largely the product of exercise, not the cause but rather the outcome of function.

It is functional exercise which creates the connections between the images and motor patterns involved in speech. The main direction of association is from sensory to motor. Any spoken word activates, to some extent, the articulatory system. The inverse association is much weaker. In fact, there are no motor images at all, let alone speech motor

[48] Remember that this had not been Wernicke's prediction; he had stressed the patient's diminished self-monitoring, leading to paraphasias.

[49] Expressed in Wundt (1900a), I, p. 496.

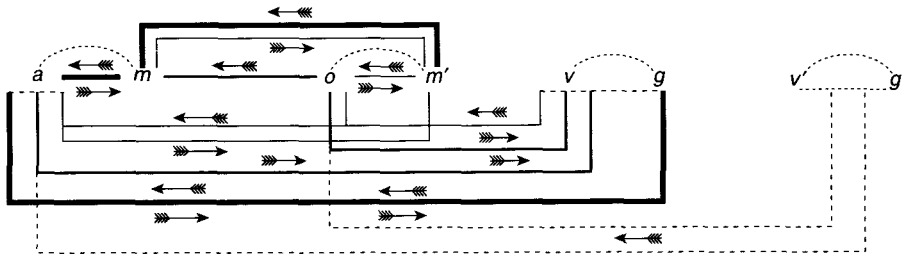


Fig. 6.3. Wundt's associative diagram of speech components—see text (Wundt 1900a, Vol. I, p. 521).

images (Broca's area is just a motor region). The strong forward association explains the remnant of "echo speech" that can be observed in just about any patient. The exercise of associations also explains the aforementioned category-specific character of word loss in patients. Related, associated words, such as verbs or substantives, come and go together. To explain the *hierarchy* of loss, Wundt introduces a further principle. Concrete items can easily be imagined without activating their word names, but to entertain more abstract notions one cannot do without the abstract word in consciousness. Images of people or concrete objects suppress the corresponding names in consciousness. It is no surprise that these words are the first to go in aphasia. Ultimately it is the particles that remain. They have no image content; they almost *are* the abstract concept. This is all rather ad hoc, and presents a premature simplification of aphasic phenomenology.

Wundt draws his own diagram of functional associations in speech (Fig. 6.3). These are the stronger and weaker associations among three ingredients of words: sound, concept, and graphics. A word's sound has two aspects, the auditory image (a) and the motor impulse (m). Its concept or meaning has the two aspects of image content (v) and feeling or affect (g). A word's graphics also have two aspects: optics (o) and writing motion (m'). There are both stronger and weaker associations among these six aspects. Wundt happily concludes that "the essential disorders of speech can without difficulty be derived from the general association conditions" in this diagram, and that "it is immediately obvious that it would be impossible to think of this association schema as turned into a schema of centers and connecting bundles."[50] The message is: it is alright to make diagrams, but do not interpret them neurologically.

After this, Wundt turns to visual, tachistoscopic word recognition, reviewing some of Cattell's work (but of course not Erdmann and Dodge's). The main point is that a word's letters are not elements that are successively or simultaneously perceived, but rather they are *features* of the visual word. It is, therefore, not letters that are recognized during

[50] "Lassen sich hiernach die wesentlichen Störungen der Sprache ohne Schwierigkeit aus den allgemeinen Associationsbedingungen ableiten, so ersieht man doch ohne weiteres, dass es unmöglich sein würde, etwa das oben dargestellte Associationsschema in ein Schema von Centren und Leitungsbahnen umgewandelt zu denken" (Wundt (1900a), Vol. I, p. 524).

tachistoscopic perception, but the word as a whole, on the basis of whatever features manage to jointly activate the word's image in memory.

Then, finally, Wundt gets to morphology: "word formation" proper. His first issue is the old root-language theory of historical linguistics. The roots must, if anything, have been pure stems, without flexional elements. Wundt nicely ridicules Humboldt and Steinthal's "peculiar" idea of denoting a word's root and flexional elements as substance and form, respectively. This would make Chinese a "formless" language. Then he gives short shrift to the "romantic" notion of a "half mythical root period" in the origin of language. In particular the notion of the "Sanskritists" (and he must have meant Max Müller in particular)—that the root period had provided the basic materials from which all later word formation had derived—is untenable. The introduction of new words and stems into a language has never come to an end. And there is no way for us to determine whether the described root repertoire of Indo-European languages is not itself the product of derivational, combinatorial processes. Roots are linguistic abstractions, not facts. Here Wundt praises Hermann Paul, who hardly ever uses the term *Wurzel* ("root") in his *Principles of historical linguistics.*

Having rejected that obsolete theory, the door is open for consideration of the various types of ongoing new word formations and their psychological origins. Here are the types Wundt mentions or analyzes in detail: the slang of students, the technical terms of scholars, the sound metaphors that form open classes (*stand, stiff, stately, stable, . . .*), sound and word reduplication (*mama, bonbon*) and word compounding (*household*). Curiously, a category totally absent in Wundt's treatment is derivational morphology, the formation of new words by adding derivational affixes. In fact, the distinction between inflection (*work–worked*) and derivation (*work–worker*) is never mentioned by Wundt, although it figures clearly in Paul's praised text. Wundt's attention is rather on the psychology of new formation. Let us consider just one of his more original analyses, the treatment of word compounding.

From the syntactic perspective, you can distinguish three types of compounding. When you speak, you start building a sentence from a "total image" (*Gesamtvorstellung*), which you then successively analyze into smaller images or concepts that go with words. At this level of analysis adjacent words sometimes entertain a close conceptual relationship and, if this happens with some frequency, the pair can be singled out as a compound word. Wundt gives French *pourboire* ("for drinking"–"gratuity") as an example. That is the closest possible, Type I association. Next is the case where two non-adjacent words that still entertain some conceptual relation in the sentence are singled out as a pair to form a useful compound. *Apple tree* is such a Type II case, according to Wundt. The pair arises from the same image, but the words rather rarely end up in adjacent sentential positions. The third, least associated type arises if the association is between two totally different images; the words are not likely to arise in the same sentence. Wundt's example here is *Hirschkäfer* ("stag beetle"), where *stag* and *beetle* are rarely part of the same image. Here it is some similarity between the two images that causes their association. Wundt discusses "logical" theories of compounding, which aim at delineating the possible relations two words in a compound can entertain. But this is a dead-end approach: psychologically, any

thinkable relation is possible; there is no limit. On this matter Wundt is in good agreement with modern approaches to compounding.[51] Like syntactic phrase structure (to be discussed) compounding is always binary. When a speaker creates a multi-word compound, such as *Grossvaterstuhl* ("grandfather's chair"), this is composed of two binary relations, one between *grand* and *father* and another one between *grandfather* and *chair*. Wundt was the first to present such morphological relations in tree graphs.

Parts of speech

Words come in different categories, in particular nouns, verbs, adjectives, and particles. These categories play different roles in the sentence, such as a noun being the direct object of some action denoted by a transitive verb. Languages differ widely in which roles they mark in the sentence and how they achieve the marking. The three major marking options are first, positional marking, such as the subject noun or noun phrase appearing in first or early sentence position (as in "*John* hit the ball"); second, inflectional or case marking (as in "John gave *me* the ball," where *me* is the dative-marked first person pronoun); third, marking by means of particles or prepositions (as in "John gave the ball *to* Peter"). In addition, the verb may get implicitly or explicitly marked for various functions, such as expressing the time or tense of the event (for instance, past, present, future), the mood (such as indicative, imperative), aspect (duration, completeness of the action), and more. In a massive chapter, over 200 pages long, Wundt reviewed the rich comparative and historical linguistic state of the art on these matters. We will only consider his reasons for going into such enormous detail.

His motivation is, again, to provide a psychological explanation. It was Wundt's conviction that the history of human consciousness, the evolution of thought, could still be read from the linguistic phenomena at hand, somewhat like the astronomer who can today observe what happened billions of years ago in the universe. Analysing these still-available phenomena is Wundt's road into understanding the "higher" psychological mechanisms, the structure of mind. The structure of a language reflects the level of intellectual development of its users. Accordingly, there is a hierarchy of languages, from least to most developed. This is by no means a new idea in nineteenth-century linguistics; it was the general belief among linguists. But Wundt argues against the more primitive renderings of this idea. So, for instance, he argues against the Humboldt–Steinthal view that languages climb in the hierarchy from "isolating" to "agglutinative" to "inflectional." The state of a language's development is a multidimensional matter. The language can be advanced in one respect, such as its case structure, but less advanced in another respect, such as the expression of mood.

Let us take the expression of case as an example of Wundt's approach. There are three levels of development in this respect. At the lowest level, languages hardly express any case relations at all. Examples are, according to Wundt, the languages of Hottentots, Bushmen, Mande-Negroes, and the Papuas. The second level of development is shown by languages with an excessive flowering of case relations, to mark all sorts of temporal,

[51] See, for instance, Fanselow (1985).

spatial, and visual relations. Here the examples are the American-Indian languages and the Basque and Caucasian languages. The highest level of development, finally, is reached by languages that have a case system that marks a small set of basic relations among concepts, expressing all other more specific relations by means of prepositions. This is the case for Semitic and Indo-European languages. But then, Indo-European languages beat the Semitic ones in another respect, the expressive power (*Aktionsart*) of verbs. In the Semitic languages, according to Wundt, the use of verbs is "developed" in its expression of objective ("genus") features of the action, such as its temporal properties of duration, continuity, or repeat. Here the Semitic languages match the Indo-Germanic ones. But the latter are more developed in the expression of "subjective" features, the relations the speaker has to the mentioned action, such as the subjective moods of wanting, believing (now called "propositional attitudes"), as well as the temporal relations of the event or action to the present speaker ("tempus" tense). This difference makes Wundt conclude that Semitic languages remained at a level of an "objective (concrete) way of thinking" (*Anschauung*), and were less able to conceive of subjective states.

Correspondingly, Wundt distinguishes between two types of case relation. There are, on the one hand, the "concrete" or "outer" cases, which express concrete, visual, locative relations, such as the source and goal of a movement, the spatial inclusion of one object in another, and so on. There are, on the other hand, the "conceptual" or "inner" cases, which express a small number of logical–grammatical relations around a verb, such as nominative, accusative, or dative. Wundt then observes (1900a,Vol. I, p. 78) that the first type of case *always* requires case marking by means of additional elements, such as suffixes or prepositions. The second type, however, can in principle do without case; position marking around the verb suffices (in "Hans sees Gretel," Hans is nominative and Gretel accusative, without any further marking). As we will see in Chapter 9, this observation was approvingly taken up by Karl Bühler.

Meaning change

Wundt's treatment of word meaning is entirely from the perspective of meaning change, which was a major chapter in nineteenth-century historical linguistics. A synchronic theory of lexical meaning was still to be developed. The basic ingredients of Wundt's treatment are as follows. Words express three kinds of concept: entities/things (*Gegenstände*), properties (*Eigenschaften*), or states (*Zustände*), or any relations among them. The corresponding parts of speech are substantives/nouns, adjectives, and verbs. Among the earliest relational words are (paired) nouns such as *father–son*, (paired) adjectives such as *big–small* and (paired) verbs such as *love–hate*. The primordial words must have been substantives, expressing the image of some concrete object. In the mind of the speaker there is a conscious image of the observed or remembered object (for example a church). The speaker's focus of attention is on some dominant property (δ) of the imagined object; it is "apperceived" as "standing in"[52] for the object as a whole (such as a church's tower standing in for the whole church). More in the background of consciousness are,

[52] "... stellvertretend." Wundt 1900a, Vol. II, p. 475.

first, relatively constant properties of the imagined object (a set denoted by *A*, including in the example, some front entrance) and, second, the more accidental properties (the set *X*, for example the church being full). If that structured image is named by the sound form *n* (*church*), the complete, whole-word image can be represented by $n\delta(A.X)$.[53] Here the other two kinds of concept—properties and states—come in naturally. The set *A* is the set of object properties, the set *X* is the (ever changing) set of states of the object.

Historical linguists had often observed that first labels for objects denoted some dominant property, but that does not justify the (still common) conclusion that first words denote abstract, general concepts, Wundt argues. The primordial speaker is simply not conscious of the fact that the dominant feature δ is shared by other objects. But as soon as that level of consciousness develops, a real property term may arise denoting δ or some other property in *A*. Because original concepts of property and state were always relative to entities/objects, they were in the same way "sensory" or concrete in character. This is one of the many ways in which language evolution tells us the story of the evolution of thought.

Leaving an entity's variable properties (*X*) aside, Wundt's word schema is $n\delta A$, which is similar to Saussure's concept of "sign" (as will be discussed in the next chapter), but with the addition of a dominant or characteristic property δ. That property is to play a role in several aspects of meaning change. In the strictest sense, meaning change involves preservation of *n* (the word's sound form is kept) with some change in δ, *A*, or both. When it is only *A* that changes, the dominating or distinguishing property δ is kept. An example is extending the original meaning of *leg* to supports of tables and chairs. Here the meaning change is from $n\delta A$ to $n\delta A'$.

When the dominating property δ is lost in a meaning change, some other feature of *A* (call it ε) must be carrying the transfer. This happened, for instance, when original *land* (some area of ground) came to mean "people," the inhabitants of some land. Here the common feature ε is some spatial location on earth. This is a good example of meaning change leading to ambiguity. But in other cases the original meaning gets entirely lost. Historically, whole chains of meaning shift have taken place, at every step involving some other feature ε. The common feature is often one of affect. To call a taste *sharp* is playing on an affective correspondence between a point or knife and a particular type of taste; similarly for a *high* tone or a *black* moment.

Wundt extends his formal treatment to many other cases of meaning change, involving such interesting transfers as going from the tactile to the mental domain, which gave rise to German terms such as *begreifen* ("to grasp"–"to understand").[54] Wundt argues that such meaning changes are hardly ever "willed"; there is no conscious volitional process involved. Rather, if the conditions are fulfilled, existing associations will coalesce to apply

[53] Wundt's δ-notion has the same flavor as George Miller's (1969) "distinguisher" or "assertion," the aspect of lexical meaning which is dropped under negation, as opposed to the presupposition (Wundt's *A*), which is maintained.

[54] See Lakoff and Johnson (1980) or Fauconnier and Turner (2003) for modern versions of this idea.

the shifted sense to the term. But Wundt makes an exception for metaphor. Here, there is, in most cases, some intentional act involved, which—if effective—is picked up by the language community. A metaphor is sentential in nature, expressing an associative similarity relation between states of affairs in different domains. One of Wundt's examples is Heine's "hearkening stand the mute forests, every leaf a green ear,"[55] where the silence of a forest at night is effectively expressed by the image of intensive listening. Such intentional transfers of meaning also occur occasionally in the coining of new words. The French word *lunettes* ("little moons," "glasses"), for instance, must have been an intentional application of the shape similarity between the two glasses and the moon.

Wundt's analyses of meaning change are intended to demonstrate the psychological regularity of linguistic phenomena. It is always the laws of association, by similarity or contiguity, that are at work, often triggered by accidental circumstances or by volitional, apperceptive acts. These latter psychological processes are, however, most apparent in the speaker's generation of sentences, to which we will now turn.

Formulating sentences

Wundt's treatment of syntax is his most innovative contribution to the language sciences. The title of Chapter 7 of *Die Sprache* is *Die Satzfügung*, "sentence formation," and that is exactly what it is about: the speaker's ways of formulating sentences. Any explanatory account of syntax must be psychological in nature. We will follow Wundt step by step. His starting issue is this: how does a sentence arise in the speaker's mind? The answer defines the notion of "sentence." Wundt then turns to the varieties of syntax and to phrase structure; it is his core contribution to linguistics. He introduces the formal, binary phrase structure tree as a representational format and applies it to a wide variety of comparative linguistic and historical phenomena. Phrase structure, in its turn, is one determinant of sentence prosody, according to Wundt. The other one is the speaker's affect. Prosody has become measurable and Wundt was among the first to apply such measurements. A final discussion in the chapter of how these "outer" speech forms, such as prosody, relate to "internal" speech, the speaker's style of thinking, completes the picture.

Where do sentences come from?

This is the title of a much-cited and innovative paper by Charles Osgood (1971). Wundt had raised many of the same issues and, not surprisingly, provided many of the same answers decades earlier. But quixotic history and language barriers had done their work; a rich tradition had been entirely lost in the "new" psycholinguistics.[56]

[55] "Horchend stehn die stummen Wälder, jedes Blatt ein grünes Ohr" Wundt 1900a, Vol. II, p. 560.

[56] When I was a postdoctoral fellow at Harvard's Center for Cognitive Studies during 1965/66, I had the pleasure of occupying the office next door to Arthur Blumenthal, who had set out to re-establish the lost connection. (And he did. Some of the best and most inspiring modern publications on Wundt's psychology and the history of psycholinguistics are from his hand; see in particular Blumenthal 1970,

The origin of a sentence is a "total image" (*Gesamtvorstellung*) in the speaker's consciousness.[57] In Wundt's psychology, you can entertain some complex image or idea in consciousness in its entirety, but you can consciously attend to only one of its aspects at a time. Attention can, as it were, successively scan the total image. So, there is simultaneous presence of the image in the "field of view," but only a successive presence of aspects of the image (parts, features, relations) in the "point of view." In formulating a sentence, the speaker successively dissects a total image into its parts in such a way that the parts enter into specific relations among them. This is a volitional, apperceptive process. There are the three kinds of conceptual parts into which an image can be analyzed, expressible in words: entities/things (*Gegenstände*), properties (*Eigenschaften*), or states (*Zustände*). These can entertain a small set of binary ("logical") relations, such as subject–predicate. As with any volitional act, there is always an affective component involved in the process, which is most marked in requests, warnings, and sentential expressions of feelings. The ultimately apperceived, focused conceptual parts of the image automatically activate the associated word patterns, which "stream out" as successive apperception proceeds.

This leads Wundt to his (then) famous definition of the sentence: "The sentence is the linguistic expression of the volitional analysis of a total image into its parts that are put in mutual logical relations."[58] This idea raises two psychological issues: how does the total image arise to start with and what is it that motivates the dissectional process?

The total image is always the outcome of associative and apperceptive processes. They are quite similar to what happens in object perception. There is a host of associative connections among an object's sensory parts, which can be apperceptively combined in a unified perception. The total image is a "wanted" unified conscious conglomeration of associated parts. Speakers have great experience in generating, from established associative patterns, images for linguistic expression. This ability we probably share to some extent with higher animals, but that does not hold for the second step, putting focused parts of the image in mutual "logical" relations that are "allowed for" by the total image. This is essentially a *linguistic* process, language at work. Genetically, however, this process did not begin with the three kinds of concepts and some set of logical relations as pre-established notions. Rather, these conceptual entities must historically and ontogenetically have emerged from multiple, concrete, factual partitionings of images in consciousness. It is the story of increasing abstraction in human thought. The experienced speaker manages to generate a succession of logically related parts under the unifying umbrella of the enduring total image. There is an apperceptive tension here, with a variety of syntactic consequences. The ultimate motives for the syntactic partitioning of the total image

1980). He regularly shared with me the finds he made in the Widener library. These books contained a slip with loan dates. Almost without exception they had not left the library for half a century, including Wundt's books.

[57] This is roughly equivalent to Levelt's (1989) notion of "preverbal message."

[58] "... den sprachlichen Ausdruck für die willkürliche Gliederung einer Gesammtvorstellung in ihre in logische Beziehungen zu einander gesetzten Bestandtheile" (Wundt 1900a, Vol. II, p. 240). Notice that Wundt uses the expression "logical relation" here in the psychological sense of "thought relation."

should be sought in our self-awareness; we conceive of ourselves as volitional. The motives of our volitional acts transpire in the ways we construct our sentences, which is Wundt's next topic.

It is the psychology of this partitioning that is responsible for the strong saliency effects reported in Osgood's paper. In describing visual scenes, speakers tend to express salient elements in early sentence positions. This is how Wundt formulates it: "where word ordering is free, not bound by fixed traditional norms or other conditions, words follow each other according to the degree of conceptual emphasis."[59]

It is remarkable that neither Wundt's definition of "sentence" nor the extensive explanatory text mentions any communicative function of the sentence, not even among the "motives." Sentences are nothing other than highly complex, willful expressive movements of the individual. It is not impossible that Wundt wanted to keep self-talk (which he occasionally refers to in *Die Sprache*) under the same general definition. But then, with his introduction of self-awareness as a motivational concept, a function of communicating with oneself would not have been out of place.

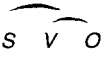
Varieties of syntax and phrase structure

The traditional distinction between indicative, imperative, and interrogative sentences is also adopted by Wundt. He introduces his innovative phrase structure theory for the domain of declarative sentences. Here he first addresses the existing terminological chaos about the notion "subject." From the time of Steinthal, there had been a proliferation of definitions, making subtle distinctions between the grammatical, logical, and psychological subject of a sentence. Wundt does away with all that and limits the notion to "what the sentence is about." Hence, *Romulus* is the subject of *Romulus founded Rome* and *Rome* is the subject of *Rome was founded by Romulus.* An entirely different notion is the speaker's attentional focus. It will often coincide with the subject, but it need not. It does not, for instance, when the speaker focuses on Romulus and says *Rome was founded by Romulus.*

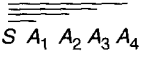
The notion of "subject," paired with the notion "predicate" (what is said about the subject), are essential ingredients in Wundt's treatment of phrase structure. Wundt defines two basic types of syntactic relation, the "closed" and the "open." Subject–predicate is a closed relation; it is binary and cannot be further extended to ternary or anything else. Almost all declarative sentences in Indo-European languages have subject–predicate ($S \frown P$) as their primary partitioning of phrases. This pair expresses the first apperceptive partitioning of the speaker's total image (G): in the example, the partitioning between the subject *Romulus* and what is said about him, the predicate *founded Rome.* Any further partitioning of the image is, again, closed and binary. The predicate, for instance, will be further partitioned into the predicative verb (V) *founded* and its

[59] "...wo die Wortstellung frei, nicht durch eine überlieferte feste Norm oder durch andere Bedin-gungen gebunden ist, da folgen sich die Wörter nach dem Grad der Betonung der Begriffe" (Wundt 1900a, Vol. II, p. 350).

object (O) *Rome*. Wundt's formal notation for a closed syntactic relation is "⁀" The phrase structure for *Romulus founded Rome* thus becomes:



Open syntactic relations are very different. The basic type here is the relation between an entity and its attribute. The primary partitioning in the total image is binary, as between *man* and its attribute *good*. But this relation is not closed, because additional attributes of *man* may come to consciousness such as *reliable, conscientious, diligent*. They are associative appositions to the first, binary attribution. Wundt represents such open syntactic relations by "—." Hence *the man is good, reliable, conscientious, diligent* has as phrase structure:[60]

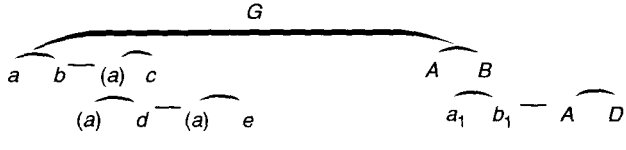


Appositions of this kind are, in general, not further partitions of the original total image. Instead they derive from new images that are associatively activated by the entity focused in the original total image.

The phrase structures sentences speakers (or writers) produce will often combine both types of syntactic relation. To demonstrate this, Wundt presents the phrase diagrams for two sentences taken from Goethe. Sentence (1), from Goethe's *Wahlverwandtschaften*, almost entirely consists of closed syntactic relations. Sentence (2), from *Werther*, largely consists of open, attributive relations.

(1) Als er sich den Vorwurf sehr zu Herzen zu nehmen schien (a⁀b) und immer aufs neue beteurte (c), daß er gewiß gern mitteile (d), gern für Freunde tätig sei (e), so empfand sie (A⁀B), daß sie sein zartes Gemüt verletzt habe (a_1⁀b_1), und sie fühlte sich als seine Schuldnerin (A⁀D).

"When he seemed to take the objection much to heart (a⁀b) and ever anew declared (c), that he certainly liked to talk (d), and help his friends (e), she realized (A⁀B) that she had hurt his gentle mind (a_1⁀b_1), and she felt guilty towards him (A⁀D)."[61]



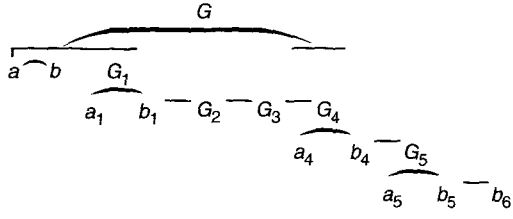
(2) When ich zu meinem Fenster hinaus an den fernen Hügel sehe (a⁀b), wie die Morgensonne über ihn her durch den stillen Nebel durchbricht (a_1⁀b_1), und den stillen Wiesengrund bescheint

[60] The copula (*is*) is in Indo-European languages a general means of forcing an essentially attributive relation into their preferred S–P format, according to Wundt. Here we ignore the subtleties of that analysis. The represented phrase structure is my own rendering, following Wundt's format.

[61] Gerard Kempen surprisingly alerted me to three errors in Wundt's corresponding diagram. Two of them are typographical: (i) the c in line 2 should vertically align with the leftmost "⁀" in line 3; (ii) the '—A⁀D' at the right end of line 3 should shift up to line 2, connecting to 'A⁀B'. The third, factual error concerns the leftmost (a) on line 3. Here the parentheses should be deleted, because the element ('er') is overtly there in the sentence.

(G_1), und der sanfte Fluß zwischen seinen entblätterten Eichen zu mir herschlängelt (G_2) – O! wenn da diese herrliche Natur so starr vor mir steht wie ein lackiertes Bildchen (G_3), und alle die Wonne keinen Tröpfchen Seligkeit hinauf in das Gehirn pumpen (b_4) kann (G_4), und der ganze Kerl (a_5) vor Gottes Angesicht steht (G_5) wie ein versiegter Brunnen (b_5), wie ein verlechter Eimer (b_6).

"When I look outside from my window at the distant hill ($a \frown b$), see how the morning sun above it breaks through the still fog ($a_1 \frown b_1$), and lights the quiet meadowland (G_1), and the gentle river between its leafless oaks meanders towards me (G_2) – Oh! when this glorious nature stands before me as frozen as a small lacquered picture (G_3), and all delights cannot pump (b_4) up a drop of bliss into the brain (G_4), and the whole fellow (a_5) stands in sight of the Lord (G_5) as a run-dry well (b_5), as a cracked bucket (b_6)."



Both sentences are representations of global syntactic structure, clauses, subject–predicate, modifier phrases. It is a level of "logical" structure for Wundt, displaying what is thought by the speaker; it is at the same time a syntactic phrase structure. Wundt was aware of the fact that syntactic order and logical phrasing could diverge, leading to various kinds of syntactic "dislocation" (*disloziert werden*) and "entanglement" (*Verschlingung*). Wundt could also handle such discontinuous constituents by way of his "structural formulas." He diagrammed the "broken up" long-distance relation below the sentence and the simple contiguous relations above it. So for the Latin sentence *magna dis immortalibus habenda est gratia,* ("Great is the gratitude owed to the immortal gods," in the Latin word order: "Great to (the) gods immortals to-be-had is (the) gratitude") Wundt presents the diagram:

where the long-distance relation between adjective *A* (*magna*) and subject substantive *S* (*gratia*) is depicted in the lower half of the diagram and the *OVS* phrasing in the upper half. Wundt is also fully aware of the fact that two almost synonymous sentences may still differ in their phrase structure. This holds, for instance, for question forms such as *Has John gone?* versus *John has gone?*, which only differ in affective tone. "These are transformations occurring in all areas of linguistic expression," Wundt says prophetically.[62] As Seuren (1998, 219*ff.*) has extensively demonstrated, there can be no doubt that Wundt's linguistic innovations, the use of phrase structure diagrams and recognizing the complex dependences they reveal, were far ahead of his time. It introduced the structuralist program of immediate constituency (IC) analysis, with Bloomfield as its great leader.

[62] Wundt's example is: *hat die Uhr geschlagen?* versus *die Uhr hat geschlagen?* "Das sind Transformationen, wie sie in allen Gebieten des sprachlichen Ausdrucks vorkommen" (Wundt 1900a, Vol. II, p. 257).

Bloomfield was heavily indebted to Wundt, as we will see in Chapter 8. It is the more surprising that it took another half century before *any* linguist published a tree graph.[63] Apparently, as Seuren suggests, it was just "not done."

The main difference between the two Goethe diagrams concerns coordination. The second sentence is heavily coordinative, whether or not expressed with "and" connectives. There are only three coordinative connections in the first sentence and all three are parts of the one total image in consciousness. The second sentence, in contrast, is a concatenation of ever new total images that arose associatively in Goethe's mind. Such structures can originate when the original total image gets "broken up," according to Wundt, by loosening of attentional control. New associations flow in and the original total image fades from consciousness before it is fully expressed. Goethe did not even complete the framing sentence ("When I looked outside from my window . . ."), which is indicated in the phrase diagram by an empty slot.

Closed and open syntactic relations are two equally basic devices for the expression of thought. From Wundt's genetic, evolutionary perspective, the attributive relation was probably the original one. All languages with closed relations also display appositive relations, but there is still the occasional language around with exclusively attributive syntax. Wundt mentions the language of the Bushmen as an example. Citing a Bushman's sad diatribe about the unreliability of the white man, he argues that the speaker time and again introduces a topical subject, keeping it in attentional focus, while one attribution after another is being made, a structure as in (ii) (*He standing up, he going home, he going jolly, he drinking water, he going to sit . . .*, etc.).[64] Wundt then develops the theory that, from this attributive starting position, the languages of the world took two different routes, leading to a major typological distinction. Some dominantly developed the closed, binary type of syntax. This holds in particular for western languages. There the original S–A relation developed into a strictly binary predication S¯P, which got further partitioned as in (i) binary all the way down. Attribution was never entirely lost in those languages. The dominant attentional bookkeeping, however, was to keep the one original total image available in consciousness until all of its parts were expressed. This leads often to deep syntactic hierarchies, with all types of syntactic subordination. The advantage, according to Wundt, is that the total image remains in control, expressing its simultaneous conceptual unity. At the same time, however, it is a rather terse style of expression, where it is hard to keep track of the relational hierarchy, which soon exceeds the limits of consciousness. The latter is easier in the attributive style, where successive attributions are made to some focused entity; there is no need for them to be simultaneously present in consciousness. As Wundt argues, the latter style is advantageous in oral discourse, whereas the closed style is easier to cope with in writing.

In contrast to most of his contemporaries, Wundt did not qualify the closed-type languages as more advanced than the open-type ones. After analyzing, in almost Whorfian

[63] That was Nida (1949, p. 87), but see Chapter 10 for a curious exception.

[64] ". . . er stehend auf, er gehend heim, er gehend lustig, er trinken Wasser, er gehend sitzen . . ." Wundt 1900a, Vol. II, p. 332.

fashion, the text of a Yakut song[65] as largely attributive he writes: "That is certainly a type of syntactic construction, which is very different from our ways of thinking."[66] But this appositional style allows for "an exceptional pluriformity of composition." "Therefore, one doesn't have the least right to speak in view of such phenomena of 'formlessness' of the language or even of a lesser perfection of the syntactic form."[67]

If word order is free, it follows conceptual emphasis, but in many languages word order is not free. The world-wide dominant pattern, according to Wundt, is to place the subject in the initial position, whether in closed (subject–predicate) or open (subject–attribute) type syntax. The genetic origin here is no different. The conceptual center of the total image is the entity whose attribute or state is expressed. The entity (*Gegenstand*) is primary, because no attribute or state can exist without its subject. It becomes what Wundt calls the "explanatory" word order, S–V–O or S–O–V. It is, as Wundt had observed, also the naturally arising order in Deaf sign language. But there are languages that place the verbal element in first position, either as V–O–S or V–S–O. Wundt calls this the "narrative" word order. It follows from a preference to focus on the sequence of events. The Indo-European languages are predominantly of the explanatory, the Semitic languages of the narrative type.

Sentence prosody

Speech rhythm expresses the way in which the total image is partitioned over successive clauses and phrases, according to Wundt, whereas speech intonation is expressive of affect, accent follows the affective dimension of strain versus relaxation. Successive images evoke varying degrees of stress, which get expressed as prosodic accent. Although these two aspects of prosody, phrase partitioning and affective stress, follow their own course in the generation of speech, Wundt suggests that they both follow a probably universal law: they come in three degrees or levels. The rhythmic pausing pattern reflects the levels of phrase structure. Word groups are separated by the shortest pauses, clauses by medium size pauses and whole sentences by longest pauses. Similarly for sentence accent: the traditional distinction between primary and secondary stress does not suffice. One can easily distinguish two degrees of secondary stress ("acute" and "double acute"), but no more. However, sentence or pitch accent can be made by both upward and downward

[65] Yakut is a Turkic language, spoken in the Sakha republic of the Russian Federation.

[66] "Sicherlich ist das eine Art syntaktischer Fügung, die unsern Denkgewohnheiten sehr ferne liegt" Wundt 1900a, Vol. II, p. 338. Compare to: "How utterly unlike our way of thinking!" (Whorf on the Apache's rendering: "it is a dripping spring" as "as water, or springs, whiteness moves downward" (Whorf 1956, p. 241)).

[67] "... daß nun diese Appositionsformen eine außerordentliche Mannigfaltigkeit der Zusammensetzung gewinnen können" (1904 edition of *Die Sprache* Vol. II, p. 346). "Darum besteht nicht das geringste Recht, derartigen Erscheinungen gegenüber von einer 'Formlosigkeit' der Sprache oder auch nur von einer geringeren Volkommenheit der syntaktischen Form zu reden" (1900 edition Vol. II, p. 339). A frequent source of reference for Wundt from this perspective was Viennese linguist Friedrich Müller's four-volume *Grundriss der Sprachwissenschaft* (1876–1888), a comparative sourcebook of over a hundred languages, partitioned into language families.

Fig. 6.4 Wundt's intuitive version of the affirmative, interrogative and exclamatory versions of *Der Feind ist besiegt*. From *Die Sprache* II, first edition 1901, p. 401.

pitch modulations. Since both allow for three levels, the theoretical modulation range is six-level. Wundt argues that different languages make different "choices" here. Siamese (i.e., Thai), for instance, picks two levels up and one down.

Wundt clearly felt handicapped by a lack of "objective" prosody data. He mistrusted the intuitions of professional observers, including his own. In the first, 1900 edition of *Die Sprache* he depicted his own rendering of the intonation patterns for the declarative, interrogative, and exclamatory versions of the same sentence *Der Feind ist besiegt* ("The enemy is defeated"). This is shown in Figure 6.4.

However, unhappy with this he asked Felix Krüger, his trusted assistant from 1901 and later his successor, to really measure intonation patterns, which it had become possible to do. The second, 1904 edition of *Die Sprache* displays the results for a single speaker (see Fig. 6.5).

These new diagrams surprised Wundt, because the pitch range was much larger than he had earlier intuitively estimated. The pitch range in the question, for instance, is as large

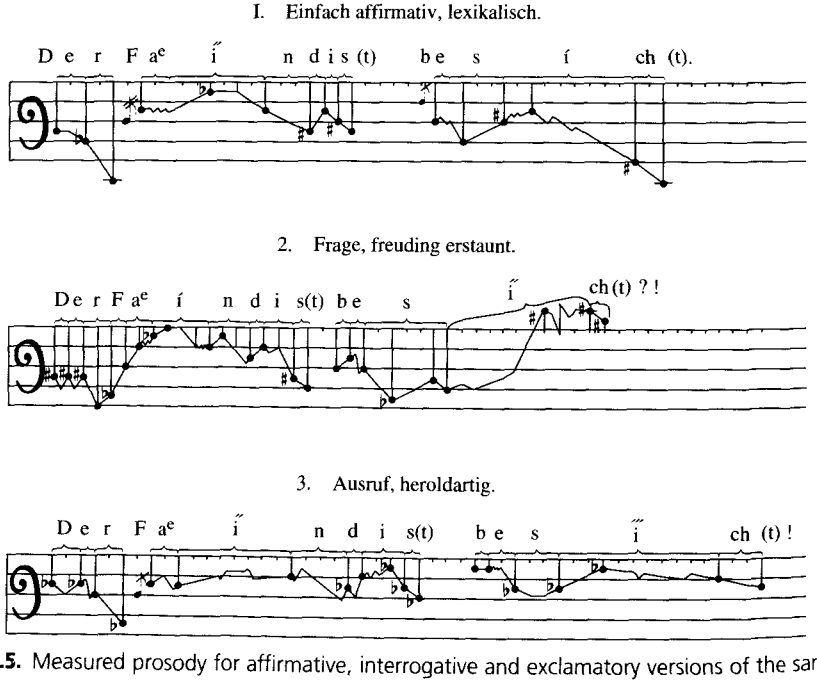


Fig. 6.5. Measured prosody for affirmative, interrogative and exclamatory versions of the same sentence. From *Die Sprache* II, second edition 1904, p. 423.

as a twelfth, which is an octave plus a fifth; in the "intuited" version it was no more than a fifth. These new data also prompted Wundt to introduce the notion of what we now call encliticization. In all three versions of the sentence the auxiliary *ist* attaches to the "main image" *Feind* to form a single word unit *Feindist*.[68] The same holds for the article *der* attaching to *Feind* in the second, interrogative version of the sentence.

The sentence–final pitch movements provided Wundt with the opportunity to stress that it is the intensity (Osgood's potency) of affect that causes the speaker's pitch movement. The quality of the affect, in particular the dimension of pleasantness, is irrelevant here. The falling sentence-final tone of the declarative sentence expresses the "calming down" that goes with a feeling of certainty or conviction. The rising pitch of a question expresses a rising tension, which works at the same time as a stimulus for the interlocutor.

Outer and internal speech form

Wundt followed the general practice since Humboldt and Steinthal of distinguishing outer and inner or internal speech form. The outer form is the sum total of word form and syntax.[69] The internal language is whatever psychological factors determine the outer forms and processes. But in contrast to Humboldt, Wundt rejected the idea that the outer forms of the world's languages can be scaled in one-dimension from (rather) formless to "ideal," with Greek in top position. Instead he proposed that languages vary in their outer form along some 12 dimensions or opposites.[70] Among them are "isolating" versus "agglutinative," "nominalizing" versus "verbalizing," "complex" versus "simple" morphology, "prefixing" versus "suffixing," "attributive" versus "predicative," "free" versus "fixed" word order. Some of these parameters tend to correlate. Agglutinative languages, for instance, tend to have complex morphology and attributive syntax. Any of the parameter values can be optimal for a particular style of thinking or a particular type of inner speech. For example, there are two quite different styles of thinking that are naturally expressed in an isolating outer form. The one is really primitive "fragmentary" thought, as in children and Bushmen (according to Wundt), where each fragment is immediately expressed in an uninflected word. The other one is "discursive" thought, in which both the parts of the total image and the relations holding among them are expressed by independent words and different parts of speech (nouns, verbs, adjectives, adverbials, and particles). A language community where "object-centered" thinking is dominant will be best served with an attributive language, whereas "state-centered" thinking is best served with a predicative language. Similarly, more concrete versus more abstract thinking goes, optimally, with rich-versus-lean case-marking morphology.

The preferred style of thinking in a language community normally evolves in the course of history. The outer language form is, however, rather conservative; tradition

[68] Now called a "phonological word."

[69] To outer speech also belongs articulatory phonetics in as far as it is not genetically fixed. Wundt 1900a, Vol. II, p. 403.

[70] "Parameters" is the current expression. (See Wundt's parameter set ibid.)

governs this. As a result, the outer language may become a less-than-optimal expression for what is going on in internal speech. Here Wundt takes a rather non-relativistic position. Any style of thought can find expression within any given parameter setting. "A language can . . . maintain its outer form, even when the inner speech form has long transcended it."[71] In other words, styles of thinking greatly vary among language communities ("nations"). Genetically speaking, they have caused a rich variety of outer speech forms in the world's languages. But these outer forms do not, in their turn, limit or rule out varieties of thought that have moved beyond them.

The origins of language

In the final chapter of *Die Sprache*, Wundt addresses the oldest and most prominent issue in nineteenth-century language scholarship: how did language originate? Wundt would not be Wundt without comprehensively reviewing every single theory—from the Stoics and Plato to Steinthal and Schleicher—and without rejecting them all. He ridicules the idea that there has ever been a pure root language (as Sanskritists had suggested), that these roots expressed actions (as Max Müller had claimed), that these actions were the meanings of the sounds expressed in joint activities (as Noiré had proposed);[72] all this was dismissed. The same fate befell Steinthal's interjectional reflex theory and Schleicher's language-as-organism theory. Darwin's theory of sexual selection had already been crossed off in the earlier chapter on speech sounds. Wundt's only word of sympathy goes to imitative sound painting, some direct rendering of affect in the character of speech sounds (discussed in the section on natural sounds). But all this is of marginal significance.

The real psychological issue is a genetic one, according to Wundt: How did the expressive movements that were natural and adequate at successive stages of development and consciousness become speech sounds and symbols of conscious content? Wundt's answer is two-part, combining a negative and a positive component.

The negative component is this: there has never been a *direct* relation between a speech sound and its meaning; it is *indirect* by nature. This claim has already been discussed: it is the articulatory movement that is directly expressive of affect or meaning, not the resulting sound. Some relation may shimmer through in as far as the intensity of affect is concerned, stronger affect resulting in louder or higher-pitched sounds. But the speech sound is hardly ever iconically depictive of meaning. This indirectness, even arbitrariness of the meaning–sound relation can only be overcome in a theory of language origins by introducing the positive component. The articulatory movement is originally no more than one aspect of the total expressive movement, which is largely a pantomimic one. But this gestural side happens to be *directly* expressive, according to Wundt, as was discussed in the section on sign language. Deictic and imitative or iconic gestures naturally and

[71] "Eine Sprache kann . . . ihre äußere Form festhalten, wenn die innere Sprachform längst darüber hinaus geschritten ist" (Wundt 1900a, Vol. II, p. 412).

[72] How can organized joint activity arise at all without language, Wundt asks rhetorically.

directly express mental content. They are the first carriers of meaning. And they still are, as can be seen in the spontaneously arising natural sign language of deaf people. It is the simultaneity of the arbitrary speech sound and the meaningful gesture that creates the association between sound and gesture, and from there between sound and meaning. They become sound *gestures*, either deictic or imitative ones. Hence, "the originator of a speech sound is not chance, it is originally rather fully determined in its relation to what it denotes by the accompanying mimic and pantomimic movements."[73]

This is the heart of Wundt's theory of origins. There is never a moment where "no language" becomes "language." At each state of development there is always "adequate" expressive movement, with articulation as just one component. As consciousness develops, so does the pantomimic repertoire. By pure association, the speech sounds that are arbitrarily caused by articulatory movements adopt the meanings that are transparently there in the gestures they accompany. It was in order to develop this gestural theory of language origins that Wundt expounded so much effort in his analysis of sign languages, in particular Deaf sign language. It is in sign language that the origins of language can still be observed today, according to Wundt.[74] Everything else is a matter of meaning and sound change, as also extensively discussed in *Die Sprache*. These changes are caused by three major factors: the vicissitudes of oral tradition, language contact, and spontaneous mutations that speakers in any culture display.

Gestural theories of language origins have always been around, even up to the present day.[75] Wundt was the main nineteenth-century proponent of this view. But both then and now at least two riddles are not touched upon by these theories, or at least hardly so. The first is why we would ever have gone for spoken language if already in possession of such a magnificent tool as sign language can be. Wundt does not treat this issue in any depth. The second is why we developed a vocal system of such enormous complexity, with full cortical control.[76] The evolution of such complexity was not considered by Wundt. The original affective articulatory expressions must have been something like *oh*s and *ah*s, or at least must have been of a very limited repertoire. It is these few vocal sounds that had to absorb the full meaning repertoire of a rich pantomimic and mimic expressive system. There is no indication in *Die Sprache* of how new articulations evolved whose sounds could take on that tremendous load. The gestural theory was as much a *deus ex machina* then as it is now.[77]

[73] "Nicht der Zufall ist also Urheber des Sprachlautes, sondern dieser ist durch die begleitenden mimischen und pantomimischen Bewegungen ursprünglich vollständig in seiner Beziehung zu dem, was er bedeutet, determiniert" (Wundt 1900a, Vol. II, p. 608).

[74] And to modern students of sign languages arising in deaf communities, see for instance, Senghas *et al.* (2004).

[75] See, for instance, Rizzolatti and Arbib (1998).

[76] Just about fully lacking in the other apes and vocal monkeys (Ploog 2003).

[77] See Levelt (2004) and Toni *et al.* (2008).

Wundt's psycholinguistic legacy

Die Sprache is a monumental, one-off unification of language sciences. It rounds off the century-long discussion of language genesis in the human mind. Wundt brushes away the many amateurish efforts to provide a psychological base for the explanation of language evolution and use. Neither associative mental mechanics nor reflex theory meet his approval. His is a voluntaristic theory, in which drive and affect are at the steering wheel. The laws of association do not lose their power, but wilful apperception, which we experience as focused attention, keeps order in the realm of consciousness. It is by repeated attentional action that a conscious process becomes automatized. It slowly acquires the character of a reflex, working quickly and unconsciously. All this holds in particular for language use. The adult speaker no longer attends to his own word and sound formations; they run automatically. Adults can focus entirely on the thought to be expressed, successively partitioning it into its main components and their relations.

One way of reading *die Sprache* is as a running psychological commentary on the core chapters of nineteenth-century linguistics. There was no way for Wundt's linguistic contemporaries to ignore this massive psychological "intrusion" into their discipline. Wundt's theories not only became obligatory references in just about any linguistic publication, from Paul to Delbrück, Jespersen, and Bloomfield, but were also the subject of extensive treatment and controversy. Three examples should suffice here.[78]

Hermann Paul, much esteemed, including by Wundt himself, rather thoroughly disagreed with Wundt's psychology of language in at least two respects. Paul rejected the idea that the psychology of language had to be a chapter of ethnic psychology. The exclusive domain of psychology is what happens in the individual mind. "All that enables one individual to affect another one, is not psychological," Paul writes.[79] Language evolution is not psychology but history. It is "mythology" Paul says, for Wundt to claim that there is as much real existence for the "collective mind" (*Volksseele*) as there is for the individual mind.[80] But Wundt responds[81] that for psychology the mind can indeed be nothing other than the whole of an individual's psychological experience. However, the mental products of human coexistence are no less real, even they only exist in the individual minds. That reality is denoted by the notion "collective mind." Paul was also not taken with Wundt's notion of apperception. Paul always remained faithful to Herbartian mental mechanics. His definition of the sentence is, accordingly, an associative one: "The sentence is the linguistic expression, which symbolizes that the association among sundry images of groups or images in the mind of the speaker has come about, and it is the means of

[78] There are many more, such as Sütterlin's (1902) monograph-size review of *Die Sprache* and Dittrich's (1903a) pretentious non-review.

[79] "Alles das aber, wodurch die Wirkung des einen Individuums auf das andere ermöglicht wird, ist nicht psychisch" (Paul, 1880/1975, p. 13).

[80] Ibid. p. 13, footnote.

[81] Wundt 1900a, Vol. I, p. 9.

bringing about the mentioned association of images in the hearer's mind."[82] Wundt, of course, disagreed for various reasons. Here is one: "Whether I render in words the twelve signs of the zodiac or think of them in images, they are in both cases associations forming a whole, but neither way do they form a sentence. Paul's definition would also apply to any old mechanical image association."[83] Paul in his turn responds that his own definition is certainly the correct one for the hearer, who is fully ignored by Wundt. But even from the speaker's perspective there are many exceptions to Wundt's apperceptive definition. If you are asked "Who won?" and you answer "Frederick won," then the question evoked the image of winning, which in turn evoked the image of Frederick in your mind: this is purely associative-sentence generation. Paul's linguistic intuitions are more sophisticated than Wundt's and he makes no attempt to disguise that. Commenting on Wundt's thesis that word order follows conceptual emphasis, Paul remarks that you can "listen to any conversation" to find sentences where the most important or stressed word is not in sentence-initial position, although that would be allowed by the rules of syntax, and so on. But clearly, disagreeing with Wundt is not ignoring him.

Another linguist who did not ignore him was Berthold Delbrück (1842–1922) from Jena. He published a no less than 180-page review of *Die Sprache* in 1901. Wundt, equally prolific, answered the same year with a 110-page rejoinder.[84] Delbrück's review begins with an elaborate comparison of Wundt's voluntaristic psychology and the mental mechanics of Herbart–Steinthal, which was popular amongst linguists. His conclusion is an eclectic one:[85] "for the practitioner it is possible to live with both theories." This was, of course, unacceptable "superficiality" to Wundt: "there can only be one psychological truth. Two different psychological theories of some phenomenon can possibly both be false. That both are simultaneously true or alternatively, as Delbrück formulates, equally useful, that seems impossible to me."[86] Delbrück also triggered another controversy, this time on the origins of speech sounds. Delbrück had become much charmed with Jespersen's song theory of speech origins. As we saw in Chapter 2, this song theory continues the tradition of Herder, Humboldt, and Darwin. For Jespersen, it was amorousness

[82] "... der Satz ist der sprachliche Audruck, das Symbol dafür, dass sich die Verbindung mehrerer Vorstellungen oder Vorstellungsgruppen in der Seele des Sprechenden vollzogen hat, und das Mittel dazu, die nämlichen Verbindung der nämliche Vorstellungen in der Seele des Hörenden zu erzeugen" (Paul 1880/1975, p. 121).

[83] "Ob ich die zwölf Zeichen des Thierkreises in Worten ausspreche oder in Vorstellungen denke: sie sind beidemal Verbindungen zu einem Ganzen, aber sie sind hier so wenig wie dort ein Satz. Pauls Definition würde auf jede beliebige rein mechanische Vorstellungsassociation auch passen" Wundt 1900a, Vol. II, p. 224.

[84] Wundt (1901b).

[85] "Man sieht: für den Praktiker läßt sich mit beiden Theorien leben" (Delbrück 1901, p. 44).

[86] "Wie es aber nur eine geschichtliche Wahrheit gibt, so kann es auch nur *eine* psychologische Wahrheit geben. Zwei abweichende psychologische Interpretationen einer Erscheinung können möglicherweise beide falsch sein. Dass beide zugleich wahr, oder auch dass sie, wie es Delbrück formulirt, beide gleich *brauchbar* sind, das scheint mir unmöglich" (Wundt 1901b, p. 11).

and cheerfulness that invited the first song-like vocal expressions. The lad-in-love would vocalize something like *with a hey and a ho and a hey-nonino.* The others would then, derisively, call him something like *Mister-a-hey-and-a-ho-and-a-hey-nonino.*[87] That is how the first proper names arose. Wundt, in his response, is too serious to drive such fantasies *ad absurdum.* He carefully explains that speech always *precedes* song in primitive cultures and, quite visibly, in the child. Singing is the systematic use of musical intervals, in particular octaves, fifths and fourths. It is not around in any monkey or ape and develops relatively late in the child.[88]

There is, however, a deeper issue involved in the Delbrück–Wundt exchange. It concerns the relation between linguistics and psychology. Historical and comparative linguists had long been groping for the psychological underpinnings of language change and use. Not surprisingly, that is also how Delbrück read Wundt: as a running psychological commentary on core linguistic phenomena. One would expect Wundt to be gratified by this quest for an explanatory psychology; certainly he had gone all out to provide one. But he was not. Wundt, of course, did not deny that psychology can be useful to linguistics, but for him the more important relation between the two disciplines is the reverse one: The focus should be on "gaining psychological insight from the facts of language and above all from language history."[89] It is only through the historical and comparative study of the products of mind, in particular language, that we can gain insight into the "higher," complex workings of the mind. Even more strongly, they will provide us with a much richer picture of our associative, apperceptive, and voluntary functioning than laboratory studies can provide. It is the same type of response that we observed in Wundt's review (also in 1901) of linguist Thumb's experimental approach to analogy and language change. The linguistic facts are much more telling about the complex workings of analogy than those elementary psychology can provide.

It was Wundt's deep conviction that the royal road into the complexities of human thought, we would now say cognition, is through the comparative study of languages. Delbrück, the Indo-European scholar, criticized Wundt for using evidence from (largely) non-literate languages whose history is unknown, such as Yakut, Bantu, and American-Indian languages. But for Wundt it was precisely the study of still-living languages of "primitive peoples" (*Naturvölker*) that can provide us with insights into the varieties of human thought, and he continued making that point till the end of his life. Such languages "are creations of a mental evolution, within which each stage has its significance and where precisely the most primitive stages can be of eminent value with respect to the most general developmental problems."[90] Every such language "represents its own

[87] Delbrück (1901, p. 87).

[88] Here Wundt also takes issue with Darwin's too easy acceptance of a chromatic interval scale in gibbons (Wundt 1901b, pp. 97–98).

[89] "... auf der Gewinnung psychologischer Erkenntnisse *aus* den Thatsachen der Sprache und vor allem der Sprachgeschichte" (ibid. p. 9).

[90] "Sie sind Schöpfungen einer geistigen Entwicklung, innerhalb deren jede Stufe ihre Bedeutung hat und gerade die primitivsten Stufen mit Rücksicht auf die allgemeinsten Entwicklungsprobleme von hervorragendem Werte sein können" (Wundt 1920, p. 211).

characteristic organization of human thought."[91] "Here the psychologically most important data are there for the taking, leaving for psychology only the main task to subsume them under the sum total of psychological laws of development."[92] "Factual thought, which manifests itself in the pluriformity of its linguistic forms of expression, is no less a direct reality than the simplest sensory perception."[93] In short, it is in the domain of language that "the treasure is hidden which is to be unearthed, if we want to acquire a true psychological developmental history of the more complex processes of thought."[94]

Given this principle-based perspective on the study of human cognition and its emergence, it is sad and surprising that, as I mentioned earlier, none of Wundt's PhD students, of which there were at least 186, adopted it in their research. In a sense, Wundt became a loner here, not leaving a school of what we would now call "cognitive anthropology." For linguists and psychologists alike, Wundt's "grand unification" became somewhat like the proverbial elephant for the blind men. *Die Sprache*, at over 1200 pages long, could not be used as a text. It was also too large, and maybe somewhat too Germanic, to be translated into other languages. The best English-language introduction to Wundt's linguistic notions was, and maybe still is, Bloomfield's (1914) *An introduction to the study of language*. "It will be apparent," Bloomfield writes in his preface, "that I depend for my psychology, general and linguistic, entirely on Wundt." As mentioned, it was also through Bloomfield (1887–1949), who had followed Wundt's lectures in Leipzig, that Wundt's innovative phrase-structure analysis was adopted in structuralist linguistics. One more somewhat Wundtian text was to appear on the American scene. This was Pillsbury and Meader's (1928) *The psychology of language*. As mentioned in Chapter 5, Pillsbury had studied with Wundt's students Titchener and Wolfe. His clearly written mentalist textbook is especially remarkable as it appeared in the heyday of behaviorism. Indeed it got a chilly reception (see Chapter 8).

Still, Wundt's enormous influence on the worldwide establishment of psychological laboratories was not matched by his long-term influence as a psycholinguist, which was rapidly diminishing, except in isolated cases.[95] History is not fair. Young Wernicke's 68-page monograph, for instance, stood the ravages of time far better than old Wundt's

[91] "... jede Sprache repräsentiere eine nur ihr eigentümliche Gestaltung des menschlichen Denkens." (Wundt 1920, p. 215).

[92] "Hier find vielfach die psychologisch wichtigsten Ergebnisse geradezu mit Händen zu greifen, für welche dann der Psychologie nur die Einordnung in die Gesamtheit der psychologischen Entwicklungsgesetze als die Hauptaufgabe übrig bleibt" (ibid. p. 213).

[93] "Das tatsächliche Denken, das sich uns in der Mannigfaltigkeit seiner sprachlichen Ausdrucksformen darbietet, ist eben nicht weniger unmittelbare Wirklichkeit wie die einfachste Sinneswahrnehmung" (ibid. p. 218).

[94] "Gerade hier, auf dem Gebiet der Sprache, liegt aber, ..., der Schatz verborgen, der gehoben werden muß, wenn wir in den Besitz einer wahren psychologischen Entwicklungsgeschichte der zusammengesetzteren Vorgänge des Denkens gelangen sollen" (ibid. p. 217).

[95] Most remarkably in Friedrich Kainz's (1941–1969) six-volume *Psychologie der Sprache*, see Chapter 14.

1286-page *Die Sprache*. How come? Maybe applicable here is what Berlioz once famously said about Saint-Saëns: "He knows everything, but he lacks inexperience."[96]

Epilogue: turning the century

Wundt's monumental synthesis perfectly reflects the state of the art in psycholinguistics at the close of the century. A new discipline had emerged from initially disparate roots. It was coined "the psychology of language," a name that was adopted worldwide in the course of the last decade of the century. As any new discipline arising during the nineteenth century, it had strong philosophical underpinnings, in British associationism and faculty psychology, in French rationalism, in Kantianism, in idealist voluntarism and in positivistic materialism.

Wundt's perspective was a genetic one. There he did not differ from Steinthal, who had been the first psycholinguistic theoretician. Their diachronic linguistics was genetic; their psychology had to account for the grand evolutionary phenomena of sound and meaning change. Their psycholinguistic process theory was also genetic: how does the speaker generate sounds, words, and especially sentences? And their approach to the ontogeny of language was genetic almost by nature. But Wundt's psychology was more advanced, adding the attentional, volitional processes of apperception to the classical mechanisms of association. It was also the ethnic psychology, promised by Steinthal but never delivered. The evolution of thought and language can still be read today in exotic cultures; this was Wundt's claim and grand research endeavor.

Wundt was as much an empiricist as a theoretician. His synthesis covered most of the empirical literature we discussed in the previous chapters. Here is a summary of these early empirical roots of our discipline.

Maybe the oldest empirical issue was the acquisition and use of sign languages in deaf communities, with the French educational system as its main impetus. Sign language was never off the agenda during the century. Historical and comparative linguistics shaped the initial discussions about language evolution and its psychological underpinnings, with Humboldt, Rask, Bopp, and Grimm as pioneers, and with Steinthal inventing a systematic psychological account of language origins and use. The emergence of serious brain anatomy, early in the century, triggered another major development. Gall, its pioneer, put on the agenda the localization of mental faculties, in particular the faculties of language and speech. It was Wernicke, half a century later, who introduced the new approach of the "diagram makers," relating psychological components of the faculty of speech to specific localizations in the brain. Arising independently were the scattered diary studies of children's development; language acquisition emerged. Diary studies suddenly became a fashionable new methodology after Darwin presented his own diary in 1877. The mechanics of speaking had been on the empirical agenda since Kempelen's late eighteenth-century engineering approach. The emergence of the experimental study of speech and language began with Donders' reaction-time measurements in Utrecht and

[96] "Il sait tout, mais il manque d'inexpérience."

reached full swing with Cattell's work in Wundt's Leipzig laboratory and with the Erdman/Dodge approach to the study of reading. And finally, there was the launching of speech-error research as a window on the speaking mechanism.

Increasingly, these developments had touched and merged. A loose but lively international community was formed, with multifarious exchanges in person and writing. Translation among the leading languages—German, English, and French—became common practice for important publications. And soon, a first post-Wundtian text of language psychology appeared. This was Ottmar Dittrich's *Grundzüge der Sprachpsychologie* ("Foundations of language psychology") Volume I. A linguist, Dittrich (1865–1952) was temporarily Wundt's assistant in Leipzig. He dedicated his handbook to Wundt "in adoration and gratitude." That sentiment was probably not mutual. The 800-page volume introduced all of general psychology except for language. It was, moreover, a prototype of scholastic logorrhoea, unreadable and impenetrable. Happily, the second volume never appeared. Dittrich's slim 1913 booklet has one quality, namely his plea to involve both hearer and speaker in the psychology of language. For the rest, it is, perhaps, better forgotten. As mentioned, better textbooks were soon to appear in the United States, among them Bloomfield's (1914).

The century definitely had an upbeat ending for this interdisciplinary enterprise. Many shared Wundt's hope and expectation that these different disciplines, in particular linguistics and psychology, would become unified in the minds of individual researchers.[97] There was, indeed, sufficient theoretical commonality to feed such hope. Although every psychologist of language had his own good reasons for disagreeing with Wundt, nobody could ignore him. Wundt's mentalistic, genetic framework was on everybody's mind.

This was soon to change drastically, however. Synchronic structuralism would loosen the ties between linguistics and psychology. The emergence of behaviorism in the United States would create a chasm with the ongoing European mentalist approach to language and mind. And, not least, two world wars would irreversibly split the psycholinguistic academic community. That story will be told in the next, twentieth-century, part of this book.

[97] Wundt (1901b, p. 110).

Part 3

Twentieth-century psycholinguistics before the "cognitive revolution"

The global, relatively homogeneous, psycholinguistic community was granted only a short life. Soon, synchronic structuralism would loosen the ties between linguistics and psychology. "We can pursue the study of language without reference to any one psychological doctrine," Bloomfield wrote in 1933.[1] This was the same Leonard Bloomfield who, as a faithful Wundtian, had written: "To demonstrate in detail the role of language in our mental processes would be to outline the facts of psychology."[2] The emergence of behaviorism in the United States was going to create a chasm with the ongoing European mentalist approach to language and mind. In Bloomfield's words: "It remains for linguists to show, in detail, that the speaker has no 'ideas,' and that the noise is sufficient – for the speaker's words to act with a trigger-effect upon the nervous systems of his speech-fellows."[3] There was hardly any common ground left in Karl Bühler's (1934) and Jacob Kantor's (1936) major theoretical treatises, Bühler the leading European psycholinguist, Kantor the most radical behaviorist psycholinguist.

And, not least, two world wars would irreversibly split the psycholinguistic academic community. Narziß Ach, Karl Bühler, and Otto Selz were leaders in the new "psychology of imageless thought," which had emerged in Würzburg before World War I. Narziß Ach prostrated himself before Hitler in 1933, Karl Bühler was dismissed from Vienna University in 1938 and emigrated to the United States. Otto Selz was killed in 1943, on the way to Auschwitz. Over a century of German leadership in language research was tragically terminated. The center of gravity definitively shifted to the Anglo-Saxon world.

[1] Bloomfield (1933/1976), p. vii.

[2] Bloomfield (1914), p. 56.

[3] Bloomfield (1933/1976), p. 93.

Chapters 7 to 14 tell the story of these developments. Chapters 7, 8, and 9 focus on the main theoretical innovations. The emergence of structuralism in linguistics and the new "Wurzburgian" psychology of imageless thought and consciousness before World War I are discussed in Chapter 7. Chapter 8 sketches the half-century of anti-mentalistic behaviorist approaches to psycholinguistics, a curiously dogmatic and almost entirely United States affair. Chapter 9 treats the parallel European functionalist approaches to psycholinguistics, with Karl Bühler as its central representative.

We then turn to empirical research, extending the nineteenth-century "roots" up to the so-called "cognitive revolution" of the late 1950s. Research in language acquisition, language in the brain, and adult language usage are the topics of Chapters 10, 11, and 12, respectively. Chapter 13 continues the cross-linguistic nineteenth-century root in its treatment of "linguistic relativism" and the emergence of a new experimental paradigm.

This part of our history ends with an overview of the tragic repercussions in psycholinguistics, and in particular in the fate of its leaders, during the Third Reich. It left the discipline in a disarranged state.

Chapter 7

New perspectives: Structuralism and the psychology of imageless thought

This chapter will present two new perspectives, in linguistics and psychology respectively, which emerged during the first decade of the twentieth century. These perspectives, structuralism and the psychology of thinking, were to have long-lasting effects on psycholinguistics. Structuralism would enrich psycholinguistics with the notion that language is a system of rules and structures in the mind. Proponents of thought psychology, in particular Otto Selz, developed the notions of "sentence schema" and "mental operation," which became foundations of computational psycholinguistics. The two innovations eventually fused in generative psycholinguistics, where linguistic rules were related to the mental operations of sentence parsing and production.

Emerging structuralism: Taine, Baudouin de Courtenay, and Saussure

In 1870 Hippolyte Taine (whom we have already met as the first to publish an acquisition diary in *Mind*) published his three-volume book *De l'intelligence* ("On intelligence"). The 19th and last edition of this popular book appeared in 1914. The first four chapters of volume I concern the structure and use of signs, "semiology" as Saussure was going to call it. Signs, according to Taine, are always associations of two "experiences." You perceive smoke distantly rising in the field and you imagine immediately the fire the farmers are stoking up. One image or idea suggests the other, the former is the sign for the latter. Names are special cases of signs. If you hear or read the word *Tuileries*, it will invoke some vague image of a park with statues and fountains. At least, this is what will happen to millions of Frenchmen. But now, Taine continues, if you read something like *Paris has many public gardens, such as the Jardin des Plantes, le Luxembourg, le Bois de Boulogne, les Tuileries, les Champs-Élysées, . . .*, not much of an image arises when scanning over the word *Tuileries*. Only one of the sign's components, the image of the written word, becomes conscious. The word alone provokes to some extent the tendencies that the full image of the park normally provokes. If you read a listing of London's public gardens, such as *Hyde Park, Regent's Park and the Tuileries*, then the "pulls and pushes"[1] provoked by the word *Tuileries* suddenly make you surprised, because they do not align with those of *Hyde Park* and *Regent's Park*. The vague visual image of the *Tuileries* does appear. "In this way," Taine concludes, "the name alone can substitute for the called-up image and as a

[1] "des affinités et des répugnances" (Taine 1870/1914, Vol. I, p. 29).

Fig. 7.1 Ferdinand de Saussure.

consequence for the experiences that go with it; it deputizes for them and it is their *substitute.*"[2] This is exactly what Steinthal had claimed. For him, as we saw in Chapter 2, the word is the sign in consciousness, which connects to an unconscious meaning complex, the "apperceptive mass." That is where the pulls and pushes of association take place. It was Taine's elegant exposé of sign phenomenology rather than Steinthal's stodgy formulations which inspired Parisian linguistic circles, in particular the Société Linguistique de Paris, of which Ferdinand de Saussure became a member.

The young Genevan linguist Ferdinand de Saussure (1857–1913, Fig. 7.1) went to Leipzig in 1876, where he studied linguistics, supervised by such Neogrammarians as Curtius and Leskin, close linguistic colleagues of Wilhelm Wundt (who had also just started in Leipzig). Saussure's 1880 dissertation was on the Sanskrit absolute genitive. Meanwhile he also published a major and influential book on the original Indo-European vowel systems. Immediately after his doctorate Saussure moved to Paris, where he studied and taught at the École des Hautes Études till 1891. It is probably there that he drew inspiration from Taine's work, in particular his theory of signs. From Paris Saussure returned to Geneva, where he was to stay till his death in 1913.

Saussure became highly influential through a book he did not write himself, the *Cours de linguistique générale* ("Course in general linguistics") of 1915. The book was compiled

[2] "De cette façon, le nom tout seul peut tenir lieu de l'image qu'il éveillait, et, par suite, de l'expérience qu'il rappellait; il fait leur office et il est leur *substitut*" (Taine 1870/1914, Vol. I, p. 5).

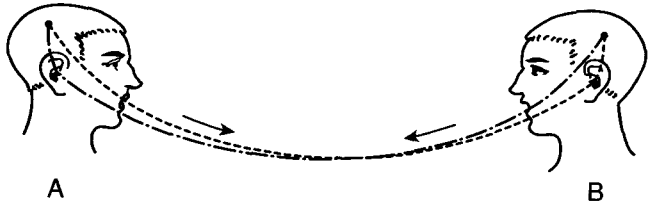


Fig. 7.2 Diagram of the signing circuit in the *Cours*. Reproduced from Ferdinand de Saussure, *Cours de linguistique générale*, p. 27 © 1916, 1972, 1985, Editions Payot; © 1995 Editions Payot & Rivages with permission.

by Charles Bally and Albert Sechehaye from available lecture notes. These were notes made by Saussure's students and only a few of his own notes, because he always destroyed them straight after use. Both editors had been close to Saussure and were familiar with his thinking. That does not mean, however, that their rendering of the notes was entirely faithful. It was not until both Bally and Sechehaye were dead that the original notes were retrieved from the Genevan library by Godel (1966), who then published a careful analysis of what he found and how the notes related to the published *Cours*. The differences in both organization and theory were substantial. Harris (2001) provides a full account of this creative editorial performance. He also shows how both editors in their later work refer to self-produced statements in the *Cours* as Saussure's. It is most regrettable, therefore, that Saussure, who was haunted by depression, never managed to write his own monograph, so we do not know precisely what he would have written. For the history of psycholinguistics, however, the text of the *Cours* has been the relevant source, not Saussure's unpublished opinions. Hence, I will make reference to the *Cours* instead of to Saussure when I discuss the new structuralist views expounded.

"Everything in language [*langue*] is basically psychological." (p. 6).[3] Language [*langue*] is "a system of distinct signs corresponding to distinct ideas." (p. 10). *Langue* has to be distinguished from the facts of speech (*langage*) in the way depicted in Figure 7.2.

"A given concept unlocks a corresponding sound-image in A's brain. This purely *psychological* phenomenon is followed in turn by a *physiological* process: the brain transmits an impulse corresponding to the image to the organs used in producing sounds. Then the sound waves travel from the mouth of A to the ear of B: a purely *physical* process. Next the circuit continues in B, but the order is reversed: from the ear to the brain, the physiological transmission of the sound image; in the brain the psychological association of the image with the corresponding concept. If B then speaks, the new act will follow" etc. (pp. 11–12).

Language (*langue*) only concerns the psychological parts of the circuit: the concepts, the sound images, and the relationship between them. The *Cours* denotes the concepts as *signified* and the sound images as *signifier*. Together they form a sign. A linguistic sign's two components entertain an *arbitrary* relation. These coupled pairs of concept and sound image are social products, to be carefully distinguished from individual products.

[3] English citations are from *Course in general linguistics* (1959), translated and edited by Wade Baskin.

The executions of moving from concept to sound image in the speaker and from sound image (the "signifier") to concept (the "signified") in the hearer are individual products. This individual executive side is called *parole* in the *Cours*. In other words, *langue* is the store house of social codes, linguistic signs, which reside in individual brains, or rather the sum of all these store houses in a language community. *Parole* is the individual act of going from concept to sound-image or vice versa. To complete the picture, the *Cours* distinguishes a third notion, that of *langage*, which means the sum total of human language abilities. It is put aside as too diverse and involved to be studied systematically. What remains is the pair of psychological notions, *langue* and *parole*. When we translate those, following the tradition, as "language" and "speech," it must be kept in mind that "language" is the set of socially shared signs (*faits socials*) and "speech" is the process where the individual turns the sound image (or printed word image) into a concept or vice versa; it does not cover the physiology of articulation or audition, which is all outside psychology according to the *Cours*.

The object of linguistics is language, not parole. "Language is a system whose parts can and must all be considered in their synchronic solidarity." (p. 87). This is a second main distinction made in the *Cours*, that between the synchronic and diachronic study of language. "*Synchronic linguistics* will be concerned with the logical and psychological relations that bind together coexisting terms and form a system in the collective mind of speakers. *Diachronic linguistics*, on the contrary, will study relations that bind together successive terms not perceived by the collective mind but substituted for each other without forming a system." (pp. 99–100). Synchronic or static linguistics, the *Cours* claims, is "much more difficult than the study of historical linguistics" (p. 101). It is by no means easy to delineate the here and now of a language state. With its emphasis on synchronic linguistics, the *Cours* drastically departed from the views of Hermann Paul and his many followers. According to Paul, explanation in linguistics could *only* be diachronic.[4]

A third important distinction the *Cours* introduced was between syntagmatic and paradigmatic relations. "In a language-state everything is based on relations." (p. 122). The elements in a discourse entertain a syntagmatic relation. They are the relations among sounds in a word, words in a sentence. The "value" of an element depends on the values of the other ones that precede or follow it. This is especially apparent in fixed expressions, such as "break a lance." Syntagmatic relations often display regular patterns. But sounds or words also entertain paradigmatic relations, outside of discourse. Words in the "storehouse" often relate in terms of their meanings. The word "education," for instance, relates to the word "apprenticeship." This is a paradigmatic relation, which is associative in character. Other paradigmatic relations are morphological in character, such as *painful, delightful, frightful*. They also are of an associative nature.

It is, of course, a main task of synchronic linguistics to characterize these existing types of syntagmatic and paradigmatic relations that figure in a particular language state. The *Cours*, however, does not provide much detail. There is no shimmering of syntax, there is the occasional morphological example and there is some phonology, originally

[4] According to Harris (2001), this interpretation of the *Cours* editors deviates substantially from Saussure's own views.

Saussure's main research area. At all these levels one can distinguish units or elements, but it is ill-conceived, according to the *Cours*, to consider the state as just the set of its units. Units have values that derive from the system as a whole. They are like chess pieces whose material make-up is irrelevant. It is the chess game that endows its pieces with their specific values. The *Cours* is keen on a particular kind of value, a negative value. In phonology the units, i.e., phonemes, are characterized by being *distinct.* They "are above all else opposing, relative and negative entities." (p. 119). So, in French, there is no opposition between glottal and tongue-tip trill *r*; one can use them interchangeably in spite of their huge articulatory difference. In fact, one could as well use the German *ch.* But in German *r* and *ch* are opposed; hence they have different values in the system.

The *Cours* is very explicit in considering language as a psychological phenomenon. The signs and their relations are social codes in the first place. However, their functioning in the minds of speakers or listeners is outside the scope of linguistics and the *Cours* is silent about that functioning. Taine's phenomenology of the mental life of signs is lost in the *Cours.*

The *Cours* proposes "a science that studies the life of signs within society," which shall be called "semiology." Linguistics, according to the *Cours*, is only part of this semiology; it "will circumscribe a well-defined area within the mass of anthropological facts." (p. 16). From the description in the *Cours* it appears that the new science of semiology differs hardly at all from Wundt's ethnic psychology (*Völkerpsychologie*).

According to tradition, the *Cours* marks the birth of *structuralism.* That is an acceptable christening as long as we realize that the term does not appear in the *Cours*, nor for that matter does the term "structure." Rather the *Cours* treats language as a *system*: "for language is a system of pure values which are determined by nothing except the momentary arrangement of its terms." (p. 80). The values of the elements derive from their arrangement in the total system. That arrangement came to be called "structure" in later developments of structuralism. A structuralist studies a system's synchronic state of affairs, the momentary arrangement of its terms. The *Cours* became a landmark for a change in perspective that was taking place around the turn of the century, a change from the dominant genetic, evolutionary perspective of nineteenth-century language sciences to the static system-perspective that became a dominant hallmark of twentieth-century approaches. Parallel developments were taking place in psychology. Titchener, Wundt's student, became the father of "structural psychology" in America, experimentally studying the structure of mind and the contents of consciousness (traditionally sensations, images, and feelings). Gestalt psychology and related *Ganzheit* (totality) psychology stressed the primacy of the whole (in particular the whole perceptual organization) over its parts or elements. When you see a face, you see a face in the first place, not the sum of eyes, ears, nose, mouth, etc. These elements derive their (visual) values from the structure, the Gestalt as a whole. Wundt's theory of the sentence was essentially structuralist. The total image has primacy, its further partitioning, the values of subject and predicate, etc., derive from the image as a whole. The primacy of the sentence over the word is a structuralist notion, interestingly absent in the *Cours*. Primacy is given instead to the word sign.

In fact, syntax was by no means the playground of emerging structuralism. The playground was actually phonology. Saussure used the term "phoneme" in his 1879 book on the primordial Indo-European vowel system[5] and so did Baudouin de Courtenay in his extensive studies of phonology, which began in the 1870s and continued into the 1920s. It had been Baudouin de Courtenay's student Kruszewski who had picked up the term from reading Saussure's book. We have already met Baudouin de Courtenay (in Chapter 4) as the pioneer of the systematic linguistic diary, but he is also recognized as the founder of modern, structuralist phonology. Mugdan (1984) provides an excellent review of Baudouin de Courtenay's phonology, from which the following remarks are derived. In Baudouin's (ever developing) system, the phoneme is the memory representation of the speech sound, the "mental substitution of the speech sound."[6] The same phoneme can become realized as a multitude of speech sounds, without losing its identity. The speaker's phonetic intention is guided by the phoneme, but its articulatory realization is subject to a variety of factors. The German speaker, for instance, will realize the phoneme *d* of the word *Rad* ("wheel") as a *d* sound in *des Rades* ("of the wheel") but as a *t* sound in *Rad*, as is the German habit in word-final position. Similarly, the pronunciation of a particular vowel often varies with the consonant that precedes or follows it in the syllable. These are "divergent" articulations of the same phoneme, according to Baudouin, thus introducing the notion of what we now call "allophonic variation." Baudouin also elaborated the further analysis of phonemes in terms of their articulatory features. Among them are their place of articulation (such as lips or palate), their manner of articulation (fricative, plosive), voicing versus non-voicing, and so on. Baudouin presented the phonemes of a language and their features in systematic tables, which were so daunting for some readers that they were likened to chemical formula.[7] Speech sounds function in articulatory syllables, but phonemes relate to "morphemes," a term also introduced by Baudouin. Phonemes derive their values in a language from the morphemes in which they function. The morphemes, in turn, function in the context of word building (morphology) and meaning. Here one would expect Baudouin to develop the notion of phoneme and feature *distinctiveness*, such as, for instance, the role of phonemic voicing in distinguishing *big* from *pig*. That, however, did not dawn on Baudouin. It only became a core notion in the structuralist phonology of Trubetskoy of the Prague School (see Chapters 9 and 10).

Structuralism and the psychology of language: Sechehaye

The emerging structuralist perspective affected the psychology of language in both positive and negative ways. On the positive side was the increasing insight that language users

[5] The term "phoneme" had been introduced by Dufriche-Desgenettes in a paper presented to the Société de Linguistique in Paris on May 24, 1873 as a general term for speech sounds, whether consonant or vowel (cf. Abercrombie 1991, pp. 24–25). See Mugdan (1985) for the early Polish uses of the term.

[6] Cited in Mugdan (1985, p. 68).

[7] Ibid. p. 72.

Fig. 7.3 Albert Sechehaye. © BGE, Centre d'iconographie genevoise.

have access to a regular system of grammatical rules, a "marvelous instrument,"[8] whose existence supersedes the individual, but is at the same time psychological in nature. The best original formulation of what we now call "linguistic competence" was presented by Albert Sechehaye (1870–1946, Fig. 7.3), one of the editors of the *Cours*, in his own earlier book on theoretical linguistics (1908). Sechehaye, born in Geneva, had Saussure as his teacher from 1891 to 1893. He then left Geneva for Göttingen, where he wrote his dissertation. After 10 years he returned to Geneva to teach linguistics and ultimately, late in his career in 1939, he became Professor of Linguistics.

The starting point of his 1908 book is Wundt's *Die Sprache*. "Only a master is capable of writing such a powerful book,"[9] but then Sechehaye becomes quite critical. Wundt, according to him, handled all linguistic problems that seemed to be of psychological interest, but neglected everything in the domain of grammar. "In a word, he didn't understand the importance of the grammatical problem."[10] What is this grammatical problem? It is to characterize the language user, "as far as he submits to the laws of his language."[11] A genuine psychology of language cannot exist without systematic treatment of this issue. "Grammar exists in our mental and physical life in just the same way as all other factors

[8] "ce merveilleux instrument" (Sechehaye 1908, p. 41).

[9] "Seul un mâitre était capable d'une oeuvre aussi puissante" (ibid. p. 21).

[10] "Il a abordé tous les problèmes de la linguistique qui lui semblaient présenter un interêt psychologique, et il a négligé tout ce qui lui a paru être plus spécialement du domaine de la grammaire; en un mot, *il n'a pas compris l'importance du problème grammatical*" (ibid. p. 23).

[11] "... c'est l'homme parlant, en tant qu'il subit les lois de son langage" (ibid. p. 24).

that compete in the production of speech, and wanting to make abstraction of it would be purely arbitrary."[12] It is "a known means of expression" for the speaker, a "product of custom."[13] To talk about customs is to talk about rules. Each such rule has a real, though abstract, existence.[14] It is only in their totality that these rules constitute a means for the expression of thought, because together they form *a system.*[15] This system resides in the nervous centers of the individual. But it is precisely this conventional system "which escapes the direct influence of speakers."[16] We received it from the community, which is a collection of individuals; the language system exists by consensus. How such grammatical consensus evolves and is maintained within a community is a matter of "collective psychology" (i.e., social or ethnic psychology). It is "a kind of second-degree psychology."[17] That system of conventions or rules is "embedded" in our individual psychology, which means that we can use it, but not act upon it. It forms a "tissue of language habits." Let us cite this core passage in full:

> ..all our phrases are the products of activity of these two factors: automaticity and free spontaneity, which combine in infinitely variable proportions. Even the most automatic ones are not simple brute repetition, simple echo of a phrase already produced or heard: they result from a voluntary intellectual act, which edges its way through a tissue of language habits. On the other hand and inversely, those that are uttered with clear consciousness of all their parts and their composition, are nevertheless a product of psychological automatism to the degree that they borrow their elements from an organized language. It is not pure creation, but rather triggering of certain acquired dispositions. The sign responds to the idea by the working of a reflex, by virtue of habit.... Without automaticity language would be condemned to eternal resumption; it would always stay in its infancy, and it is only thanks to this [automaticity] alone that the complex mechanism of our propositions and our sentences is possible.[18]

[12] "La grammaire existe dans notre vie psychique et physique au même titre que tous les autres facteurs qui concourent à la production de la parole et c'est pur arbitraire que d'en vouloir faire abstraction" (Sechehaye 1908, p. 25).

[13] "... *c'est la mise en oeuvre d'un moyen d'expression connu;* c'est un produit de l'habitude, c'est d'ordre grammatical" (ibid. p. 31).

[14] "Qui dit habitude dit règle, et tout règle expérimentalement constatable a une existence réelle, bien qu'abstraite" (ibid. p. 24).

[15] "*un système* de réprésentations de sons et d'idées" (ibid. p. 23).

[16] "La convention, c'est justement ce qui échappe à l'influence directe des sujets parlants" (ibid. p. 26).

[17] "C'est une sorte de psychologie du second degré" (ibid. p. 53).

[18] "... toutes nos phrases sont des produits de l'activité de ces deux facteurs: l'automatisme et la spontanéité libre, qui se combinent dans les proportions infiniment variables. Mêmes les plus automatiques, ne sont pas la simple répétition brute, le simple echo d'une phrase déjà entendue; elles sont le résultat d'un acte intelligent et volontaire se frayant un chemin à travers un tissu d'habitudes de langage. D'autre part et enversément, celles qui sont au contraire émises avec une conscience lucide de toutes leurs parties et de leur agencement, sont cependant un produit de l'automatisme psychologique dans la mesure où elles empruntent leurs éléments à une langue organisée. Il n'y a pas création pure, mais déclenchement de certaines dispositions acquises. Le signe répond à l'idée par le jeu d'un réflexe en vertu d'une habitude. [...] Sans l'automatisme la langue serait condamnée a un éternel recommencement; elle serait toujours dans l'enfance, et c'est grâce à lui seul que l'organisme compliqué de nos propositions et de nos périodes est possible" (ibid. pp. 39–40).

So, where for Wundt sentence production is primarily the voluntary, conscious activity of partitioning a total image, Sechehaye stressed the role of "underground" automatisms, linguistic habits endowed in the individual by the language community. These conventions are the rules of grammar, which form a coherent system. The linguist's objective is to study this system. A genuine psychology of language must fully incorporate that system in its account of the language user. Sechehaye's treatment of the relations between linguistics and psychology are a lot more explicit and sophisticated than what we find in the *Cours*. Their common core is the system perspective on linguistics. Language is a complex social convention but one subject to laws; these laws or rules form a coherent grammar. It should be noticed that, practically, the linguist's grammar in those days was largely morphology and phonology/phonetics. But, like the *Cours*, Sechehay's first book provides only scant account of what is systematic in morphology or phonology. It is a *program* for synchronic linguistics in the first place and that is indeed what its title says. It would take Sechehaye till 1926 to develop his own systematic account of syntax.[19] In this publication he is true to his program. He presents the major syntactic means available to the speaker for the expression of his thoughts, such as coordination, subordination, and complementation, the subject–predicate relation (here following Wundt), and the means to express entities, events, qualities, manners, and relations. At the same time he sketches the tension existing between these traditional, even conservative, grammatical devices, which reside in us as automatisms and the rich creativity of thought they have to serve. The result is perpetual compromise.

Structuralism also had a negative effect, increasingly divorcing linguistics from its traditional psychological base. The *langue* of linguistics, although represented in the brains of the language users A and B, is primarily a social code, according to the *Cours*. Linguistics becomes the independent science which dissects that relatively stable and arbitrary code. It is a system on which the individual hardly exerts any lasting effects. It can and should be studied *sui generis*. Or, as Sechehaye puts it: "within collective psychology there is a linguistic science, which is the true science of language and which cannot be confounded with anything else." And "we consider it as an autonomous part of collective psychology."[20] Sechehaye calls it a "deductive science:"[21] "Each grammatical procedure can abstractly be considered in algebraic fashion."[22] It was also Sechehaye who wrote the famous last sentence of the *Cours* on the autonomy of linguistics: "The true and unique object of linguistics is language studied in and for itself." (*Cours*, p. 232).

And indeed, the great structuralists did establish synchronic linguistics as an independent, deductive science. None of them explicitly denied a psychological matrix of language,

[19] Sechehaye (1926).

[20] "Il y a donc à l'intérieur de la psychologie collective une science linguistique, qui est la science même du langage organisé et qui ne se confond avec rien d'autre." (Sechehaye 1908, p. 100). ". . . et nous la considérons comme une partie autonome de la psychologie collective." (ibid. p. 101).

[21] Ibid. p. 100.

[22] "Tout procédé grammatical peut donc par abstraction être considéré algébriquement." (ibid. p. 117).

but for many it became irrelevant in linguistic practice. We have already mentioned that move in Bloomfield's work, but it also holds for Hjelmslev's *glossematics*, which was a "flight into abstract space,"[23] as well as for Pike's *tagmemics* and for Harris's beginning generative syntax. In fact, Sechehaye himself formed an admirable exception, always "embedding" his linguistics in a wider psychology of language.

Another consequence of separating linguistics from psychology was a diminished awareness among structural linguists of the biological underpinnings of language. Linguistics became an entirely social science. Language is primarily a social code. Its dependence on human biology, a hot theme in the nineteenth century, faded from the linguistic agenda. "Abnormal conditions in which speech is disturbed, seem to reflect general maladjustments or lesions and to throw no light on the particular mechanisms of language," Bloomfield (1933, p. 34) writes.

Let us return to the *Cours.* Tradition has elevated it as the first landmark of structuralism and Saussure as the mastermind behind it. We have, however, seen that it not only had its precursors, but more importantly that Sechehaye's 1908 book already provided the outlines of the structuralist program. That book is not only a more balanced and comprehensive statement of the program than the *Cours*, but provides a clear theoretical frame for the psychology of language. We also know that Sechehaye's contribution to the editing of the *Cours* has been substantial. One wonders who, after Sechehaye's return to Geneva in 1902, was teaching whom. Sechehaye's role has definitely been underestimated.

Parisian structuralism and Henri Delacroix

When Saussure was still in Paris (till 1891), one of his students was Antoine Meillet (1866–1936). The two kept in friendly contact till Saussure's death in 1913. Meillet was professor at the Collège de France from 1905 and became the leading structuralist linguist in Paris. For Meillet psychology was only of secondary significance to linguistics. Language is a social phenomenon in the first place, a system of linguistic conventions. The individual mind does not contribute much to language maintenance and change. The child acquires language by imitation and the average adult largely expresses himself by means of stock formulas. However, his prolific student Gustave Guillaume (1883–1960)[24] took a broader, Humboldtian view. Language is an activity. A first step in the act of language is the creation in the speaker's mind of an intended message. One of the core linguistic issues, then, is how such a message gets represented linguistically. This involves, on the one hand, the selection of appropriate words ("lexemes") and, on the other hand, the grammatical shaping of the linguistic expression. Since both processes are highly language-dependent, the speaker's conceptualization must be language-dependent. Guillaume's (1933) is an early formulation of what Slobin (1987) would call "thinking for speaking."

Henri Delacroix (1877–1937) was a philosophical psychologist, professor at the Sorbonne since 1919 and dean of the faculty of letters there from 1928. Delacroix had

[23] Seuren (1998, p. 161).

[24] Not to be confused with psychologist Paul Guillaume, who wrote a book on imitation, see Chapter 10.

been a student of Bergson at the Lycée Henri IV. His dissertation was on mysticism in fourteenth-century Germany. It was, among others, supervised by ethnologist Lévy-Brühl. Under the influence of William James, Delacroix published on religious experience and the psychology of mysticism. He was definitely not an experimental psychologist and hardly an empirical psychologist. This is quite apparent from his major work *Le langage et la pensée* ("Language and thought"), which appeared in 1924. He was, however, much influenced and inspired by Saussure and in particular by Meillet, his colleague at the Collège de France. The book had a double purpose. The first was to convince psychologists of language that they cannot do without linguistics: "There is no psychology of language without recourse to linguistics"[25] is the book's opening sentence. The second was to convince linguists that they cannot do without a psychology of language. "Linguistics necessarily calls for a psychology of language."[26] The latter was doubtless triggered by the structural linguists turning to sociology and history for their explanations. This is misguided, according to Delacroix. As soon as linguistics goes beyond the mere facts, "as soon as it attempts to explain, it has to make reference to the physical and psychological condition of the speaking subject. That is not the only consideration; history and sociology also furnish explanatory principles. But that consideration is unavoidable. Part of linguistics ends up in psychology."[27] Delacroix runs, as it were, Wundt's program, but in the light of modern linguistics. After an introductory "First Book" which reviews the historical views on the relations between language, mind, and society, Book II reviews structuralist linguistic theory. It extends the *langue/parole* distinction of the *Cours* to also include, on the one hand, what we would now call universal grammar, the language function shared by all humans and, on the other hand, the act of linguistic formulation (*le parler*), which precedes the psychophysical mechanism of *parole*. It then treats the sound structure and the morpho-syntax of language. There is much talk about language as a coherent system, the structuralist notion, but this is not worked out in any detail.

Delacroix then turns to psycholinguistics proper. The first chapter of Book III tackles the acquisition of language. Here Delacroix tries to keep a midcourse between a fully maturational and a fully imitational account of language acquisition; "it is likely that there are no innate language centers" in the brain.[28] But some degree of brain maturation is indispensable for the acquisition of language and this goes stage by stage. In that respect the mind is not a *tabula rasa*. Delacroix's main argument for this staging is Piaget's recent "finding" (see Chapter 10) that up to the age of six, children's language use is entirely egocentric and prelogical. The child is "initially almost impenetrable to experience; somewhat like Lévy-Brühl's primitive mentality." This is Haeckel's theory in disguise. It was long-lived indeed.

[25] "Il n'y a pas de psychologie du langage sans recours à la linguistique." Delacroix (1924, p. 7).

[26] "La linguistique appelle nécessairement une psychologie du langage." (ibid. p. 8).

[27] "Dès qu'elle cherche à expliquer, il lui faut faire appel à la constitution physique et psychologique du sujet parlant. Cette considération n'est pas la seule, l'histoire et la sociologie fournissant aussi des principes d'explication. Mais elle est inévitable. Une partie de la linguistique s'achève en psychologie." (ibid. p. 8).

[28] "il est probable qu'il n'y a point de centres innés du langage" (ibid. p. 264).

Delacroix could not know it, of course, but Piaget's data would soon receive a quite different interpretation. As soon as maturation of brain and intelligence allow, according to Delacroix, imitation catches on, first very much in trial-and-error mode, but increasingly purposeful. Delacroix treats acquisition neatly, more or less following the Sterns (1907), but clearly without their hands-on experience. The chapter ends with a discussion of language acquisition in deaf people . He had studied Degérando (1827) but did not share his views on the full potential of sign language. Still, the best procedure in the education of deaf people is to develop as far as possible the signed communication between teacher and pupil. "All language learning is based on natural language and on a language already known by both interlocutors."[29] The oral method fails on both scores. Delacroix does not object to oral/lip reading training, even if it is not particularly effective, but thinks it should be based on a pre-existing common language, i.e. sign language.

The other chapter in Book III concerns the speaker-hearer. Delacroix again keeps a midcourse here. He is clearly charmed and impressed by aphasiologist Pick's (1913) hierarchical approach to the process of speaking. As we will see in Chapter 11, Pick adopted the notion of the Würzburgers (this chapter) that a thought to be expressed, the intention, gives rise to a sentence schema, which can be conscious without being an image. In turn, that sentence schema is filled with appropriate words. Delacroix adopts the idea that sentence schemas are habitual patterns in the mind. They are automatisms. This is exactly the position Sechehaye had taken already in 1908. Delacroix does not refer to Sechehaye in this connection, although Sechehaye's book figures in his bibliography. Also, Delacroix does not elaborate this sentence schema notion in any detail, as the Würzburgers and in particular Selz (to be discussed) had done; Selz is not even referred to. Delacroix envisages the possibility that thought, schema, and words emerge in parallel, interactively as we would now call it. This mode of sentence production is less hierarchical, more linear. For all habitual acts "the first phase of the act determines the execution of the second one. All our habitual acts are long chains of movements, where each one furnishes the stimulus for the next one, whether or not consciously."[30] This was going to be a central thesis in behaviorist theories of verbal production, a thesis which Lashley head on attacked in 1951, as we saw in Chapter 1. But in contrast to the behaviorists, Delacroix calls on mechanisms of attention. Guided by his intention, the speaker exerts some degree of control. "The speaking subject . . . is simultaneously attentive on several levels; attentive to the general sense of his discourse, to the intention that guides it, to the dominant theme, to the general correspondence of his talk to the thought, to certain delicate or important passages, to certain difficult moments, ideas or words."[31] This statement

[29] "Tout apprentisage de langue se base sur le langage naturel et sur une langue déjà connue des deux interlocuteurs." (Delacroix 1924, p. 351).

[30] "dans toutes les habitudes constituées, la perception de la première phase de l'acte détermine l'exécution de la seconde. Tous nos actes habituelles sont de longues chaînes de mouvements, donc chacun fournit au suivant son stimulant, conscient ou non." (ibid. p. 362).

[31] "Le sujet qui parle, nous l'avons déjà vu, est simultanément attentif à plusieurs plans; au sens général de son discours, à l'intention qui le dirige, au thème qui le domine; à la correspondence générale du

is, at the same time, characteristic of Delacroix's global level of argument. There is an abundance of generalities and a lack of specific proposals for the mechanisms at work. The same is true for his treatment of the comprehension process. Comprehending language is "construing a sense," not constructing an image. It is the systematic ordering of notions. It is understanding words in phrasal contexts, etc. One is happy to read that "language is an intellectual melody" and "understanding, truly understanding a word in the scholarly sense, that is doing science."[32]

Book IV addresses aphasiology. It cites Bergson's plea for abandoning the still dominant grand mechanistic schemes (i.e., the Wernicke–Lichtheim approach) and to return to the "naiveté of observation." He then reviews and generally adopts the more holistic views of Hughlings Jackson, Pick, and Head, in particular the theories of agrammatism of the latter two authors. We will return to these issues in Chapter 11.

Although Delacroix did not appreciably contribute to the advance of psycholinguistic theory and not at all to empirical research, he did play a useful role on the psycholinguistic scene. He was a master in bridging disciplines, psychology, linguistics, and medicine, not only in his writing but also personally and practically. He was the convener of the only international, multidisciplinary conference on the psychology of language before World War II. It took place in Paris and involved Cassirer, Jordan, Sechehaye, Doroszewski, Paget, K. Bühler, Pongs, Meillet, Vendryes, Meriggi, Bröndal, Trubetskoy, Sapir, van Ginneken, Sommerfelt, De Groot, Jespersen, Bally, G. Guillaume, Grégoire, Cohen, Gelb, and Goldstein. We will come across many of these names in the following chapters. Delacroix (1933) published the proceedings as a carefully edited volume. A one-time post-war echo of this tradition was a special 1950 issue of the Paris-based *Journal de Psychologie*,[33] in which structural linguists, including Vendryes, presented their views to the psychological readership of the journal.

The psychology of imageless thought: the Würzburg school

In Chapter 5 we met Karl Marbe who, in 1901, together with Karl Thumb published a now classic study of analogy and word association. Marbe worked in Würzburg, where Oswald Külpe had established a psychological laboratory in 1896, which he led till 1909, when he left for Bonn. Oswald Külpe (1862–1915) had been a student and assistant of Wilhelm Wundt in Leipzig. From his 1894 appointment in Würzburg, a growing number of students, postdocs, and research visitors both from Germany and abroad gathered around him.[34] Although this was later called the Würzburg school of psychology, it was a rather loose collection of gifted young psychologists who were mutually inspiring,

débit à la pensée; à certains passages délicats ou importants, à certains moments difficiles, idées ou mots." (Delacroix 1924, p. 409).

[32] "le langage est une mélodie intellectuelle," "comprendre vraiment, comprendre à sens savant du mot, c'est faire la science" (ibid. pp. 439, 440).

[33] Guillaume and Meyerson (Eds.) (1950).

[34] Among them Ach, Bühler, Dürr, Marbe, Mayer, Messer, Ogden, Orth, Taylor, and Watt.

without much central direction, let alone authoritative leadership. Oswald Külpe was rather an inspiring *primus inter pares*, as much a philosopher as an experimental psychologist. As a philosopher he worked on aesthetics and on epistemology, slowly moving from Wundt's idealism to a form of realism, probably his major, although unfinished, project.[35] Külpe's role as an experimentalist was almost exclusively his participation in and guidance of the work by others in his laboratory. He did not have a special project of his own, but there are numerous publications by his co-workers where "K" was a distinguished subject in the experiments. It must have been largely under Külpe's inspiration that the psychologists around him increasingly turned to so-called "higher" mental processes, in particular processes of thought and volition.[36] The new perspectives emerging among the Würzburgers were also to affect the psychology of language. This was in particular mediated by two of Külpe's students, Karl Bühler and Otto Selz. The former did his habilitation under Külpe's supervision in Würzburg and then followed him to Bonn and to Munich. The latter did his habilitation with Külpe in Bonn. The remainder of this chapter will largely be devoted to their early work. Before turning to that, however, two core features of the new Würzburg perspective should be mentioned.

The first feature is a methodological one. It was Karl Marbe's (1901) introduction of a new experimental method, so-called "systematic experimental introspection" (a term coined by Narziß Ach). The basic idea is simple. There is an experimenter and a subject, both trained psychologists. The experimenter assigns the subject a task, which can be as simple as a psychophysical task or as complex as a philosophical problem. After completing the task (timed by the experimenter), the subject is asked to give a detailed introspective account of what he experienced while performing the task. This was carefully recorded by the experimenter in a record called the "protocol." Initially, this was no more than a minor extension of the traditional reaction time experiment. Marbe, for instance, timed reactions such as determining which of two weights is the heavier. Collecting the subjects' introspections, or rather retrospections, on how they had arrived at this simple judgment might reveal the conscious process underlying the response. Psychology, according to Wundt, was the study of immediate experience, experience manifesting itself in consciousness. Psychology was broadly considered as dealing with "the stream of consciousness" (William James). It did not deny the existence of unconscious processes, not at all. We saw, for instance, Wundt's theory of automatization through exercise of volitional action. But consciousness is where the action is; it is in the psychological driver's seat. This should hold in particular for the "higher" psychological processes, such as judgment, thought, and will. The bare reaction time experiment would hardly reveal any details of the subtle conscious events involved. An immediate retrospective report by the experimental subject might fill in this empirical gap. This made it critical, of course, that

[35] Külpe (1912) was the first of four planned volumes on this topic.

[36] Albert Michotte, who had worked on volition in Louvain, went to Külpe after his less than successful stay with Wundt. I remember Michotte telling me about the excellent time he had had with Külpe. In his autobiography he writes about his stay: "J'y fus parfaitement heureux et ce fut une véritable révélation, car c'est à Külpe que je dois ma véritable formation psychologique." Michotte (1954, p. 3).

the subject was a well-trained (self-)observer, able to focus his (yes, always "his" for some time to come) attention on the essential features of the recollected experience.

The second feature was an accidental "discovery" made by way of this new introspective method. When Marbe questioned his trained subjects, among them "K," on the conscious contents leading up to the response, all sorts of images were mentioned but none specifically relating to the judgment itself. The judgment itself is an imageless experience. That did not fit the traditional categories of conscious content, sensory images, images from memory, feelings, and their relations. A new idea was born, the existence of conscious but imageless thought. The idea was also born (or immediately picked up) in France, where Alfred Binet devoted a whole chapter in his 1903 book *The experimental study of intelligence* to "thought without images." He informally applied the method of systematic introspection in working with his two child subjects, Armande and Marguerite. When, for instance, he presented Armande, 13 years old, with the word *tempête* ("storm") she responded "Oh! I cannot imagine anything! Because it isn't an object, I don't imagine anything."[37] But she understood the word, a thought without an image. In this respect, Armande was apparently as sophisticated a subject as "K." Reading the transcript of what Armande said and the ever increasing number of protocols of the Würzburg experiments, it is astonishingly apparent that the new idea was so much on the minds of the trained observers that it almost obligatorily appeared in their introspective reports. Here are just two of numerous examples in Bühler's study, to which we will presently turn. "K" reports on how he judged a complex statement: ". . . I didn't speak, I didn't imagine anything . . ." Or in another case: "The thought didn't get formulated, indeed I just thought it."[38]

The third new Würzburg perspective concerned the "determining tendency" of the task. Subjects often reported being quite aware of the *Aufgabe*, the purpose of the task. That was the topic of Watt's 1904 study of thought processes, where he used tasks such as mentioning a superordinate or a subordinate for a stimulus word (*dog*-"animal," *dog*-"collie"). But also here, although subjects experienced the task as "determining," they had very little conscious detail to report. Probably the most systematic study in this framework was the one by Ach (1871–1946), published in 1905. In one among several experiments a subject had to respond with a rhyme to a presented nonsense syllable (such as *lan*-"man"). The subject would then retrospect on the process. One subject reported consciously applying the procedure: "If the presented syllable doesn't start with 'm,' I replace it by 'm.'" That would indeed have worked for the example *lan*–"man." Ach mentioned various such problem-solving procedures. More importantly, Ach introduced a theoretical notion that was to spread among the Würzburgers and that Selz was going to bring to full fruition. It was the concept of "determining tendency." It was to serve as a theoretical account for the effectiveness of task awareness. In the traditional accounts, the stimulus presented to the subject would, by association, activate a set of related images (*Vorstellungen*) in memory,

[37] "Oh! je ne peux rien me réprésenter! Comme ce n'est pas un objet, je ne me répresente rien." Binet (1903), p. 84).

[38] K. Bühler (1907), pp. 305, 347.

as in Herbart's "apperception." Among them there would often be one relevant to solving the task. But association alone would not do as a causal account of solving the task. Let us cite Ach:

> By means of the stimulus several images are reproduced, and in some cases the one image that corresponds to the point of the task gets excess value. The determining tendencies now bring about that, among the many activated tendencies brought about by the perception, the one that is associated with some image connected to the intention gets strengthened. This way the image of the result often springs up right after the perception.[39]

The determining tendency is causal in a different way from time-honored association and we will see how Selz turns it into an algorithmic notion.

The Bühler–Wundt clash

It was in 1905 that Karl Bühler (1879–1963) came to Würzburg, where he soon became Külpe's assistant. Bühler was born in Meckesheim near Heidelberg. In 1903 he obtained an MD with Johannes von Kries in Freiburg (his thesis was on color perception) and the following year obtained a PhD with Baeumker in Strasbourg. He then went to Würzburg, where in 1907 he completed his habilitation on the psychology of thought processes, under Külpe's supervision. In the following chapters there will be many occasions for us to consider Karl Bühler's further biography. Here we only consider his remarkable start as a thought psychologist.

Bühler's habilitation work was published as a sequence of three journal articles in 1907/8.[40] The first 1907 paper was enough to make 75-year-old Wundt explode. He wrote a 60-page devastating review of the new experimental method, coining it the "interrogation method" (*Ausfragemethode*), and of Bühler's use of it in particular. It appeared immediately after Bühler's first paper, still in 1907. But what had caused Wundt's anger?

Bühler, like all Würzburgers, was at the time a consciousness psychologist. He wanted to know what happens in consciousness during the execution of "higher" mental processes. Many of the tasks his Würzburg colleagues had used had not been very revealing, because they were apparently too simple. Marbe's psychophysical judgment tasks, Watt's super-subordinate tasks, and Ach's rhyming and alliteration tasks usually led to the sudden, immediate awareness of the solution, the one imageless thought. Bühler decided to go for much more complex tasks in the hope that they would require more attention and run at a slower pace. He certainly achieved that. As was the habit in Würzburg he measured his subjects' response latencies with a simple stopwatch. Solving his task often took a participant several seconds (although Bühler does not give details).

[39] "Durch den gleichen Reiz werden verschiedene Vorstellungen reproduziert und zwar wird im einzelnen Falle jene Vorstellung überwertig, welche dem Sinne der Absicht entspricht. Die determinierenden Tendenzen bewirken hier, daß unter den vielen durch die Wahrnehmung in Bereitschaft gesetzten Tendenzen diejenige zu einer überwertigen verstärkt wird, welche einer der gegebenen Absicht entsprechenden Vorstellungen assoziativ zugeordnet ist. Hierdurch taucht die Vorstellung des Resultates häufig unmittelbar nach der Wahrnehmung auf." Ach (1905, p. 187).

[40] K. Bühler (1907, 1908b,c).

Different types of complex tasks were used in the papers. The tasks in the first two papers were to answer "yes" or "no" to some philosophical or scientific question. After all, Bühler's subjects were "professors or doctors of philosophy." Here is such a question: *Can we by way of our thinking understand the essence of thought?* After the subject's positive or negative response he was asked to report on his conscious experience in reaching that response. The third paper introduced four more restricted tasks. The task of "thought pairing" was to study a list of 20 unrelated "A–B pairs of thought," such as *Spinoza's Ethics* (A) and *blessings and dangers of solitude* (B). After memorization, the subject would be presented with an A-thought and asked to reproduce its B-thought. Subjects were quite successful in this and happily reported on what had been going on in their minds. The "completion task" presented participants with a list of broken off (German) proverbs, such as *birds that sing too early . . .* which they had to attend to for a few minutes. Then Bühler would present his subjects with the second half of the idiom, for example *. . . are caught by the cat*, and the subject had to provide the corresponding first half. This, again, was followed by introspection. The "analogy task" started with the participant reading a list of "thoughts," in fact proverbs such as *who has bread is given bread*. The experimenter would then present another proverb (such as *where doves are, doves fly to*), asking for the analogical pair (like the former one) in the remembered study list. Finally, in the "probe word task" the subject first studied a list of proverbial statements (such as *the wood has ears, the meadow eyes*). Then Bühler would present a probe word from one of them (such as *wood*) and the full statement had to be reproduced. Again, the participant would report on his conscious experiences.

It is interesting to observe Bühler's bold invention of complex psycholinguistic tasks, more sophisticated versions of which would only appear some eight or nine decades later.[41] It is, however, equally interesting to observe the total lack of experimental control, which was, of course, a main source of irritation for Wundt. But before turning to that, let us consider some of Bühler's results and conclusions. The dominant issue is the phenomenology of imageless thought. Bühler skimmed many hundreds of protocols to develop a typology of imageless thought. Without claiming comprehensiveness, he discussed three major types. There is, first, "consciousness of rule." The thinking subject mentions awareness of some regularity that is relevant for the problem at hand. It is not an image of anything, but rather something like the major premise of a syllogism. For example, subject "D" describes his experience in dealing with the statement *Can we by way of our thinking understand the essence of thought?* The protocol reads: "I was conscious of the general thought that sentences in which the same concept is in some way doubly present, only seemingly pose special difficulty." This, indeed, can be seen as the major for a syllogism in which the stimulus sentence can be recast as the minor premise. No image around, just a pure consciousness of some regularity.

A second variety of imageless thought is "consciousness of relation." The thinking subject becomes aware of some relevant relationship. This is what subject "K" experienced

[41] The first probe-word primed experiments on idiom production since Bühler, for instance, were published by Sprenger *et al.* (2006).

when asked to consider the statement "The smaller a woman's shoe, the bigger the shoe bill": "I was initially surprised by the sentence, but then I suddenly realized the trick it involved, becoming aware of the relation between part and whole figuring here."

The third variety is "consciousness of intention," which is in a way "meaning itself." It is the act of meaning rather than its object, i.e., what is meant. Here the influence of contemporary philosopher Edmund Husserl is apparent. He talked about "significative acts," i.e., entertaining an intentional relation to something. Here is an example from Bühler's protocols. In responding to a statement about skepticism in antiquity, subject "K" states: "I thought of antique Skepsis (the word Skepsis internally pronounced), with much included therein; I had temporarily present the whole development in three periods." Here philosopher "K" is aware of a whole chapter of historical philosophy but there is hardly any further conscious content. It is being aware that one knows, rather than having any of the known details in consciousness. This is, according to Bühler, the normal case when we understand a word: "when, with the sensory impression of a word, I am aware of its sense, it does not mean that I imagine something, but only that I know something."[42] Here, Bühler is reinventing Steinthal's theory, the word in consciousness standing for its whole meaning complex.

Bühler's protocols are scattered with theoretical concepts such as imageless thought, task, schema, and so on. The employment of "trained subjects," professors and doctors of philosophy, led to the reification of their pet theoretical notions. After all, they became conscious of them in dealing with Bühler's tasks and Bühler correspondingly accepted them as realities of consciousness—full circle. This was not, however, what Wundt criticized. Before turning to that, we will focus on one rather more psycholinguistic issue in Bühler's papers, sentence understanding and formulation. Bühler's remarks became the seeds for Selz's later theory of sentence formulation. Bühler described how, in many cases, his subjects some time after having read one of his complex philosophical statements suddenly realized the gist. Here Bühler introduced the term "Aha experience" (*Aha-Erlebnis*), which would become a standard expression in the psychology of thinking. Bühler then defined the psycholinguistic core question for sentence understanding: "How is the thought to be understood built up from the word meanings?" But he adds immediately, "Regrettably the protocols let us down almost completely."[43] There are a few remarks on how we handle word ambiguities, but that's all. Bühler is, however, soon to return to these issues,[44] as we will discuss later. More relevant are Bühler's occasional observations on the process of sentence formulation. They stem mostly from the completion and analogy tasks where some sentence is to be reformulated from memory. A frequent observation in the protocols is that the sentence just appears. In one of

[42] "wenn mit dem sinnlichen Eindruck der Sinn des Wortes mir präsent ist, so heißt das nicht, ich stelle mir etwas vor, sondern ich weiß um etwas." K. Bühler (1907, p. 363).

[43] "wie baut sich der zu verstehende Gedanke aus den Wortbedeutungen auf? Leider lassen uns hier die Protokolle fast ganz im Stich." K. Bühler (1908b, p. 18).

[44] K. Bühler (1909a).

the "K" protocols it says: "I started to speak and the sentence comes more or less complete into the world." Not exactly the unpacking of a total image as Wundt had formulated from introspection "at his writing desk" (*Schreibtisch*, a term Bühler happens to use repeatedly while referring to Wundt). Later, Selz would explain this from the fact that these were reconstructions of remembered whole sentences. But there is more in the protocols. In another "K" protocol we read: "The words came under the guiding consciousness of a sentence form." Also: "It concerns an either-or." This leads Bühler to make the theoretical claim: "When we want to express a complex thought, then we first select the sentence form for it, we first become internally aware of the operation plan, and then it is this plan which first masters the words."[45] Selz also picked up this idea.

Wundt, in his critique, set out to characterize Bühler's experiments as "pseudo-experiments" (*Scheinexperimente*). His approach was to postulate four rules that experiments (in any science) should adhere to, and then to show that Bühler's experiments violated all four of them. Rule 1 is that the observer has full control over initiating the event. In Bühler's experiments, however, the observer is the *subject*. The observations are self-observations. These observed events are not self-initiated. Worse, they come as surprises triggered by extremely complex statements. Rule 2 is that the observer can fully focus on the event. This is violated in Bühler's procedure because the observer, i.e., the subject, is at the same time concentrating attention on solving the problem and on introspectively registering what is going on in consciousness. Such doubling of attention is unfeasible, a "doubling of personality." Rule 3 is that the experiment should be repeatable. This is to gain control over natural fluctuations in the studied events. This is not the case in the interrogation experiments. Each problem presented is an entirely new one. Rule 4 is that conditions are systematically varied. This can either be achieved by switching some condition entirely on versus off, or by systematically varying a condition in strength or quality. This is, according to Wundt, the most important rule ("Bacon's rule"). However, there is no such systematic variation in Bühler's experimental conditions. In short, they are pseudo-experiments.

Wundt then went on to discuss several theoretical issues. One is that nowhere in Bühler's paper is the essential and elementary distinction made between "field of view" (*Blickfeld*) and "point of view" (*Blickpunkt*). Consciousness is much wider than what is in the observer's focus of attention. Wundt presents experimental data suggesting that a field of view can contain as many as 40 items, whereas only one of them is in focus. This undermines the logic of Bühler's and other Würzburgers' conclusion that imageless thought exists. The only thing demonstrated is that one can entertain a thought without having an image in the focus of attention. The "field of view," i.e., wider consciousness, may contain one or more relevant images. They announce their presence through the strong feelings that go with them. The self-observations may have been reporting on such image-based feelings that occasionally appeared in the focus of attention. Instead, Wundt

[45] "Wenn wir einen schwierigen Gedanken ausdrücken wollen, dann wählen wir erst die Satzform für ihn, wir werden uns innerlich erst des Operationsplanes bewußt, und dieser Plan ist es dann, der erst die Worte meistert." Bühler (1908b, p. 86).

quips that the Würzburgers throw themselves into the arms of the scholastics with their "pure acts."

Bühler replied right away. Accepting Wundt's four experimental criteria, Bühler argued that his studies meet all four of them. Though eloquent in places, his response does not quite convince the reader. Bühler, clearly, is not a born experimenter. Rather he is an analytical and wide-ranging theoretician. That is how we will see him time and again in the following chapters. Instead of playing Wundt's game, Bühler should have admitted that his studies were not experiments in the classical sense, but systematic observations—a quite different kind of "experiment." He got close to that in his third paper. Here Bühler sketched the core problem of how thinking relates to speaking and how psychologists have been philosophizing about it. He then adds "But the so very obvious thought, to study in living reality the laws by which speaking links itself to thinking, has dawned upon no one."[46] That is what he should have responded to Wundt: let us, psychologists, gather live observations on how speakers go about formulating ideas. That is exactly what Selz set out to do.

Wundt replied quickly once more. It was an easy response because Bühler had accepted his gambit. It is also a funny paper, a rare performance for Wundt. He picked up Bühler's repeated references to Wundt's "writing desk" experiments. "I obtained the impression," Wundt writes, "that Bühler imagines the Leipzig laboratory as essentially a set of writing desks, where the co-workers are seated, thinking up experiments they don't run, or reflecting on those performed by others."[47] It was more than a quarter century later that Bühler admitted having doubts about his own method: "But I was neither satisfied then, nor now, about the state of affairs and in particular about our method."[48]

Oswald Külpe was not happy with his respected teacher's lashing out at the Würzburgers and expressed this in a letter to Wundt. Wundt's answer to Külpe is dated 26 October 1907. Although that letter is conciliatory in tone, Wundt does not budge an inch. In fact, Wundt expresses his "painful regret" to read how much Külpe identifies with the interrogation method. Until then, Wundt had thought "Marbe the intellectual initiator" of this "reprehensible method." "As Marbe's method, I could understand it completely, however. Because I consider Marbe as a man who can occasionally construct a sensible instrument, but who lacks any ability as a psychologist, so that I had long accustomed myself to consider his work in this direction as non-existent."[49] History has judged Marbe differently.

[46] "Aber der doch so nahe liegende Gedanke, einmal in der lebendigen Wirklichkeit danach zu forschen, wie, nach welchen Gesetzen sich an das Denken das Sprechen anschließt, ist noch niemand eingefallen." (K. Bühler 1908b, p. 85).

[47] "Ich habe daraus den Eindruck gewonnen, daß sich Bühler das Leipziger Laboratorium, als eine Ansammlung von Schreibtischen vorstellt, an denen die verschiedenen Mitarbeiter sitzen, um sich Experimente auszudenken, die sie nicht machen, oder um über solche zu reflektieren, die andere gemacht haben." (Wundt 1908, p. 446).

[48] "Befriedigt aber war ich damals nicht und bin es auch heute nicht von dem Stand der Sache und vor allem von unserer Methode." (K. Bühler 1934, p. 254).

[49] "Als Marbe'sche Methode war mir aber diese volkommen begreiflich. Denn ich halte Marbe für einen Mann, der wohl einmal ein sinnreiches Instrument konstruieren kann, dem aber zum Psychologen alle

Fig. 7.4 Otto Selz. Courtesy Professor Georg W. Alpers, Otto Selz Institute, University of Mannheim, Germany.

Otto Selz and Charlotte Bühler on sentence formulation

Otto Selz

Otto Selz (1881–1943, Fig. 7.4) was born into a well-to-do Munich family; his father was a banker, his mother a musically gifted and literary woman of Jewish descent. In 1908 Otto gained a law degree in Munich, but law was not to his taste. He had already spent a semester with philosopher/psychologist Theodor Lipps in Berlin and completed his PhD with him in 1909.

He then went to Bonn, where Külpe had moved in 1909, together with Karl Bühler. The three of them would have four years together in Bonn; Külpe and Bühler left for Munich in 1913. Selz completed his habilitation with Külpe in 1912 with a thesis entitled *Über die Gesetze des geordneten Denkverlaufs* ("On the laws of ordered thinking"). It appeared in 1913 as volume I of his planned two-volume work. In fact, by then he had completed all experiments for both volumes, as well as about one-third of the analyses for the second volume. But then four years of military service, as a sergeant-major during World War I, drastically interfered with the completion of this work. Volume II finally appeared in 1922 while Selz occupied a teaching position in Bonn. The next year he accepted a professorship in Mannheim. Selz became rector of that university, but was dismissed from his

und jede Begabung fehlt, so dass ich mich längst daran gewöhnt habe, seine in dieser Richtung liegenden Arbeiten als nicht existierend zu betrachten und mich nur jedesmal wieder darüber wundern muss, wenn mir eine solche Arbeit, wie z.B. über das Urteil, zu Gesicht kommt, wie jemand, der doch etwas Naturwissenschaften getrieben hat, so sehr aller wissenschaftlichen Methodik entbehren kann." (Wundt, letter to Külpe, 26–10-1907).

professorship in 1933 under the new Nazi regime. We will return to Selz's exile in the Netherlands and his death in 1943 on a train to Auschwitz in Chapter 14.

Selz's major contribution to cognitive psychology is his theory of thinking, which is not the topic of this book. Highly relevant for the history of psycholinguistics, however, is his theory of sentence formulation, which appeared in volume II. Because it is a natural offshoot of the general theory, we will consider some of its features before turning in more detail to sentence formulation (see Selz 1924 for a short review of his theory).

The essence of Selz's theory is that thinking consists of an ordered set of cognitive operations. He explicitly rejected all traditional accounts that, in some way or another, involve a system of diffuse associations. Earlier in this chapter, we considered a citation from Ach, in which he introduced his notion of "determining tendency." Ach's notion is still based on such a system of associations: The task stimulus induces competing "reproductive tendencies" of everything in memory that is associated with it. What Ach added was that the reproductive tendency of the association *relevant to the task* is strengthened. Selz (acknowledging Ach) entirely reversed the account, marginalizing diffuse associative processes. What needs to be explained is the conception of the one specific, adequate, correct response to the task stimulus. A diffuse system of competing associations is of no help. An entirely new type of cognitive mechanism has to be introduced. This is the *operation.* Most operations are available in memory, based on earlier experience. An operation is called up when a particular, specific condition arises (which we now call the IF-condition). That triggers its execution (the operational THEN). Our memory is full of triples of the following kind: "When the goal was G and we applied means M, we obtained the desired effect E." Or as a formula, $G \leftarrow^{\mu} M \rightarrow^{x} E$, where μ stands for the general means–end relation between means M and goal G, and x stands for the causal relation between applying the means and producing outcome E. Such memory traces are *specifically* retrieved when a new goal is set by some task. If the new task G' is to achieve the effect E, the subject must conceive of the means to achieve E. This is done by "schematic anticipation," which is setting up the schema $G \leftarrow^{\mu} ? \rightarrow^{x} E$, where "?" stands for the sought means. The part $? \rightarrow^{x} E$ (meaning "What caused E to result?") is then the adequate stimulus for retrieving $M \rightarrow^{x} E$ from memory and hence the means M to be applied. Or in terms of if–then statements: IF the goal is to achieve E, THEN use $? \rightarrow^{x} E$ to retrieve M from memory. This is, of course, the bare minimum. Selz treats a large variety of cases, in particular those where there is no relevant $M \rightarrow^{x} E$ in memory. This is productive thinking par excellence, where new means have to be conceived. Selz proposed a variety of workable procedures to achieve this. It was, in fact, a procedural theory of creativity, an absolute novelty in psychology. Solving any complex problem involves a *sequence* of means–end operations. The outcome of one operation is the specific condition, the if statement, for calling the next one in an emerging hierarchy of operations. This, as we will see, is what happens in sentence formulation too.

We will not follow Selz any further in his sophisticated theory of thinking, but only conclude that this was the very first *computational* theory in cognitive psychology. That term was obviously not used by Selz, but the pioneers of modern cognitive theories of

thinking, such as De Groot, Newell, and Simon, all recognized Selz as the father of the computational approach.[50]

Turning now to sentence formulation, let us consider Selz's (general) empirical procedure first. It was entirely in the Würzburg tradition, although much better controlled. The subject would be presented with a pair of typewritten words. Here are three examples:

Part	Superordinate	Definition
Clarinet	Car	Tool

The top item defined the task, the bottom item the stimulus. So, for the leftmost pair the subject's task was to mention a part of a clarinet. A correct answer would be *key* or *reed*. The experimenter (i.e., Selz) would uncover the pair and start his stopwatch, to be stopped on the subject's response. After that, the subject introspected on his experience, which was noted in the protocol by the experimenter. Each subsequent presentation involved both a different task and a different stimulus word. There were some 28 different tasks, but Selz's main analyses concerned just five of them: part, whole, superordinate, subordinate, and definition. The pairs were composed in such a way that the answer was probably not available in memory. There were also various controls, such as pairing the same stimulus with a different task, and repeating the same experiment with the same subject after several months. There were a total of eight "experienced" subjects, mostly Selz's colleagues in Bonn.

Selz's work on sentence formulation was based on the definition task. Here is one subject's answer, where "tool" was to be defined: "A tool is an instrument which assists the human hand in the construction of objects." When asked to introspect how the answer had come to mind, the subject said that he began talking right away "A tool is . . .," but then had to search for the word "instrument," whose meaning he did have in mind. Then he searched for the specifics (the *differentia specifica*—the subject was, after all, a philosopher) and came up with "which can serve for the construction of objects." Although this was felt to be too specific, the thought of manual work came up and the corresponding phrase "and assists the work of the hand." Then while speaking he combined the two phrases into one.

A characteristic property of these definitions is what Selz calls the "phase-wise formulation" (*Phasenweise Formulierung*). It is also the dominant, although not exclusive, manner of formulation (as will be discussed). The process is quite different from what Wundt had suggested. Formulating begins without a "total image" of what is going to be expressed. Formulation is, rather, what we now call "incremental." Selz was the first psychologist to express that notion in any detail.[51] Both the content to be expressed and the grammatical schemata are produced piece by piece and hand in hand, sometimes the content leading,

[50] See Mandler (2007) for historical details.

[51] It had been expressed much earlier by Heinrich von Kleist in his famous 1805 essay *Über die allmähliche Verfertigung der Gedanken beim Reden.*

sometimes the grammatical schema. Each of these small moves is an operation; an operation that is conditioned, i.e., triggered, by the outcome of the previous one.

In discussing the formulation process in the definition task, Selz distinguishes two major phases. Phase I of formulating begins before the full content has been fixed. At the same time there may already be some sentence schema available. So, for instance, the same subject immediately produced "A tool is . . .," without having conceived of the further content of the sentence. Here the sentence schema takes the lead, both grammatically and logically, because what is to come should be both a substantive (grammatically) and an appelative, a kind term (logically). After retrieval of the appropriate kind term ("instrument"), the next phase of the sentence schema is initiated, Phase II. This is because the definition schema requires a further specification of the kind term ("instrument") or because the logic requires a further specification of the retrieved generic term. Either way, both the conceptualization of that specification and the grammatical schema for it are triggered. The grammatical choice is quite limited. The speaker can go for a relative clause, as the example speaker did ("which is . . ."), or for an infinitival or participial clause ("assisting the hands. . ."). In some cases a prepositional phrase can handle the further specification, as in defining "war" as a "conflict of arms."

At each step in the formulating process, whether it is a logical or a grammatical one, the choice of available operations is extremely limited. Selz demonstrated this with a wealth of examples from his protocols. He also kindly expressed his opinion that it is not of much importance whether these operations involve imagery or even consciousness, softly distancing himself from the Würzburg hotspots.

Selz summarized his analyses in a schematic overview of sentence formulation procedures.[52] The phase-wise procedure is the centerpiece, but there are three further variants. In colloquial practice, speakers do amalgamate them. Here is Selz's classification:

I. *Sentence reproduction.* Here the sentence is produced from memory, ready made. That happened in some of Karl Bühler's experiments, where subjects reproduced an idiom.

II. *Sentence construction.* Here the sentence has to be built. There are two variants:

   A. The content is fully there at the outset. Formulating is "subsequent." It is also "analytic," because it is the partitioning of the content which takes the lead. This sounds familiar, because it is essentially Wundt's formulating procedure. It is, however, exceptional.

   B. The content is still to be found. Two cases should be distinguished:

      (a) Phase-wise formulating. The development of thought and of linguistic formulation runs in parallel. This is the common, dominant case.

      (b) Synthetic formulation, which is a particular type of "subsequent" formulation. Here the development of content is "synthetic," i.e., not by analysis of a pregiven thought or "total image," but rather by adding related thought fragments,

[52] Selz (1922, p. 361).

> one after another. The final "collection" of pieces activates a sentence or phrase schema that can handle them. This is the one case that roughly corresponds to Paul's sentence definition ("The sentence is the linguistic expression, which symbolizes that the association among sundry images of groups of images in the mind of the speaker has come about.").

So, according to Selz, both Wundt and Paul have dealt with rather marginal cases of sentence formulation and ignored what is normally going on in a speaker's spontaneous formulation.

Selz completed his chapter on sentence formulation with a review of related work. He acknowledged the inspiration he received from Karl Bühler's papers, the first recorded analyses of sentence formulation, in particular Bühler's notion of effective sentence schemata. He referred to Pick's work on aphasic speech, also inspired by Karl Bühler, and quite similar in spirit to his own work (we will return to this in Chapter 11).

Charlotte Bühler

Selz also mentioned the (1918) dissertation of Charlotte Bühler (1893–1974). Charlotte Malachowski, daughter of a Berlin architect, after studying medicine, education, and psychology, moved in 1915 to Munich in order to get a PhD with Oswald Külpe. By then, Charlotte had already spent two years probing her new method of studying the generation of thought and sentences. Her idea was to present her subjects with lists of words that had to be synthesized into well-formed sentences. This required the subjects to both infer from the words presented the likely sentence content and to formulate this content, using the presented words. But her paradigm was much older, or so she claims. In her autobiography Bühler tells the story that she had invented this experimental procedure when she was 17 years old, because she had conceived the idea finding out "how the nature of our thought processes is really constituted."[53] Parents, brothers, and friends had been her subjects.

In Munich she collected her definitive set of subjects. Here is one of her test items: *simplicity—heart—mind—stupidity—innocence.* The subject produced: "Simplicity of heart is called innocence, simplicity of mind stupidity."[54] In good Würzburgian tradition, the (trained) subject added a detailed introspection. In this way Charlotte Bühler collected 738 protocols, most of them reported in her dissertation. Among her conclusions is the observation that the test words evoke concepts, which in turn combine into thoughts. These thoughts then trigger sentence formation, which is a purely grammatical process. But there is also the frequent observation that the stimulus words already combine into phrases, before or even without evoking thoughts. Bühler was struck by the effortless automaticity by which sentence or phrase schemata presented themselves to her subjects.

[53] "mich darüber zu unterrichten, wie eigentlich die Natur unserer Denkprocesse beschaffen sei" (C. Bühler 1972, p. 10).

[54] "Einfachkeit des Herzens nennt man Unschuld, Einfachkeit des Geistes Dummheit." (C. Bühler 1918, p. 197).

Oswald Külpe suddenly died the same year Charlotte Malachowski arrived in Munich and Karl Bühler took over her supervision. Charlotte's was probably the very first experimental dissertation in psycholinguistics by a woman. We will return to the Bühlers' story in Chapter 9 and then in Chapter 14.

Retrospect

Selz's highly original work on sentence formulation has only slowly dawned on the minds of modern students of speaking, including my own. This is not only due to a rather general feature of the history of science, the short-lived memory of any science and the cyclic re-invention of leading ideas. It is, in this case, in particular due to the ravages of World War II, which caused a general amnesia for major pre-war German achievements in psycholinguistics.

Still, whether underground or intentional, the new perspectives inherent to Genevan structuralism and Würzburgian thought psychology did affect the course of twentieth-century psycholinguistics. Synchronic structuralism was to beget generative linguistics. Thought psychology was to beget the computational approach to symbolic mental processing. Together, these became major ingredients of modern psycholinguistics. But there was still a long and bumpy ride ahead. The behaviorist perspective, to which we will now turn, was as much a challenge as an obstacle.

Chapter 8

Verbal behavior

In 1904 Ivan Pavlov (1849–1936) received the Nobel Prize for Physiology and Medicine for his work on the physiology of digestion, that is, not for his work on the conditioned reflex. That work had only just begun and Pavlov surprised his audience in Stockholm when he finished his Nobel lecture with some remarks on the "psychical phenomenon" of the conditioned reflex, the dog starting salivation on the *sight* of the food alone. The basic set-up of his later classic experiments is familiar. When a dog is stimulated in the mouth with meat powder, there is an immediate salivation reflex. If you repeatedly pair the administration of the meat powder (the "unconditional stimulus") with the sound of a bell (the "conditional stimulus"), ringing the bell alone will also trigger the salivation reflex. Pavlov called this "psychic stimulation" of the salivary gland or "signalization." In later work, Pavlov distinguished between this "first" signaling system and a "second" signaling system unique to humans, based on language and responsible for thought. "The word created a second system of signals of reality which is peculiarly ours, being the signal of signals. On the one hand, numerous speech stimuli have removed us from reality. ... On the other, it is precisely speech which has made us human." (Pavlov 1927, p. 357). This *second signaling system* is no less mechanical and deterministic than the first system, but of a different order of complexity: "Of course, a word is for man as much a real conditioned stimulus as are other stimuli common to man and animals, yet at the same time it is so all-comprehending that it allows of no quantitative or qualitative comparisons with conditioned stimuli in animals." (ibid. p. 407). It was Pavlov's first signaling system which induced American behaviorism, as we will presently discuss. It was not felt necessary to set apart a different signaling system for language and thought. The notion of a second system, however, lived on in Soviet psycholinguistics and it led, by the end of the 1930s, to an experimental paradigm of "semantic conditioning," which became quite influential on the American scene, as we will see.

Pavlov had discovered a new mechanism of animal learning, which stirred the imagination. John Watson, a young comparative psychologist at Johns Hopkins University, applied the method to human subjects (Watson 1914). When he stimulated the subject's finger with an electrical shock, the finger was immediately withdrawn. That was the unconditional reflex. If Watson rang a bell just before applying the shock, after a few trials the withdrawal reflex would be triggered by sounding the bell alone. Not only animal, but also human behavior could be conditioned. In another famous study, Watson and Rayner (1920) conditioned a baby ("little Albert") to have a fear response to white furry objects by pairing them with a very loud noise. Apparently, learning or habit formation could be

brought under experimental control without recourse to such notions as "consciousness." Psychology no longer should focus on conscious experience, as had been the Wundtian tradition. Introspection was no longer a valid methodology. Rather psychology should focus on objectively measurable behavior.

American behaviorism was not triggered by Watson alone, as we will presently discuss. It was heterogeneous from the outset; no commonly shared view ever developed over the half century we consider. The present chapter will look at these developments from the psycholinguistic perspective. Most behaviorists had more important concerns than human language, but some recognized the theoretical challenge of describing and explaining verbal behavior. This chapter will focus on behaviorist theory in psycholinguistics. It will provide the basis for understanding much of the American empirical work in the psychology of language to be discussed in Chapters 10 through 13.

Heterogeneous behaviorism

John Watson introduced the terms "behaviorist," "behavioristic," and "behaviorism," adding "While it is admitted that these words sound somewhat barbaric on a first reading, they at least have the merit of being expressive and natural" (Watson 1914, p. 1). And indeed, Watson, who would spend the second half of his professional life in the advertising business, had a great talent for public rhetoric. His intention was no less than to lead a behavioristic revolution in psychology in order to establish it as a full member of the sciences. This "behaviorist's program" involved two core ingredients, one metaphysical and one methodological. The metaphysical one was the rejection of dualism in psychology. It makes no sense for psychology to distinguish between a physical/physiological sensory-motor reality and a mental conscious reality. Psychology has only one reality to deal with, observable behavior. It is a purely physical reality, the reality of motions. They may be muscular, glandular, or neural-mechanic, but in all cases they are physical motions. They have their natural place within a physical hierarchy extending from electrons and protons to molecules, cellular life, behavioural life, social life, and civilization at large,[1] There is no other than physical reality; the existence of a "second," mental reality is denied. Traditional mentalistic concepts, such as consciousness, mind, sensation, mental image, idea, will, attention, and imageless thought, can have no place in a scientific psychology. Rather the stuff of psychology is "the observable fact that organisms, man and animal alike, adjust themselves to their environment by means of hereditary and habit equipments" (Watson 1914, p. 10).

The methodological consequences follow naturally: psychology only deals with objectively observable and verifiable facts, just like any other science. These facts are the organism's observable responses to observable stimuli. Or in Watson's words: "In a system of psychology completely worked out, given the responses the stimuli can be predicted; given the stimuli the responses can be predicted." (p. 10). The culprit methodology of mentalism is introspection. It should be radically eradicated, Watson proclaimed.

[1] Weiss (1925a) would formulate these claims as "postulates."

Together, these two ingredients, objectivism and anti-mentalism, caused a massive translation exercise. The total abolishment of mentalistic concepts could not involve the total denial of a wealth of psychological facts that had been formulated in the language of mind, "mentalese." The classical Ebbinghaus data on verbal learning and memory, for instance, were prototypical for objective behaviorist methodology, but they had been cast in a mentalist formulation. Traditional psychophysics was entirely conceived of as a dualistic project, relating the physics of stimulation to the psychic of sensation. Still, its data were incontrovertible. What was urgently needed was translating the mentalist notions into a behaviorist, physicist language. Margaret Washburn, in a lucid paper on the inescapability of introspection, called this new language "behaviorese."[2] Indeed, a major part of all behaviorist work on the psychology of language is nothing more than translation into behaviorese. Or as Esper put it: "It was the changes in the lexicon of psychology to match the new meanings which gave the appearance of novelty to behaviorism."[3] It became a "Great War of Words," Esper remarks, which received much more attention than "references both to experimental research already accomplished and to projected research" (p. 184). That observation is entirely correct, at least for psycholinguistic research. Theory-guided empirical research remained quite meager in the behaviorist tradition. Rather, the behaviorist penchant for objectivism led to blunt, theory-less data gathering at a large scale, as will transpire from Chapters 10–12. The translation exercise, however, never subsided; it was still major business in Skinner's (1957) *Verbal behavior.*

Physicalism and "objectivism" were about all that the behaviorists we will consider agreed on. They differed in almost every other respect, such as the role and importance of hereditary, instinctive behavior, the inclusion or exclusion of physiology in theorizing, the allowance of mediating mechanisms between stimulus and response, the relative roles of classical Pavlovian learning and instrumental trial-and-error learning in habit formation, and last but not least, the types of fact accepted as objective.

In this chapter we will consider six behaviorists (and their "circles") who devoted more than accidental attention to language. We will discuss them, by and large, in the chronological order of their main publication(s) in this area. They are John Watson, Grace de Laguna, Leonard Bloomfield and his main source of behavioristic inspiration Albert Paul Weiss, Jacob Kantor, Fred Skinner, and Charles Osgood. All of them went their different ways and it is remarkable that mutual reference between them is minimal. Kantor makes no reference to Watson in his lengthy text. Skinner likewise does not cite Kantor, who had brought him to Indiana University, where they were close colleagues from 1945 till 1948. But then Kantor (1977) ignores Skinner's *Verbal behavior.* Skinner makes no reference to Osgood. The latter, although quite generous in citing his colleagues, never mentions Kantor. And so on. So, let us consider them each on their own terms.

[2] "I shall try throughout to assume the attitude of a behaviorist and to speak behaviorese." (Washburn 1922, p. 94).

[3] Esper (1968), p. 155, but "the basic natural science principles and the objective methodology had been adopted by such psychologists as Stumpf, Helmholtz, and Ebbinghaus."

Watson and vocalic thought

John Broades Watson (1878–1958) was born in Greenville, South Carolina. There he received a Masters degree at Furman University at the age of 21. He found his vocation as a student of comparative psychology at the University of Chicago, where he obtained his PhD in 1903, the year Pavlov discovered the conditional reflex. Among his teachers were the pragmatist philosopher John Dewey, the functionalist and consciousness psychologist James Angell, the animal physiologist Henry Donaldson, and the reflex physiologist Jacques Loeb. In 1907 Watson received an appointment at Johns Hopkins University, where he was dismissed at the height of his fame in 1920 because of an adulterous affair with a graduate student (his co-author on the "little Albert" study, who later was to become his wife) and a subsequent divorce.[4] Unable to find a new position at another university, Watson went into the advertising business, but he continued publishing psychological monographs.

In 1913 Watson published, in *Psychological Review*, his behaviorist manifesto "Psychology as the behaviorist views it." His plea is to "never use the terms consciousness, mental states, mind, content, introspectively verifiable, imagery, and the like" (p. 166). We can write a psychology "in terms of stimulus and response, in terms of habit formation, habit integrations and the like" (p. 167). "Human psychology has failed to make good its claim as a natural science. Due to a mistaken notion that its fields of facts are conscious phenomena and that introspection is the only method of ascertaining these facts, it has enmeshed itself in a series of speculative questions which, while fundamental to its present tenets, are not open to experimental treatment." (p. 176). As with any revolutionary proclamation, Watson's is a mixture of facts and fantasies. He was by and large right in saying that, so far, experimental psychology had been dealing with conscious phenomena, for Wundt the phenomena of "immediate experience," such as processes of sensation, image formation, voluntary decision, and the like. But it was fantasy to claim that introspection is its "only method." That only held for the Würzburgers. Wundt's vicious attack on that method is not mentioned by Watson anywhere, neither in the manifesto nor in *Behavior* (1914), which is a detailed elaboration of the 1913 proclamation. And as we will presently see, Watson was himself not averse to Würzburgian methods.

What has been Watson's contribution to the psychology of language and speech? The short summary is: his speculation that thought is "implicit language activity," which in turn "goes on in terms of sensori-motor processes in the larynx."[5] These views were first worked out in *Behavior*, in the chapter on "Man and Beast." The breeding ground for a child's language is a rich, instinctive, hereditary repertoire of vocal reflexes. A wide range of stimuli will trigger reflexive cries and sounds of all kinds. This repertoire, combined with complex musculature of the vocal apparatus, creates ideal conditions for the development of simple and complex language habits. It is only a matter of time before we can record the child's muscular activity and plot the learning curves as language

[4] Many rumors have circulated about this dismissal. The air was cleared by Benjamin *et al.* (2007).

[5] Watson (1913, p. 177).

habits develop. That will be objective science. It makes no sense to, out of nowhere, drop in some mechanism of "imaginal thought" for the explanation of such habitual behavior. The most economical assumption is that thought is just vocal language habit. This should, in principle, be testable. Ask somebody to think, and measure activity in the musculature of the vocal tract. To make his point, Watson showed some curves measured by Anna Wyczoikowska, displaying tongue movements made by subjects instructed to *think* of a particular word or sentence, to *remember* some verse or song or to *listen* to spoken words. There is lingual activity in all cases, but as Watson concedes, this evidence falls short of proving his point. Watson's speculation, however, was enough to prompt others to respond.

The most eloquent response of all came from intelligence psychologist Arthur Otis (1886–1963) of Stanford University. His 1920 paper in *Psychological Review*, "Do we think in words?," begins by exemplifying modes of thinking that are convincingly non-linguistic. Among his examples are the refolding of an unfolded new map, which involves spatial thinking, which can be "introspected" in detail. Another case is playing chess. Notice how hard that is blindfolded. Thinking is done by perceiving patterns of pieces on the board. A third case is composing music at the piano, which involves both visual and tone images. Without reference to the Würzburgers, Otis argues that such thinking is directed by some conscious aim. It depends on the set aim, for instance, whether two digits written one above the other are to be added or to be subtracted, although the same stimuli are involved in the two cases. Thinking often involves symbols. Words are, admittedly, much used symbols in thinking. Like other symbols, words symbolize meanings and ideas. Words are not themselves the ideas, the images that are manipulated in thought, they only symbolize them. Both for the child and for the adult new words are coined for new ideas (such as "automobile"), not the other way round. One can think of ideas without naming them. When I see a man, I do not think "man." "When I think of a crowd of persons I do not think of a crowd of words!" Only in rapid thinking will the name of a person "nearly take the place of the concept of the individual as imagined" (p. 412, which brings us back to Steinthal's seminal suggestion). In short, "the concepts of 'meaning,' 'idea,' 'concept,' 'conscious,' 'purposeful,' 'association of ideas,' 'abstraction,' 'symbolization,' etc. . . . are fundamental to structural psychology . . . a consideration of the acquisition of language habits, their function in thinking, and the material of truly non-language thought, is totally inadequate without these concepts." (p. 413). Introspection, finally, is irreplaceable in the study of both verbal and non-verbal thought.

Just a month earlier, and completely independent of Otis' paper, the October 1920 issue of the *British Journal of Psychology* reported on a symposium called "Is thinking merely the action of language mechanism." It involved five papers, by Bartlett and Smith, Thomson, Pear, Robinson, and finally one by Watson as a rejoinder. Opinions differed widely, but nobody fully agreed with Watson. Tom Pear, supported by Bartlett and Smith (1920), argues that the explicit or implicit expression of a thought is not the thought itself, just like the figure traced by the skater is not the process of skating. Thought involves the recall of relevant past experiences, abstraction, comparison, and drawing conclusions. This is very different from a speech habit like saying "good morning." Watson's approach

does not help us to explain even the simplest experimental facts of thinking. How, for instance, can we learn 15 word pairs of similar meaning in one repetition, but not pairs of dissimilar meaning? How can we associate words that are meaning-related easily even though they have never been in consciousness together nor in immediate succession? Why do subjects give "the right thought" in "the wrong words"? Melrose (1922), in a review of the symposium, accused Watson of a "doctrinaire" reduction of objective data to "the reactions of muscles and glands." There is more to the objective data of thought than that. Verbal responses are expressions of thought "*in its audible sign system – language.*" (p. 130). This does not undermine behaviorism as "*the* scientific method for psychology," but only its doctrinaire limitation of psychology's full data.

Watson's (1920) rejoinder began with the statement that the behaviorist "has never really held the view that thinking is merely the action of language mechanisms," and he apologized for recent "over-emphasis" on his part (p. 87). He now "emends" his statement as follows (pp. 87–89): "A whole man thinks with his whole body in each and every part," which brings us back into the advertising jargon. "Deaf and dumbs" and individuals with their larynx removed can still think, Watson admitted, using other "parts" of their body. But this is still "activity substitutable for language activity," like "the shrug of the shoulder and the lifting of the brows." If we include all this under "subvocal behavior," then "thinking" is the correct term to cover all subvocal behavior.

Watson then proceeded to distinguish three types of thinking, hence (sub)vocal behavior (pp. 89–90). The first type is the production of fixed, invariable word sequences, such as hymns or "2 and 2 equal 4." The second type is similar, but the stuff is harder to reproduce. Now some "trial" behavior is needed. In the third type the problem is really new. Some action is needed, but it must first be worked out verbally. No single "habit" will bring about the required adjustment. Now Watson took the surprising step of reporting on the record of a thought which he had written down himself (p. 92). He had made his subject (a friend) think aloud about the use or function of a particular object in the passage to his apartment. The object was a hairdresser's hair-washing basin. It took two exploration sessions for his subject to solve the problem, resulting in "Ah! I have it!" The protocol reads like the protocols of Bühler and Selz. And indeed "we make our subjects think aloud and thereby observe a large part of the process of thinking" (p. 93). The training of introspection, however, makes the subject "pedantic and insufferably prolix and descriptive of his inward processes." If "we are ever to learn scientifically any more about the intimate nature of thought" . . . "we shall have to resort to instrumentation" (p. 97). Here Watson referred to unpublished data obtained by his young colleague Karl Lashley, showing that tongue movements while whispering a sentence are "wholly similar except for amplitude" (p. 97) to those recorded when the subject is just to think the same thing. (Most regrettably, however, five years later Agnes Thorson reported her extensive and quite sophisticated experiments using Lashley's sensitive equipment. Her findings were devastating. Internal speech is by no means always accompanied by tongue movements. If it is, these movements hardly ever show similarity to movements in the overt speech of the same words. They also show no similarity to those obtained in repetitions of the same internal thought. In short, they are nothing other than random motor activity.)

Responding to the argument by Pear and Bartlett/Smith, Watson explained that the studying processes of thinking or skating are indeed more than recording the implicit muscular activity, the ultimate "expression" of the process. Taking up the skating simile: much more must be noted down, all the way from the skater's training age, training scheme, training period, to, yes, his final verbal report. The latter, however, will "throw very little light upon the act he is engaged in." The skater may answer: "What was I thinking about? I was wondering whether that 'queen' over there in the red sweater was watching me." (p. 100).

And finally, what about "meaning"? Meaning is an abstraction according to Watson. He had studied the meaning literature at a seminar at Johns Hopkins. "A more barren wilderness of words it has never been my lot to meet." (p. 103). It is all quite simple. We watch "what the animal or human is doing. He means what he does." . . . "His action is the meaning. Hence, exhaust the concept of action and we have exhausted the concept of meaning." (p. 103).

Watson's simple psychology was somewhat of an ideology. It fitted the no-nonsense pragmatic view on science that dominated American academia between the two world wars. It was in particular appealing to educational psychologists who had to measure and train children's verbal abilities, as we will discuss in Chapter 10. But it was also the simplest variant of behaviorism, as will become apparent from the following sections.

Lashley added substantial sophistication to behavioristic philosophy, in particular in his two papers on consciousness of 1923. The psychology of language is not a major issue in these papers, but it is worth citing Lashley's general and somewhat paradoxical conclusions: "The statement, 'I am conscious' does not mean anything more than the statement that 'such and such physiological processes are going on within me.'" (Lashley 1923a, p. 272), "The attributes of mind, as definable on introspective evidence, are precisely the attributes of the complex physiological organization of the human body and a statement of the latter will constitute as complete and adequate an account of consciousness as seems possible from any type of introspective analysis. The behaviorist may go his way without fear that his final account will fail of including 'mind' and with the conviction that the inclusion of 'mind' will add nothing to scientific psychology." (1923b, p. 352). For Lashley "mind" is reduced to the mechanics of physiology. This position he shared with Meyer and Weiss, as we will see.[6] Their behaviorism, quite different from Skinner's, allows for theorizing about a "mediating mechanism."

Speech for social control: Grace de Laguna and John Markey

Grace Andrus de Laguna (1878–1978, Fig. 8.1) probably was the most liberal of behaviorists dealing with speech and language. She was a philosopher, not an empirical psychologist, with a PhD from Cornell University. In 1907 she obtained a position in the

[6] And with many others. Dashiell (1925), for instance, concludes that "the phenomena of thinking are consequently restatable as the physiological phenomena of interacting reaction circuits" (p. 73). Hunter (1924) claims language responses to be a mediating mechanism in thinking, explanatory of the "symbolic process." (p. 497).

Fig. 8.1 Grace de Laguna. Reproduced with permission of Marie-Françoise Guédon © 1906.

philosophy department of Bryn Mawr College in Philadelphia, mainly working on the philosophy of psychology. Her major contribution to the psychology of language is her 1927 book *Speech. Its function and development.* The book is explicitly behaviorist in the methodology it propounds. It is all right to believe in conscious processes, revealed by introspection, according to de Laguna, but it does not add anything scientifically and risks the pitfalls of dualism. Later in her long life Grace de Laguna moved away from behaviorism and involved herself with the anthropological work of her famous daughter Frederica. Her next book, essays in cultural anthropology, appeared when she was 88 years old.

De Laguna begins her book by addressing, or rather attacking, the traditional conceptions of language psychology, not so much because they are mentalistic, but because they are individualistic. Their basic premise, according to de Laguna, is that language is a means of expressing ideas, and indeed this is essentially Wundt's position, language as expressive movement. Expression of ideas is the fundamental function of speech. That point of view, according to de Laguna, is shared by a wide range of authors, including not only Wundt, but also Whitney, Paul, Tyler, Russell, and even Sapir ("Language is a purely human and non-instinctive method of communicating ideas, emotions and desires") (de Laguna 1927, p. 18). But these are "sterile" preconceptions. The real issue is "What does speech do? What objective function does it perform in human life?" (p. 18). The answer is: speech performs the fundamental function of social coordination. It is there "to stimulate and control the behavior of individuals with reference to each other, on the one hand, and with reference to the objective situation, on the other hand." (p. 21). De Laguna then

proceeds to sketch her theory of the "vocal-social" origin of language. She argues that speech did not develop from gesture, but from animal cries. Although animals usually respond to external stimuli in order to adjust themselves to the environment, that is not the case for certain vocal responses. The alarm cry, for instance, developed in evolution to stimulate *other* individuals so they will adjust themselves to new external conditions. The alarm cry has a social function. The way it works is two-pronged: it draws the attention of the other to the threatening stimulus *and* it triggers the appropriate response, for instance flight. These are two sides of a single, unitary response.

Over evolution, humans came to draw these two functions apart. While moving from arboreal to terrestrial life, cooperation became essential for survival. It was no longer the case that there was a single appropriate response to a threatening stimulus. The complexity of the environmental conditions called for quite different (group) responses in different circumstances, for different foes, etc. Also, in cooperative tool use, for instance in hunting, the same stimulus might call for a variety of adequate responses. Vocal calls therefore lost their function of triggering a specific response in group mates. They became *proclamations.* It would then depend on further distinctions to determine what course of action should be taken. Sometimes the group's leader would decide and give a *command,* or action was prepared in *conversation*, in which action goals are anticipated.

Speech as proclamation is, therefore, characteristically *indirect* in its social control. It must make others attend to some state of affairs, without immediately triggering their responses. This indirectness will have developed slowly and we can still observe the process in children. Their initial sentence words are as much proclamations as commands. Uttering *shoe* may mean nothing other than *give me the shoe*, i.e., calling for a specific action. Here de Laguna reinvents, without reference, the theorizing of Romanes and Meumann. Both in evolution and in the child, common nouns come to signal real objective continuity between situations. The same objects come to figure in more than one action situation and play different roles. The common noun abstracts from that variety. Words, and speech in general, became useful "in controlling the behavior of others with reference to objective features not directly present to perception" (p. 100). "The evolution of language is characterized by a progressive freeing of speech from dependence on the perceived conditions under which it is uttered and heard, and from the behavior which accompanies it." (p. 107). Here de Laguna differs drastically from Kantor, as we will see. For her, speech exerts its social control precisely by the objective "freedom" our language tool has attained, its independence of context. For Kantor, no word, no utterance has any meaning other than the circumstances of its use.

Proclamation is the primary function of language, subject-predicate its essential structure. Predication can be realized in different types of syntax, but a particular ordering of the syntactic elements is essential. This is even true in the spontaneously developed gesture language of deaf people and in newly arising pidgins. De Laguna (p. 105) also cites Jespersen's observation on Danish boy twins who had been practically uncared for until the age of four.

So far, nothing is specifically behavioristic here. But de Laguna declares she has adopted behaviorism as *method.* Observations made by one investigator should be verifiable

by others. That rules out introspection, or, more precisely, it rules out the experimental subject as his/her own observer. It does not rule out the use of the subject's verbal report. That is objective data like anything else, but then it is the *experimenter* who is the observer. (Again, there is no reference to Wundt's critique of the Würzburger's use of introspection.) Behavioristic methodology makes it possible to study psychological phenomena as *functions* of their objective conditions. It "objectifies" mind. Behaviorism considers stimuli, and more generally our environment, in terms of our responsive behavior. Psychologically our physical world is *relative* to our psychological repertoire. "Behaviorism attempts to apply a sort of relativity-theory to psychological phenomena."[7] In a sense, therefore, human behavior is not made up of "responses" at all. "The unit of behavior is the adaptive *act.* It is a *functional* unit." (pp. 147, 171). We will not follow de Laguna in her extensive treatment of the fundamentals of objective functional psychology, but close by considering some of her applications to language behavior.

The complete speech act elicits in the hearer a verbal reply or rejoinder, which in turn may elicit a response. This is *conversation.* The function of conversation is "primarily the preparation for concerted or socially determined action." (p. 280). This involves two phases, the analysis of the situation (the proclamation part) and the proper planning of action (the imperative part). Questions are stimuli that elicit verbal responses. Allegiance to behavioristic methodology in this text is hardly more than allegiance to behavioristic terminology. "The verbal response," we read, "is unique, in that it constitutes a specific response to what is objective." (p. 299). Self-talk becomes "the habit of internal speech," and so on. These are exercises in behaviorese. Although de Laguna is relatively modest in this respect, it is all there when she comes to discuss belief and thought, "higher forms of cognition." Our memory of events, she claims, is "verbalized to an indefinite degree," "in terms of propositions." (p. 317). The planning of purposive action, i.e., thinking, depends on verbal recall: "When one is able verbally to recall bits of past experience as present exigencies make them relevant, it is evident that one's chances for successful action are greatly increased." "In the activity of thinking itself, speech reaches the highest and most typical of its autonomous function." "Thinking is the internalization of this form of conversation and its independent practicing by the individual." (pp. 352, 353). The explanatory force of all this is far from obvious. It is painful to compare such ruminations about thought to the sophistication of Selz's approach, apparently unknown to the author.

De Laguna stands in the philosophical tradition of Dewey's and Mead's social behaviorism (which we do not consider here). It was George Herbart Mead's idea, for instance, to consider thinking as internalized conversation. Another treatise in that tradition, by John Markey (1928), appeared just a year after de Laguna's. It is about the "symbolic process." The function of symbols, including language symbols, is to exert social control, not to "express ideas." It occurs "in the social–vocal–auditory situation" (p. 168). The mutual control of person and group creates the "self" and "other" aspects of social

[7] De Laguna (1927) p. 134. The notion of relativity was very much of the *Zeitgeist.* Wundt (1920) discussed its relevance for psychology, p. 202.

interaction, and it is certainly objective: "The behavioristic explanation of reflective behavior, showing how the whole body, muscles, glands and bodily organs, is involved, means that symbolic stimulation by one's self, or by others, also exerts a reciprocal influence over muscles, organs, glands. etc." (p. 170). We will return to some data analyses in Chapter 10 on language acquisition. Here we close with a few citations from a review that John Markey published in 1929. It shows the diverging of opinions, or rather the unbridgeable chasm between the traditional mentalistic and the "objective" behaviorist psychology of language. It is a short review of *The psychology of language* (1928) by Pillsbury and Meader, which we introduced in Chapter 6. The book "is so handicapped by a moribund system of psychology, by the ignoring of recent developments in physiology, psychology, . . ., social psychology, and sociology that very serious reasons against its use as a text are obvious," Markey writes, and "The dynamics of biological functions, glands, viscera, muscular responses which enter into language are ignored," which is unforgivable for the faithful behaviorist.

From Stumpf to Bloomfield

Behaviorism affected not only American psychology, but also linguistics. Linguist Leonard Bloomfield became the central figure in this transition. This section discusses how this transition took place and how Bloomfield's brand of behaviorism, in its turn, affected Zellig Harris and his student Noam Chomsky.

Erwin Esper (1968) left us with a detailed, scholarly account of the historical roots which engendered Bloomfield's conversion to behaviorism. Esper (1895–1972) was trained as a linguist at Ohio State University. His supervisor, classicist George Bolling, sent him to the laboratory of psychologist Albert Weiss, who helped Esper to replicate Thumb and Marbe's experiments on analogy. Weiss was a personal friend of Leonard Bloomfield and, as we will see, Bloomfield's main source of behavioristic inspiration. Bloomfield was kind enough to help Esper design his verbal materials. They became life-long friends. Esper deeply abhorred Wundtian psycholinguistics, but unlike most behaviorists he knew it in minute detail. We will discuss his early experimental work in Chapter 12; here we will, among other sources,[8] use his account of the "making" of Bloomfield the behaviorist.

Max Meyer

Albert Weiss, who "converted" Bloomfield, had himself been a student of Max Meyer (1873–1967). Our story must begin there. Max Meyer studied in Berlin, where he was strongly inspired by physicist Max Planck and psychologists Hermann Ebbinghaus and Carl Stumpf. If these men had anything in common, it was that they were all scientists. They shared the opinion that the scientific study of mental processes should take the "external view." Max Planck put it this way: "Observed from without, the will is

[8] Other helpful sources are R.H. Wozniak (1994, 1997a, and 1997b).

causally determined. Observed from within, it is free." (Esper 1968, p. 116). You can have first-hand knowledge of what you feel or think, but others can only infer it from your words or actions. For Meyer, this meant a causal, scientific theory of mental processes should take the external view. His psychology would be *Psychology of the other-one*, which became the title of his 1921 psychology text. Meyer's first experimental work, as a doctoral student and later research assistant of Carl Stumpf (1848–1936), was in acoustics, i.e., sound and music perception. Meyer was dismissed by Stumpf in 1898 following a quarrel about the interpretation of data on musical consonance-dissonance (Esper 1968, p. 122).[9] This dismissal did not diminish Stumpf's influence on Meyer's thinking. Stumpf's experimental approach was psychophysical, but he rejected Wundt's doctrine of psychophysical parallelism, which was dualistic. It is "the brain processes, which might be regarded as the immediate cause and effect of particular mental activities." (p. 121). The rejection of dualism and the objective, quantitative methodology in Stumpf's approach, made him in retrospect a proto-behaviorist for Max Meyer. In 1966 Meyer wrote: "In *Tonpsychologie* Stumpf was a 'behaviorist' without knowing himself this fact, obvious to me now." (p. 118). And he had the same qualification for Ebbinghaus: "Ebbinghaus, whom I knew well, was surely a behaviorist, although, of course, he could not name himself by such an English designation." (p. 149). Stumpf lived long enough to explicitly deny that he was a behaviorist, in a 1927 letter to Meyer (p. 127), but Meyer's point was simply that Stumpf and Ebbinghaus were models of empirical rigor and objectivity. Objectivism was not a new invention of John Watson; it had been long practiced in continental psychology. Meyer's own *The fundamental laws of human behavior* (1911) preceded Watson's manifesto by two years. Another inheritance from Stumpf was Meyer's disdain for Wilhelm Wundt. In 1891 a bitter clash between Wundt and Stumpf arose over some experiments in tone interval perception executed in the Leipzig laboratory. According to Stumpf, Wundt's criticism of Stumpf (1991a) "is in all points based on distortions and presuppositions" and it "abounded with insults to my scientific skills."[10] Meyer decided to further ignore Wundt. In a short review of Hoffmann's (1922) *Würdigung* that had appeared shortly after Wundt's death, Meyer (1923) wrote: "But little reason for its publication will be seen by those who regard Wundt as playing in the history of science only the rôle of an astonishingly industrious and mainly for that reason during his time highly respected plodder." But for Meyer ignoring Wundt did not mean ignoring language or ignoring the Leipzig experimental tradition. As a 16-year-old boy, Meyer came across Geiger's *The origin of language*, which deeply impressed him, in particular Geiger's idea that all thinking is "inner speech," which in Meyer's own terms became "that all thinking is governed neurologically by mediation of speech. That fixed my psychology for the remainder of my life." (Esper 1968, p. 113). Meyer was also a life-long admirer of Titchener, Wundt's former student, who was an experimentalist, introspectionist, and opponent of behaviorism.

[9] See Wozniak's article on Meyer (1997a).

[10] "welche in allen Punkten auf Verdrehungen und Unterstellungen beruht, den Kern des Angriffs unberührt lässt, dagegen von Insulten auf meine wissenschaftlichen Fähigkeiten strotzt." (Stumpf 1891b, p. 266).

After being dismissed by Stumpf, Meyer decided to emigrate and eventually found a position at the University of Missouri in 1900. Meyer's own contributions to the psychology of language are limited. There is a chapter called "The other-one's talking machinery" in his 1921 textbook. It is an excellent chapter for what it is, a treatment on articulatory phonetics and the child's acquisition of speech sounds. The child learns by imitation, Meyer claims, and this is peculiar enough, because other types of imitation are largely absent during the period of speech sound acquisition. Auditory imitation is reflex-like. The child reacts to a sound by producing a (similar) sound. There is no such reflex or instinct as "imitation," but sound imitation is reflexive in the child. Later imitative behavior of the child is not reflex-like; it is instead based on the knowledge that imitation is useful. But by then, reflex-like sound imitation gets lost, which explains our difficulty in acquiring a foreign language. The very early appearance of sound imitation not only marks its essential function in "the business of living a human life," but also its hereditary nature (Meyer 1921, pp. 328–329). Unfortunately, Max Meyer was suspended from his university in 1929 and later dismissed, because he had tacitly approved the distribution of a questionnaire assessing students' attitudes toward extramarital sexual relations. The questionnaire had been distributed by his student Hobard Mowrer, whom we will meet at the end of this chapter. Meyer went on to devote himself to the training of deaf children. His 1934 book *Fitting into a silent world* presented his invention of a clever phonetics-based stenograph system for deaf people and provides detailed longitudinal studies of its application in Deaf (oral) education.

Albert Paul Weiss

We now turn to Meyer's student, Albert Paul Weiss (1879–1931). Weiss's place in the history of language research is clear: he was the one who converted Leonard Bloomfield to behaviorism. This is what Bloomfield had to say in his 1931 obituary for Weiss: "Weiss was not a student of language, but he was probably the first man to see its significance." (p. 219). After this dismissal of all earlier linguistics, Bloomfield continued: "He saw that language supplied the key to those phenomena of human conduct and achievement which hitherto had been attributed to non-physical forces. . . . Weiss was a devoted pupil of Max Meyer; the latter's system, most thorough in eliminating animism and finalism, formed the basis of Weiss's work. The pupil's enormous advance was due to his evaluation of language. In addition to 'handling' responses, man has developed a system of vocal responses, language. These vocal responses serve as stimuli to the speaker's fellows: the members of a speech-community co-operate, by means of language, in spite of the discontinuity of their nervous systems." (p. 219). These members have been trained to produce and to respond to conventional speech sounds. "By virtue of this common attunement the members of a speech-community co-operate; the space between the nervous systems is bridged, from moment to moment, by the sound-waves which they utter and hear. Beyond the other types of organization – atomic, molecular, crystalline, protoplasmic-unicellular, multicellular – language brings into the universe the *compound multicellular* or *social* type of organization." (p. 220). It was from Weiss that Bloomfield adopted this full-blown version of behaviorese. The obituary cites a deep aphorism from

Weiss (1925a): "The language mechanism forms the characteristic factor in human behavior"[11] and ends with "The time may be near when Albert Paul Weiss will be counted a heroic figure in the progress of science." (p. 221). In short, there can be no doubt over Bloomfield's indebtedness to Weiss.

Albert Weiss (1879–1931), born in Germany, but raised in St Louis, studied at the University of Missouri, where he became Meyer's research assistant and his only PhD student. In 1912, before receiving his PhD (in 1916), Weiss became an instructor in psychology at Ohio State University, where he would stay in successive professorial ranks till the end of his short life. In 1917 Weiss published two papers in *Psychological Review* analyzing the basic tenets of structuralism and functionalism, and arguing for a new, behaviorist approach in psychology.[12] They are a natural elaboration of Meyer's views; as Weiss puts it: "These assumptions are not essentially different than those formulated by Max F. Meyer." (Weiss 1917a, p. 315). Weiss's attack on structuralism used Wundt's former student Titchener as the target (Wundt himself was systematically ignored in Meyer's circle). Titchener shared Wundt's view of *parallelism* between conscious and neural processes; they are considered to be two sides of the same coin. It is wrong to say that mind and body can influence one another, because they are "two aspects of the same world of experience." This means that "any change which occurs in the one, will be accompanied by a corresponding change in the other."[13] However, Weiss argued, not all behavior has a correlate in consciousness. Think of reflexes and automatic actions. Consciousness only becomes a scientific datum if it is expressed in speech. But such "introspective" expressions are more variable than the behaviors themselves. The latter are much more predictable than the former. Hence, behaviors should be the sole subject matter of a scientific psychology. In fact, consciousness as studied by structuralists is nothing other than a collection of highly trained speech habits. Here, the tacit target is clearly the "trained introspection" of the Würzburgers. The classical "contents" of consciousness, sensations, images, and feelings, are a product of such introspection, not of adaptive behavior. "Under the ordinary conditions, sensations, images and affections are absent. The average man probably never has them; he must be trained to have them in the same way that we must be trained to read and write." (p. 312).

Functionalism is an even easier target for Weiss. Here his scapegoats are James and Meyer's teacher Angell. Their issue is: "How does consciousness function in human behavior or conduct?" (Weiss 1917b, p. 354). But how can consciousness control behavior? You are walking, a neural process which transmits what the eye receives as input ("sight of the road") to the locomotor musculature ("walking activity"). Now you see an electrical wire dangling from a telegraph pole and you stop walking. Did you do this because you got the conscious idea of danger? How can this idea act on the neural process?

[11] Weiss (1925a, p. 86).

[12] Weiss (1917a,b).

[13] Titchener (1914, p. 9).

(Remember, this was exactly the problem Steinthal encountered.) There is no way out, according to Weiss, other than to state that "the *neural correlate* of consciousness, rather than consciousness as such," is "the factor that modifies action" (p. 360). Stronger, and surprisingly modern: "the conscious processes *follow* the neural processes, they do not *lead* them" (p. 362). And finally, the introspections so highly valued by the functionalists are no more than minor speech habits in the major stream of adaptive behavior.

There was one additional theoretical paper, "One set of postulates for a behavioristic psychology" (Weiss, 1925a), which Bloomfield would later take as a model. It outlined the place of psychology within the physical hierarchy from electrons to civilization at large.

These three papers set the scene for all of Weiss's further publications, in particular his 1925 textbook *A theoretical basis of human behavior.* Here we must limit ourselves, however, to what Weiss had to say about language. In that respect the text is rather limited. One looks in vain for anything empirical. Weiss makes a distinction between "biophysical" and "biosocial" responses, which Bloomfield embraces. "Behaviorist psychology occupies an intermediate position, on the one hand investigating the effects of physical conditions on sensory-motor functions, and on the other, the effects of sensori-motor function on social organization." (Weiss 1925c, p. 55). The effect on the hearer of speech as a stimulus is "biophysical," but speech as a speaker's response is "biosocial" because the sound waves transmit it to the nervous system of another person. In the chapter on language responses we read: "The behaviorist is not concerned with determining the exact effectors that function in a given speech reaction; his problem is that of determining how the final result (whether this is a sound, a written word, a bodily movement, a facial expression) acts as a stimulus on others and thus determines the social status of the individual. Two socially equivalent responses may be entirely different from the motor side (the *sound* of a word, or the same word *printed*) or they may be identical from the motor side but different as social stimuli, such as the oral words, *their* and *there*." (pp. 289–290). The latter is good behaviorese for "homonyms differ in meaning." The chapter also presents Meyer's imitation theory of speech sound acquisition.

Weiss displays a disappointing lack of vision of what a (behaviorist) psychology of language should achieve. That program is outlined in his 1925 paper "Linguistics and psychology." Among his seven "language problems" are: Can we "by systematic effort modify the English language to give the maximum accuracy to description and the greatest ease in learning?," "Can the parrot-like language habits be replaced by habits in which the individual modifies his language responses in conformity with the progressive alterations in his environment?," "How can we measure the excellences of various pronunciations?," and "How much of what we call progress in civilization is due to the specific type of language behavior which a nation has developed?" (Weiss 1925b, pp. 54–55). Apparently, a whole literature had been wasted on Weiss.

Weiss's legacy was less his writings than his intellectual influence on his Ohio State colleagues and PhD students, who esteemed the care and attention he gave them in spite of his debilitating, fatal heart condition. Leonard Bloomfield was one of them.

Fig. 8.2 Leonard Bloomfield.

Leonard Bloomfield

The six years that Bloomfield (Fig. 8.2) taught at Ohio State University (1921–1927) were critical for his views on the relation between linguistics and psychology. As a colleague and then dear friend of Albert Weiss, Bloomfield was converted from a Wundtian mentalist to a Weissian behaviorist. This hardly affected his linguistic work, but was certainly decisive for his place in the history of psycholinguistics. Leonard Bloomfield (1878–1949) was born in Chicago, where he also did his PhD (in 1909) at the University of Chicago on a topic in Germanic sound change. After a year as instructor at the University of Cincinatti, he accepted a position at the University of Illinois at Champaign-Urbana, where he stayed till 1921, interrupted by a study visit to Germany during the academic year 1913–1914. After his years at Ohio State University he returned to the University of Chicago as professor of German philology. In 1940 Leonard Bloomfield accepted a professorship of Germanic Languages at Yale University in New Haven. There he died in 1949.

In Germany Bloomfield first went to Indo-Europeanist Wackernagel in Göttingen and then to Neogrammarians Leskien and Brugmann in Leipzig. He also attended the 81-year-old Wundt's lectures. In Leipzig he must also have been working on his first textbook, *Introduction to the study of language*, which appeared in 1914. As mentioned in Chapter 7, this is still the best English-language introduction to Wundt's linguistic notions, which are psycholinguistic in nature. The most lasting influence of Wundt on Bloomfield and, through him, on modern linguistics is, however, what Bloomfield and his followers were going to call "immediate constituent analysis." Any complex form, whether syntactic or morphological, should recursively be analyzed into its "immediate constituents." The immediate constituents of *poor John ran away*, for instance, are *Poor John* and *ran away*. The immediate constituents of *Poor John*, in turn, are *poor* and *John*; those of *ran away* are *ran* and *away* (Bloomfield 1933, p. 161). This is, clearly, Wundt's principle of binary ("apperceptive") partitioning. This was all adopted, except for the drawing of tree diagrams, which Wundt had appropriately introduced.

The first explicit formulation of Bloomfield's new behaviorist position appeared in his important paper "A set of postulates for the science of language," which he published late in 1926, towards the end of his period at Ohio State University. The title of the paper makes reference to Weiss's *Postulates* paper, which had appeared two years before. Let us cite from the introduction:

> Psychology, in particular, gives us this series: to certain stimuli (A) a person reacts by speaking; his speech (B) in turn stimulates his hearers to certain reactions (C). By a social habit which every person acquires in infancy from his elders, A–B–C are closely correlated. Within this correlation, the stimuli (A) which cause an act of speech and the reactions (C) which result from it, are very closely linked, because every person acts indifferently as speaker and hearer. We are free, therefore, without further discussion, to speak of *vocal features* or *sounds* (B) and of *stimulus-reaction features* (A–C) of speech.

It is the schema that Bloomfield would substantially elaborate in his *Language* (1933) by way of his famous *Jack and Jill* tale, but the essence of his new position on the relation between psychology and linguistics is all there in the *Postulates* paper. In a footnote on the same page, Bloomfield makes reference to this statement by Weiss: "The significant thing about the speech reaction is that it may be either the adequate *reaction* to a situation, or it may be the adequate *stimulus* for either another speech reaction or some bodily reaction." Linguistics deals exclusively with B, the speech itself. How it is triggered in the speaker by stimulus A, or how it is causative of speech or "handling" reactions in the hearer, is a matter of psychology. Neither the speaker's "physiology," not the hearer's "acoustics" are part of linguistics: "Thus, the physiologic and acoustic description of acts of speech belongs to other sciences than ours." Moreover, the "existence and interaction of social groups held together by language is granted by psychology and anthropology."[14] Bloomfield makes the separation of linguistics from psychology complete by aligning himself with Delbrück, who was "right in saying that it is indifferent what system of psychology a linguist believes in." (p. 153).

In another footnote Bloomfield states his indebtedness to Saussure's *Cours* and Sapir's *Language* for their steps toward a delimitation of linguistics. It is indeed interesting to compare the *Cours*' delimitation to Bloomfield's. In the *Cours* linguistics deals exclusively with the psychological part of the signing circuit, namely the moving from concepts to sound images in the speaker and the reverse process in the hearer. Bloomfield recognizes neither "concepts" nor "sound images" as valid scientific notions, let alone their relations. It is useful to compare the signing diagram from the *Cours* (Fig. 7.2) to the diagram in *Language*, which is Bloomfield's diagrammatic representation of the following statement. When Jill, walking with Jack, is hungry and sees an apple in a tree, she "makes a noise with her larynx, tongue and lips. Jack vaults the fence, climbs the tree, takes the apple, brings it to Jill, and places it in her hand." (Bloomfield 1933, p. 25). This gets represented as:

S >>>————> r.....s >>>————> R

[14] Weiss (1918, p. 636).

Here S is the complex stimulus of Jill's hunger and sight of the apple, and r is the speech response it causes Jill to produce. That response is at the same time the stimulus s which triggers Jack's "handling response" R, climbing the fence, grabbing the apple and putting it in Jill's hand. The r . . . s speech event here, which is for Bloomfield the core topic of linguistics, coincides with the *physical* process from mouth to ear in Saussure's diagram, which Saussure explicitly excludes from linguistics. Bloomfield grants to psychology everything the *Cours* granted to linguistics. Whatever Saussure meant with the relations between concepts and sound images, when translated into behaviorese, must be either part of S >>> → r or of s >>> → R above. That is, of course, the relation between sound and meaning. But what is "meaning" in Bloomfield's behaviorese?

The same *Postulates* paper provides Bloomfield's initial answer: "The vocal features common to same or partly same utterances are *forms*; the corresponding stimulus-reaction features are meanings." This means that the totality of S >>> → r and s >>> → R occurrences that go with the utterance form r . . . s is the meaning of that form. This reflects Weiss's definition: "For the behaviorist the meaning of a stimulus is defined by the responses that are made to it." And in some more detail: "The various deviations in the responses, or series of responses, are the different meanings, although in the case of word stimuli there is one series which is the accepted biosocial response. This is usually what is given as the dictionary meaning of the term." (Weiss 1929, pp. 325–326). In *Language* Bloomfield puts it this way: "the meaning consists of the important things with which the speech utterance (B) is connected, namely the practical events (A and C)" (p. 27), and in his chapter on *Meaning*: "We have defined the *meaning* of a linguistic form as the situation in which the speaker utters it and the response which it calls forth in the hearer." (Bloomfield 1933, p. 139). That creates, of course, a problem for the (psycho-) linguistic study of meaning. Put in his own words: "The situations which prompt people to utter speech, include every object in the universe. In order to give a scientifically accurate definition of meaning for every form of a language, we would have to have a scientifically accurate knowledge of everything in the speaker's world. The actual extent of human knowledge is very small, compared to this." (p. 139). Bloomfield then mentions a few ways of handling this. One way is to have other sciences define words, for instance chemistry defines "salt" as NaCl. But then, the meanings of such words in language may still be different. Hence, the "statement of meanings is therefore the weak point in language-study, and will remain so until human knowledge advances very far beyond its present state. In practice, we define the meaning of a linguistic form, wherever we can, in terms of some other science." (p. 140). Meanwhile, we can use makeshift devices, such as demonstration, circumlocution, or translation, for specifying lexical meanings. Bloomfield then constructs a rather opaque argument which leads to the happy conclusion that "the mentalist in practice defines meanings exactly as does the mechanist, in terms of actual situations; . . . In practice, then, all linguists both mentalists and mechanists, define meaning in terms of the speaker's situation, and, whenever this seems to add anything, of the hearer's response." (p. 144). From here on, Bloomfield is back to business as usual, treating matters of lexical meaning, such as homonymy, synonymy, metaphor, and so on in the traditional way.

Bloomfield uses the impenetrability of situational meaning to argue for a primarily form-based linguistics. "It follows from all this that in all study of language, we must start from forms and not from meanings." (Bloomfield 1943, p. 102). Indeed, if anything is characteristic of Bloomfield's structuralism, it is its concentration on form, phonological, morphological, and syntactic. Bloomfield attained a new level of formal sophistication, which was initiated in his *Postulates* paper. The paper provides precise definitions of the basic grammatical notions, such as "utterance," "morpheme," "free" and "bound form," "word," "phoneme," "construction," "sentence," and so on. It also provides a set of assumptions, such as the finiteness of the set of forms, the inclusion of the set of phonemes, and the set of constructions in the set of forms as proper subsets. Here Bloomfield's influence has been paramount. Bloomfield's form-based linguistics ultimately led, via Harris, to Chomsky's generative grammar. But the paper also marked the complete divorce of linguistics and psychology. Bloomfield's definition of the sentence is symbolic here: "A maximum construction in an utterance is a *sentence.* Thus, a sentence is a construction which, in a given utterance, is not part of any larger construction." (Bloomfield 1926, p. 158). Compare this to Wundt's definition, the one Bloomfield followed in his 1914 text: "The sentence is the linguistic expression of the volitional analysis of a total image into its parts that are put in mutual logical relations."

Bloomfield's alignment with behaviorism was largely lip service. But he was good at that. He was, time and again, an able contributor to behaviorese. Let me cite from just one article. In 1936 Bloomfield published a paper "Language or ideas?" in which he welcomed Carnap and Neurath from the "Vienna circle" to the club of objectivists. Like the behaviorists, they make the point that "all scientifically meaningful statements are translatable into physical terms – that is, into statements about movements which can be observed and described in coordinates of space and time. Statements that are not made in these terms are either scientifically meaningless or else make sense only if they are translated into statements about language." Bloomfield then provides various examples. For instance, the statement "*Redness is a concept*" . . . "makes sense only if it is translated into a statement about language, namely: *In the English language the word redness is a noun.*" (p. 90). Or take the term "idea": "It remains for linguists to show, in detail, that the speaker has no 'ideas,' and that the noise is sufficient – for the speaker's words act with a trigger-effect upon the nervous systems of his speech-fellows." (p. 93). Further on in the paper Bloomfield goes through the exercise of translating a whole mentalistic paragraph into respectable mechanistic behaviorese. Here is the last part of it. The passage:[15]

> We are thus forced to conclude that geometrical definitions are the results of processes which may be started, but the limits of which can never be reached in perception; they are pure conceptions having no correspondence with any possible perceptual experience.

should be translated into:

> We are thus forced to conclude that the geometrical definitions are simply verbal descriptions which roughly describe classes of objects but do not exactly describe any given object. In this the geometrical terms are like all other speech forms.

[15] pp. 94–95. Bloomfield uses a text from Pearson (1911).

As we will see, more than 20 years later, Skinner still goes through a similar exercise.

Discussing Bloomfield from the psycholinguistic perspective is necessarily limited in scope. It does not take account of much of his life's work as a linguist. It is, for instance, beyond the aims of this book to review Bloomfield's oeuvre as a descriptivist of a vast collection of languages, from Tagalog to Dutch. Neither can we go into further details of his work in theoretical linguistics.

Bloomfield's behaviorist heritage: Zellig Harris and Noam Chomsky

Zellig Harris (1909–1992) spent his academic life at the University of Pennsylvania, where he founded the country's first linguistics department (in 1946). He followed in Bloomfield's and Sapir's footsteps. Sapir had positively reviewed Harris's 1934 dissertation (published in 1936). Sapir's work in phonology was particularly inspirational to Harris. But it was Bloomfield's methodological approach to the analysis of *immediate constituents* (IC -analysis) which Harris adopted as his main project. His *Methods in structural linguistics* (1951, but apparently completed in 1947) brought the methodology of distributional analysis to full fruition. It describes, chapter by chapter, the operations by which, level after level, the units of a language can be derived from a (large) corpus of utterances, as well as their recurrent combinations. Harris, like Bloomfield, thought that sound structure was the clearest battle ground. He defines operations for discovering a language's minimal set of phonemes[16] and their co-occurrences. These co-occurrences, in turn, form the basis for discovering a language's morphemes, both free and bound forms. At the next level, distributional properties of morpheme sequences are analyzed to discover a language's constructions.

Like Bloomfield, Harris only deals with linguistic *form*. Meaning, according to Harris, is "in the last analysis, the correlation of utterances with the social situation in which they occur." (Harris 1951, p. 187). That is, as we saw, Bloomfield's behavioristic position (and his excuse for not considering meaning). Still Harris leaves the door to meaning ajar, because he needs it methodologically. Corpus analysis alone will, in spite of its sophistication, often not suffice for the linguist. It will, in many cases, be necessary to check with an informant, a native speaker of the language. You may want to know whether one utterance is a repetition of another one or whether the two contrast. A native speaker of English will probably judge *camera* starting with *k* and *camera* starting with aspirated k^h as a repetition of the same utterance. But when presented with *It's lacking* and *It's lagging*, the two utterances will be judged as contrasting. They normally occur in different social situations and thus are experienced as having different meanings. Another methodological use of meaning in Harris's work is *paraphrase*. Harris (1952) introduced the notion of transformation into linguistics.[17] He set out to analyze the distribution of sentences in

[16] Elaborating the existing method of complementary distribution.

[17] But see also Wundt (1900), Vol. II, p. 257 and here, p. 197 and a much older tradition reviewed in Seuren (1998, chapter 1). Charles Morris (1938, p. 14) discusses the notion of *transformation rules* introduced by logical syntacticians such as Carnap.

larger texts. He observed that it would simplify his discourse analysis by *transforming* certain sentences in the text into grammatically equivalent ones. So, for instance, the complex sentence *I telegraphed that we'll arrive tomorrow* can be transformed into *I telegraphed: We'll arrive tomorrow.* Here, the relation is one of paraphrase. It is the beginning of Harris's program of reducing sentences to their underlying forms: "Given any sentence, we can check it for all transformations; we will then find the sentence to consist of a sequence of one or more underlying sentences." (Harris 1957). It is the sentence's "factorization." Still, Harris avoids the methodological use of meaning as much as possible. "The considerations of discovery furnish one of the reasons for avoiding any classification of forms on the basis of meaning." (Harris 1951, p. 372).

Although IC analysis, also in Harris's sophisticated version, was largely a bottom-up, inductive affair, step-wise climbing from the smallest constituents in a corpus to ever larger ones, from phonemes to morphemes, constructions, sentences, and (for Harris) units of discourse, Harris (1951, pp. 372–373), in a flight of insight, conceived of the deductive approach we cited in Chapter 1, which predicts the utterances of a language from an axiomatically defined system of theorems. The notion of a generative grammar clearly arose in the behavioristic current of linguistics. Harris (1951) hardly provided any detail. It was his student Noam Chomsky who developed the notion in his 1955 dissertation.

Noam Chomsky (born in 1928) wrote his dissertation under Harris's supervision. It is the first landmark publication in generative grammar and is an enormous 752-page struggle[18] to formulate phrase structure analysis in terms of a deductive, formal system. "With its law-like rules for the combination of elements, a grammar can thus be said to 'generate' a certain set of utterances on the basis of an observed sample." (Chomsky 1955, p. 7). It is beyond the scope of this chapter to discuss this enterprise in detail.[19] Suffice it to say that Chomsky developed the formal generative machinery for the formulation of finite state grammars, phrase structure grammars, and transformational grammars. In addition he argued, from linguistic considerations, that a transformational component will be essential to handle natural languages, such as English. At issue here is early Chomsky's methodology, which further sharpened the current behavioristic practice. A few citations make the point. Chomsky is even stricter than Harris in avoiding anything that smacks of meaning or intuition: "The central objection to meaning as a criterion of analysis has always been the obscurity of semantic notions. I think it is indeed fair to say that we are currently in pretty much the same state of unclarity with regard to meaning as we are with regard to intuition. And this is a sufficient reason for refusing to admit meaning into linguistic theory." (p. 22). Chomsky did not deny that meaning is a chapter of linguistics. It would deal with phenomena such as synonymy, ambiguity, and analyticity. But meaning can play no role in the analysis of form, be it phonological, morphological or syntactic. "Whatever the situation may be in other sciences, I think that there is hope of developing

[18] Harris' "supervision" was in fact quite liberal (Aravind Joshi, personal communication). The master's hand is hardly visible in the dissertation's wide-roaming intellectual excursions.

[19] But see Levelt (1974/2008).

that aspect of linguistic theory being studied here on the basis of a small number of operational primitives, and that introduction of dispositions (or mentalistic terms) is either irrelevant, or trivializes the theory." (p. 21). The bit of operational use of meaning that Harris could not entirely avoid, is explicitly rejected by Chomsky. The native speaker is only called in to execute a "behavioral discrimination," to perform the "pair test." If the corpus contains two utterances U_1 and U_2, for instance *ladder* and *latter*, can they be consistently and repeatedly be distinguished by the informant? This has nothing to do with meaning (according to Chomsky), just as recognizing that *pill* rhymes with *bill* has nothing to do with meaning. Chomsky is optimistic that free variants are simply not discriminated by naive informants. "The pair test is one of the operational cornerstones for linguistic theory." (p. 34).

There was no place for meaning or intuition in Chomsky's early generative analysis. There is no hint in Chomsky's dissertation that he would soon become a leader of the so-called "cognitive revolution."

Kantor's psycholinguistics

It was, to the best of my knowledge, Jacob Robert Kantor (1888–1984) who introduced the term "psycholinguistics," or rather used it as an adjective in a section heading of his 1936 book *An objective psychology of grammar*[20] (see Fig. 8.3). The term does not appear anywhere else in the book, but Kantor probably used it more often in his teaching, because it was his student Henry Pronko who, in 1946, published *Language and psycholinguistics: A review.* From then on, "psycholinguistics" was an accepted term.

Jacob Kantor was a prolific theoretical psychologist, well-read in the history of psychology and a philosopher of psychology as a science. He had no training in experimental psychology. His 1917 University of Chicago dissertation was a critical evaluation of two millennia of psychology in the hands of philosophers. In 1920 he moved to Indiana University, where he taught psychology till his retirement in 1959. In the next quarter century of his long life he taught at several universities, but then settled again in Chicago. There he died in 1984, leaving a note on his desk summarizing his life's intellectual targets: "No spirits, wraiths, hobglobins, spooks, noumena, superstitions, transcendentals, mystics, invisible hands, supreme creator, angels, demons, ..."

Indeed, Kantor was an iconoclast, never tiring of fighting "spiritistic" psychology. At the age of 90 he addressed the new cognitive psychologists with their "psychic constructions," accusing them of returning to the Church Fathers and their "antinaturalistic writings."[21] It is also very much the style of his *An objective psychology of grammar*, which is the main topic of the present section. It concerns only a fraction of Kantor's writings, but the notion of language as "psychological adjustments" intrigued him. In 1977, at the age of 89, he published a further monograph on the topic, *Psychological linguistics.*

[20] Kantor (1936, p. 55). Benjamin Whorf had earlier, in 1932, used the term "psycho-linguistic" in an unpublished paper. See Lee (1996, p. 164). He also used it (unhyphenated) in a 1936/37 paper, also unpublished, which Carroll edited and published in Whorf (1956); the term appears there on p. 74.

[21] Kantor (1977).

THE PSYCHOLINGUISTIC SITUATION ANALYZED

Despite the unfortunate contacts between psychology and linguistics, they must nevertheless coöperate in their language studies. We conclude therefore that what is required is a re-examination of our concepts of both psychology and linguistics.

Fig. 8.3 Jacob Kantor with his text introducing the term "psycholinguistic." From Kantor (1936, p. 55).

Kantor's 1936 monograph is an elaboration of his 1922 paper on language, which sets the scene for all later work on this topic. Considering language in terms of signs and symbols, the paper says, "harks back to the old spiritistic psychology. Language according to this tradition consists of words or acts which materialize spiritistic or mentalistic states. That this worthless and outworn tradition need not stand in the way of a correct understanding of language is indicated by the availability of a completely objective psychology which can entirely supplant the mentalistic tradition." Correspondingly, "we conclude that language as living phenomena cannot be regarded as other than behavior. Language consists of a series of adjustmental interactions and not a set of symbols." (Kantor 1922, pp. 156, 158).

That, indeed, is the refrain which returns on almost every page of *An objective psychology of grammar*: "Psychological grammar then is the study of how language adaptations are actually carried out." (p. 32). Stated negatively, language is not a tool or instrument, words are not language, fixed expressions such as "good morning" are not language, and so on: "living language is behavior, something that a person does (no other conception is admissible) we must exclude the materials of modern as well as of classical languages from the domain of linguistic behavior." And: "Until we actually have before us linguistic adjustments, we have no psychological language. Linguistic adjustments, like all others, exist only when they are performed." (pp. 23, 24). Nowhere in the whole book is it explained what a "linguistic adjustment" actually is. It probably just means "response." Response to what? On this point Kantor is more explicit. What makes the linguistic response special is that it characteristically involves two stimuli, not one. In the case of language there is a "triadic relationship." There is, first, the behavior of the reacting person ("speech"). Second, there is the stimulus function of the object referred to. Third, there is the person who is reacted to. In traditional terms: the speech, the referent, and the hearer. The "speech" is in quotes, because the behavior can be gestural or whatever, as long as the triadic relation holds. Such behavior is by definition linguistic. "Language is the only type of psychological interaction that involves such a three-cornered situation." (p. 73). There is a passing reference here to Karl Bühler's *Sprachtheorie*, which had just appeared, which distinguishes a fourth factor, namely the speaker or "sender" (see Chapter 13). But that is one entity too many, because the only thing observable is the speaker's behavior, as related to the two stimuli, the referent and the person addressed. When you

say *Please hand me the book*, the referent is the book. Kantor calls this the main or "adjustment" stimulus. The hearer is also there as an "auxiliary" stimulus. Now the roles may get reversed. The hearer is also doubly stimulated. The book functions, as before, in the role of adjustment stimulus, but now your vocal action is for the hearer the auxiliary stimulus. The hearer's response may be the handing over of the book. In short, this is Bloomfield's *Jack and Jill* case, but now analyzed in terms of S, S–R triads, not S–R duals.

These fundamentals are introduced in Part I of the book, with ample reference to the history of language research. It closes with stating the dual task ahead of the student of language: "The psychological student of grammar not only studies the individual and social psychological polarization of speech – this is his analytic task – but he also has an interpretative problem – namely, he wants to discover in terms of psychological principles the nature of grammatical phenomena." (p. 96). This is hardly a revolutionary view, analyzing the observed grammatical phenomena and providing a theoretical interpretation for them in terms of psychological principles. That had been precisely Wundt's program. The principles, of course, had drastically changed, but the grand purpose was the same. Notice how different Kantor's perspective is from Bloomfield's, who had eradicated all psychological theorizing from his linguistics.

Part II involves, for the four conventional domains of linguistics (semantics, syntax, morphology, and phonology), a critical evaluation of the traditional notions and a restatement in terms of the new, adjustmental approach. Kantor's approach is to review, for each domain, some of the traditional topics and then to reframe them in the new perspective. A few examples must suffice. As an issue in semantics, Kantor discusses Jespersen's treatment of past tense. What meanings or notions are expressed, and what functions are performed by the different forms such as "How did you know I *was* a Dane?" or "Men *were* deceivers ever"? Kantor presents Jespersen's table of forms/functions/notions and immediately concludes: "Why not substitute for all this machinery a set of statements as to how persons actually adapt themselves to their adjustment stimuli? . . . According to psychological grammar the function of a linguistic action is what it does, and so we find that a speech action bears a relation to its adjustment stimulus that is only external, accidental, and momentary." (p. 126). This is, of course, no more than word magic, trite behaviorese. Here is a syntactic example. It concerns the notions of subject, predicate, and object. Kantor treats a number of well-known cases where these notions create problems. The ultimate case is this: "the plot thickens when we consider the absence of a subject. . . . Certainly in a one-word sentence we have difficulty in finding subject and predicate. We must conclude, then, that it is quite a futile performance to analyze sentences into subject and object." Can we do better? The simple solution follows immediately: "To correct this situation we must turn from the artificial organization of words to the study of reference adjustments to stimuli. In general then, we classify references on the basis of the particular pattern and the conditions influencing that pattern." (p. 138). These examples are characteristic of how Kantor frames traditional linguistic notions in terms of behaviorese newspeak.

The same holds for Part III, which is a treatment of parts of speech. Here is the new approach: "The objective psychologist makes his reinterpretation of speech parts in terms

of three variables of a speech adjustment: (1) the response, (2) the adjustment stimulus, and (3) the auxiliary stimulus. To this we add, of course, (4) the characteristics of the speaker and (5) the human circumstances which always have a significant influence upon interactions." (p. 184). Notice that the three-factor model is casually expanded to a five-factor one. What does it bring? Kantor reviews the traditional treatments of person, gender, case, tense, number, voice, mood, and negation, showing that they are all useless. Then he frames them in the new adjustmental perspective. For example, after a critical review of the mood literature, Kantor draws two conclusions: "First, we may decide that mood is nothing but an accidental and variable style of particular languages." and "Secondly, we may conclude that there is really no way of separating mood out as a distinctive linguistic phenomenon." So, how should one deal with mood? "When we study speech types as psychological adjustments we are at once enabled to evaluate grammatical moods. For objective psychology, moods are nothing but particular response-patterns of speech-community styles of utterance." (pp. 289–290). And so it goes *mutatis mutandis* for all other topics.

Kantor's "objective" approach abhors explanatory theory. Traditional explanations are mentalistic and treat responses as "things," such as "words," "cases," "tenses"; hence they are useless. They are to be replaced by the oft-repeated mantra: linguistic phenomena are "merely" adjustments to stimuli. There is no further theoretical development "in terms of psychological principles" of any kind. One would, however, hope that at least new directions would be sketched for the analytic task. How are linguistic behaviors going to be scientifically observed, collected, analyzed? That hope is in vain too. Nowhere is there any indication of how linguistic behaviors could be empirically approached. Time and again we read that we should study "the drama of speech – what really goes on when persons speak," "living language," that we should analyze "actual speech adjustments," "the actual interaction in which the person is involved" and that we should "learn to observe the speaker interacting with his stimulus objects in a free referential way." (pp. 128, 148, 149, 260, 314). Such statements foreshadow the conversation analytical approach introduced by Sacks, Schegloff, and Jefferson (1974), including the transcription script they cultivated (Kantor writes *gimme* and *didja* for "give me" and "did you," and approvingly cites *wimsyae* and *wimmzel* for "oui monsieur" and "oui mademoiselle" (pp. 151, 154), but without framing a systematic approach).

Although in this book we do not follow the developments of the "cognitive revolution" of the 1950s onwards, it should be noticed that Kantor did not stop. His *Psychological linguistics* of 1977 is a further, detailed development of *An objective psychology of grammar.* Kantor now prefers the term "interbehaviorism" to stress the usual interactiveness of behavioral "fields," which is most clearly the case in games and linguistic interactions. It is probably also a way of distinguishing his own approach from Skinner's more traditional S–R behaviorism, to which he makes no reference. Kantor dedicated his last major work in the psychology of language to Edward Sapir, "eminent scholar, noble man." In his Preface he cites a 1926 work of Sapir, in which Kantor is praised for his "notable contribution to the understanding of language as a particular type of behavior." What would Sapir have thought of this?

Fig. 8.4. B.F. Skinner. © Getty Images.

Burrhus Frederic Skinner

Of all American behaviorists, Skinner has been by far the most influential, both theoretically and experimentally. From our perspective, the psychology of language, Skinner's contribution has been largely theoretical, culminating in his book *Verbal behavior* (1957), which is the major topic of this section.

Burrhus[22] Frederic Skinner (1904–1990, Fig. 8.4) was born in Susquehanna, Pennsylvania, where his father ran a law office. Wanting to become a writer and poet, Skinner went for a BA in English at Hamilton College. But he then changed his mind and decided to study psychology. He went to Harvard, where he received his PhD degree in 1931, with a thesis on the concept of the reflex and on his own comparative work in that area. As a post-doctural student at Harvard he extended his work on Pavlovian conditioning, increasingly attending to the effect of reinforcing contingencies, i.e., of reinforcing specific types of behavior. He also began working on what would become, 23 years later, his *Verbal behavior,* triggered by an after-dinner discussion with philosopher Alfred Whitehead.[23] In 1936 he accepted a position at the University of Minnesota. There he published his two empirical studies in the psychology of language, one on the perception of speech-like signals and another on word associations. After the war years, in 1946, Skinner became chairman of the psychology department at Indiana University, at Kantor's instigation. In his autobiography,[24] Skinner writes: "Another behaviorist whose friendship I have valued is J.R. Kantor. In many discussions with him at Indiana I profited from his extraordinary scholarship. He convinced me that I had not wholly exorcised all

[22] Burrhus was his mother's family name.

[23] See Skinner's own account in *Verbal behavior*, pp. 456–460.

[24] Skinner (1970), p. 19.

the 'spooks' in my thinking." In 1947 Skinner presented the William James Lectures at Harvard on verbal behavior; by then a draft of his book was largely completed. At Harvard he received the offer to become a member of the department. He moved to Harvard the next year, staying there for the rest of his life. The publication of *Verbal behavior* was delayed because Skinner was writing another book, *Science and human behavior*, and his research team were working on the operant conditioning of pigeons and rats. *Verbal behavior* finally appeared in 1957.

Which "spooks" had not been exorcised in Skinner's mind? For him behavior comprised anything a person does, including hard to observe private events such as thinking. What the behaviorist should *not* do is use such activities as *explanations* of behavior ("John bought the car because he thought it was cheap"). Also "silent" language activities are real behavior, not to be reduced to subliminal articulatory movements. Skinner also rejected the other, Meyer–Weiss type of reductionism, which regards brain processes as the immediate causes of behavior. Skinner fully accepted the existence of a biological, genetically transmitted endowment in organisms, but saw the essential task of the psychologist as studying behavior as a function of environmental conditions. The behavioral response or "operant" is the dependent variable. The psychologist studies its dependency on environmental variables, which cause the behavior. Most important here is the "schedule of reinforcement" which the environment exerts on operants. Certain behaviors are "reinforced" by providing or withdrawing particular stimuli (such as providing food or stopping an electric shock). In animal work, the dependent variable is the "strength" of the response, i.e., its probability of occurrence. The pigeon can be trained, by reinforcement, to respond to a particular stimulus by hopping an "8" pattern. The strength of this response is the probability that the hopping pattern is executed on the appearance of the conditional stimulus. The psychologist is successful to the degree that such "operant" behavior can be controlled and predicted. This research program can be executed without considering any "mediating" variables (such as brain processes). It is enough to consider animals or humans as "black boxes," or "empty organisms."

It is one thing to work in this framework when dealing with rats or pigeons in experimental Skinner boxes, but quite another to apply it to what is probably the most complex of all behaviors, language behavior. And indeed, like Kantor, Skinner never developed an empirical research program in language behavior. Instead, *Verbal behavior* tries to apply the theoretical framework of operant conditioning to the phenomena of language. This is somewhat like squaring the circle, an enormous scaling up of quite elementary principles. How far can these principles take you when you consider the enormous variety of verbal "responses" people produce or when you consider the speaker's internal "editing" activities? That, of course, you cannot know without doing the appropriate empirical research. Hardly any such research is mentioned in the book. The book's aim was clearly different but no less respectable in principle, namely to sketch the ways in which the operant framework might be applicable in this complex behavioral domain. "It has not been my purpose to present the facts of verbal behavior as such . . . , I have been trying to get the reader to behave verbally as I have." The reader is, as it were, invited to join in, to share in

"a better way of talking about verbal behavior." (pp. 455–456). This is an invitation to acquire behaviorese. Skinner explicitly talks of "translation," and as an example he translates a quote from Jespersen. Here is Jespersen's text:

> In many countries it has been observed that very early a child uses a long *m* (without a vowel) as a sign that it wants something, but we can hardly be right in supposing that the sound is originally meant by children in this sense. They do not use it consciously until they see that grown-up people, on hearing the sound, come up and find out what the child wants.

Here is Skinner's translation of it:

> It has been observed that very early a child emits the sound *m* in certain states of deprivation or aversive stimulation, but we can hardly be right in calling the response verbal at this stage. It is conditioned as a verbal operant only when people, upon hearing the sound, come up and supply appropriate reinforcement.

Skinner lures the reader by replacing or translating traditional linguistic notions, such as "intention" or "referent," by "objective" notions from the animal laboratory, such as "stimulus," "control," "reinforcement" and the like. His rhetorical arsenal is largely phenomenological. He is an able observer of verbal behavior, in particular of verbal behavior as described or commented on by novelists and poets. Where Wundt cited texts by Goethe and Schiller to exemplify his arguments, Skinner draws his wealth of materials from Shakespeare, Boswell, De Foe, Poe, Trollope, Prescott, Proust, Chesterfield, Dickinson, Keats, Swinburne, Eliot, and many others. He goes for examples the reader will easily recognize and appreciate. In many cases Skinner argues just like a linguist, with intuitions about the conditions under which particular utterances ("verbal *operants*") might be appropriate ("be emitted"). Such an intuitive, observational base had been lacking in Kantor's book, which was limited to traditional cases and examples. The obvious drawback of this intuitive approach is that the critical reader will try and provide a similar wealth of intuitive counter examples. That is, in a way, what Chomsky (1959) did in his famous review of Skinner's book. Here, however, we will join Skinner in his characteristic approach to language.

After sketching his program, the functional analysis of verbal behavior in terms of its "controlling variables," as opposed to the traditional mentalistic approach which appeals to the "expression of ideas," Skinner introduces five major types of verbal "operants," i.e., reaction types, to be considered. The first one is the "mand." The verbal response *Bread!* can be a mand, but whether it is or not depends on its controlling variables. The mand is a response which works for the benefit of the speaker. The speaker's hunger controls (strengthens) the response *Bread!* It is a stimulus for the listener, which may control (make probable) the listener's behavior of bread providing. As we will see, *Bread!* can also be a quite different operant; it all depends on the controlling conditions. Many verbal behaviors are of the mand type. There is even a category of "magical mands," such as *May you suffer the torments of Job.* In many cases the mand describes its own reinforcer. Getting bread will reinforce the probability of saying *Bread!* when you are hungry. To know whether an operant is a mand, you must know what it is controlled by. That is, largely, your state of deprivation. How long have you been without bread? If it is a long time, your operant *Bread!* is probably a mand.

A further category of operant types consists of responses that are under the control of some other verbal stimulus. Three types can be distinguished here. One is the "echoic" operant. Echoic behavior is essential for the acquisition of verbal operants. The child "imitates" the adult, initially far from perfectly, but the response is reinforced. By reinforcing ever better approximations to the adult's example, the child eventually finds the "right sound." This is called "shaping." But there are many other forms of echoic behavior, such as self-echoing in pathological, repetitive speech. Another case in this second category is "textual" behavior, in particular reading. The visual text stimulus "controls" the speaker's behavior. And, finally, there is the "intraverbal" operant case. There is no similarity now between verbal stimulus and response. Producing word associations, such as *doctor–nurse* is a case of intraverbal operant behavior. Another case is hearing *two plus two* and then responding *four.* This is, however, only the beginning. When you write an extensive answer to an exam question, you are producing an intraverbal operant. It is "under the control" of the question, whatever that means.

The fifth major type of operant is the "tact." Here the operant is not controlled by a verbal stimulus but, by "the whole of the physical environment – the world of things and events which the speaker is said to 'talk about'" (p. 81). A simple case is where you name an object. Traditionally such an operant is called a sign or symbol. The tact is controlled by the stimulus, for instance the view of the object. It is the "reference" of the tact. The child is reinforced in some way or another if it produces the correct tact in view of a particular stimulus object, such as a doll. An important role is played by "generalized reinforcers," such as "good" or a smile. Such generalized reinforcers can be used to increase the strength or probability of any operant fragment. Skinner happily refers to Greenspoon's (1955) experiment in which the speakers' use of plurals is reinforced by slight signs of approval on the part of the experimenter. This resulted in an increased probability of using plural forms. Tacts can be highly abstract, being controlled by a quite specific stimulus property. The "shaping" of such specific responses is no different to training a pigeon to pick at anything of a particular shape, a particular size, or a particular color. The "stimulus control" (behaviorese for "referent") of an abstract tact such as *Dutch* or *Mozart* is extremely subtle. Such abstract stimulus control is also involved in "generic extensions," such as metaphor. In addition the control can be exerted by a "private" stimulus, for instance for a tact operant such as *my tooth aches.* How are such private tacts reinforced? Skinner lists various roundabout ways that might be at work. One is that the conditions were originally not entirely "private." A child may have been crying and grabbing her cheek, which are "public accompaniments" of the private stimulus. When operant fragments such as *tooth* and *ache* happen to be uttered under these partly public conditions, these fragments may get reinforced by the environment. At later stages such accompaniments disappear. The tact can also itself be private, as in self-talk, as will be discussed.

The five types of verbal operant, mand, echoic, textual, intraverbal, and tact, are exclusively defined by their "controlling variables." We have seen that *Bread!* can be a mand, but it can also be echoic as in saying *bread* when you hear *bread*, textual as in reading BREAD as *bread*, intraverbal as in translating spoken "pain" as *bread* or it can be a tact, as in refer-

ring to the appropriate object with *bread.* Regrettably, it is not obvious what the precise "controlling variables" are for the various types of verbal operant. They are somewhere in "the whole of the physical environment" or even the "private" environment. They are, it seems, what people are talking about.

A final, major type of control is exerted by what Skinner calls the "audience." In many cases it is some audience, present or absent, which takes on the role of reinforcer and discriminative stimulus. Skinner discusses many more or less subtle ways in which such control is achieved. Adults may punish a child when using terms of abuse. Attending a concert will increase the probability of whispering or stopping speech, and so on.

In a subsequent part of the book, Skinner argues that verbal operants are usually multiply caused. Only rarely are there "pure" cases like the ones just discussed. The operant *Believe me, it's true,* for example, is both a mand and a tact. The mand is for the listener to react "with greater belief" to the tact *it's true.* There can also be multiple causation within a particular type of operant. The same term of abuse may be under double control; negative reinforcement by parents and positive reinforcement by peers. It is often the case that fragments of a longer verbal response are each under the control of different conditions. This is, according to Skinner, a major cause of syntactic grouping and ordering, as we will presently see. Multiple fragment-by-fragment control is particularly apparent in the domain of speech errors and verbal wit. Skinner spends a chapter reviewing lapses such as blends and "malaprops."[25] Word blends (as *snangle* for *snarl* and *tangle*) or phrasal blends (such as *that's what I think so* from *that's what I think* and *I think so*) can result when two operants have the same strength; they become simultaneously emitted. Skinner provides appropriate examples, but is apparently totally unaware of Meringer and Mayer's work. He makes reference to just two sources in the literature. One is Freud's, but Skinner does not take it very seriously because Freud only reports highly selective observations. "A careful study of large samples of recorded speech would be necessary to determine the relative frequency of different types of fragmentary recombination." (p. 294). That is exactly what Meringer and Mayer did. But it is completely ignored by Skinner, in spite of the fact that F. L. Wells (1906), Skinner's other source on lapses, refers to it in various places. In terms of mechanism, Skinner has little more to offer than degrees of response strength. That is, of course, something. It is essentially the same notion as "activation level" in modern psycholinguistics. But there is no inkling of Meringer and Mayer's ordering machinery, which, in the occasional lapse, drops units into unintended but equivalent positions. There is no systematic treatment or explanation of anticipations, perseverations, and exchanges, no review of the units that can be involved in ordering errors. Here, the limits of Skinner's "black box" approach become apparent. There is no place in this framework for any intervening mechanism.

Skinner's treatment of grammar and syntax combines the notion of multiple causation (successive fragments have different causes) with the concept of "autoclitic." An autoclitic

[25] I am not aware of an earlier technical use of this term. Fay and Cutler (1977) introduced the term "malapropism" as a, now standard, technical term in speech error research.

is a verbal operant in which one fragment modifies the functions of another fragment. When you say *I think it's raining*, the fragment *I think* modifies the function of the tact *it's raining*. The autoclitic modifies the effect the tact will have on the listener, i.e., on his response to it. It tells the listener "the sources of my tact are not entirely reliable." Autoclitics occur all over the place in what are traditionally called "propositional attitudes" (*I believe, I wish, I agree,* etc.), but also more broadly. Quantifiers, for instance, modify the effects of following tacts (*some people*). *Only X if Y* modifies the effect of *X* on the listener, and so on. Ultimately, the autoclitic becomes Skinner's jack-of-all-trades. Any "functional" relation between verbal operants, whether phonemes, morphemes, words, or phrases, is an autoclitic relation: one operant (the autoclitic) working on another. In the utterance *man is hungry*, the "assertive" autoclitic *is* modifies the effects of the operants *man* and *hungry*. Such autoclitic activity creates "the larger segments of verbal behavior" that "are usually called sentences" (p. 345), according to Skinner.

What determines the syntactic ordering of fragments? Several causes are involved here, according to Skinner. First, the ordering of fragments tends to follow the order of their relevant stimuli; that is by and large the case for the sports reporter. We now call this the "principle of natural order."[26] Second, the speaker's own intraverbal stimulation can cause a particular ordering, such as in recitation of a poem or in the free flow of associative thought. Third, the relative strengths of response fragments causes specific orders: stronger fragments take precedence over weaker ones. These are relevant observations, but it was Osgood rather than Skinner who devised an experimental approach to such ordering factors.[27]

Syntactic agreement is also an autoclitic function: "In the response *the boys run*, the *s* . . . also serves as a relational autoclitic in its "agreement" with the form of the verb." (p. 333). This is, like many other of Skinner's analyses, nothing other than behaviorese for a well-established linguistic fact. The autoclitic of order also "carries a heavy burden," according to Skinner. *The boy runs the store* is not reinforced by the same situation as *The store runs the boy*—behaviorese for "they differ in meaning." Skinner is well aware of the speaker's linearization problem.[28] "Vocal verbal behavior has only one important dimension: time. Within this dimension the speaker must describe multidimensional scenes and episodes and present complex arguments." (p. 353). But then, he has little more to offer than listing "manipulatory" autoclitics that deal with the problem, such as *but first*, or *parenthetically*.

What is it that is being ordered? The traditional notion of parts of speech (nouns, verbs, etc.) is not very useful according to Skinner. Sentence fragments are often larger than these parts of speech. We have so often produced operants such as *the boy's shoe, the boy's gun, the boy's hat*, etc. that the whole frame the *boy's* — has some noticeable "strength."

[26] Levelt (1989, p. 138). See especially Kuiper's (1995) work on auctioneer talk and sports reporting.

[27] Osgood (1971), soon joined by Bock (Osgood and Bock 1977), started a prolific experimental program studying ordering-relevant factors such as humanness, figure-ground, given-new, etc.

[28] For "the speaker's linearization problem" see Levelt (1982).

Further "specific features of the situation will then strengthen the responses fitted into it." (p. 336). This sends a salute to Ach, Bühler, and Selz who introduced the frame notion. But Skinner shows no awareness of this heritage.

Skinner further discusses such "skeletal" responding by means of characteristic speech errors. In *the wages of sin is death*, for instance, the "intraverbal" connection between *sin* and *is* overcomes "the more remote relation between *wages* and *are*." (p. 339). It would not be long before a whole research program devoted to such agreement errors took off.[29] But better notions of mental mechanics were needed, such as "spreading activation" dynamics.

Verbal behavior is written from the perspective of the speaker. It deals with the environmental conditions that "control" the "probabilities" of verbal responses, i.e., speaker's behaviors. The final part of the book examines the speaker's self-control, in particular the speaker as a self-editor. Skinner's basic position is this: "A speaker usually rejects a response because it has been punished" (p. 371). The audience may punish "objectionable" behavior of the speaker, such as speaking when silence is appropriate, speaking in too loud a voice, using vulgar expressions, lying, and exaggerating. After punishment, such responses decrease in frequency: "we undergo a reaction of fear and guilt." (p. 372). Skinner's approach to self-editing is almost psychoanalytic. "Most Freudian slips involve a failure to see the controlling variable rather than the behavior itself." (p. 386).

Speakers use "autoclitics of editing" when retracting from a despicable utterance, saying "pardon" or getting into an "autoclitic giggle." The process of self-editing involves the speaker reacting "as a listener to his own behavior." (p. 384). This fails in Freudian slips.

Self-editing is largely about "weakening" verbal operants. But speakers are also concerned with strengthening their operants. In many situations stimulus control is too weak to produce the appropriate response. You may have forgotten a name or have difficulty ordering a dinner course in your second language. You will then resort to special ways of strengthening your response, for example using a magnifying glass to have a better look at the object you cannot name or using a "self-prompt" by going down a list of names. And you can prompt your own verbal behavior by looking into a crystal ball or some other source of vague visual stimuli. Good self-prompts are, of course, written notes which strengthen your textual response. Because strengthening of a response can only occur through reinforcement, these internal actions must be reinforcing. It is unclear to the reader why that might be so, but apparently it is obvious to Skinner.

Finally, Skinner turns to the topic of thinking. There is a hierarchy of verbal response strength, going from shouting to loud and quiet talking, whispering, muttering, and subaudible speech. It makes no sense to single out one part of this dimension (subaudible speech) as thinking. Thinking is behavior just like any other kind of behavior and it can be overt, as in the self-talk we can produce aloud in the absence of an audience. And it need not be verbal. To understand human thinking we must understand its "controlling variables" (p. 449). There is nothing to look for in the "black box." "Shakespeare's thought

[29] Bock and Miller (1991).

was his behavior *with respect to his extremely complex environment*" and "the thought of Julius Caesar was simply the sum total of his responses to the complex world in which he lived." (pp. 450, 452). It would be great progress indeed to have "objective" access to the controlling variables that cause these complex verbal behaviors.

In an appendix Skinner likens his position to the Copernican and Darwinian revolutions. Radical behaviorism is the "crowning blow" to anthropocentrism. The control of action is in the environment. "The program of radical behaviorism left no originating control inside the skin." (pp. 458–459).

However, Skinner's empty organism approach forbids considering any mediating mechanisms, whether mental or physiological or computational. It is an approach quite foreign to science, which was, even in Skinner's time, a highly theory-driven, deductive, hypothesis-testing enterprise. It is for that reason in particular that *Verbal behavior*, just like Kantor's *An objective psychology of grammar*, provided neither a challenge nor direction for an experimental, theory-guided approach to human language behavior. No trace was left of James McKeen Cattell's promising beginnings.

Not all behaviorists, however, eschewed mediating mechanisms in dealing with language behavior. The following section will consider the "mediation theories" of Cofer, Mowrer, and especially Osgood. They stand in what was later coined the "neobehaviorist" school of thought, going back to Clark Hull (1884–1952) at Yale University, Edwin Guthrie (1886–1959) at the University of Washington, and Kenneth Spence (1907–1967) at the University of Iowa. Their approach was to some extent theory driven and deductive, and provided theoretical but testable mechanisms mediating in the processes of learning and motivation. When this was applied to the mechanisms of language and meaning, the windows were set ajar for ever more "mental" mechanisms, which led to the eventual undermining of the foundations of behavioristic psycholinguistics.

Mediation theory

Semantic conditioning

Subjects are watching a screen onto which the four words *style*, *urn*, *freeze*, and *surf* are flashed. The words appear 15 times, in random order. Meanwhile the subjects are sucking at lollipops, chewing gum, or eating small tea sandwiches. After three minutes the nibbling session is stopped. This whole procedure could be repeated once or twice. In a new experimental session the same subjects get a dental cotton roll under their tongues for a period of one minute, during which they see a test word on the screen. The cotton ball is removed and weighed. How much saliva did it absorb? That depends on the test word. Suppose the amount of saliva obtained with any of the four words *style*, *urn*, *freeze*, and *surf* on the screen was 100%. When the test word was a synonym, *fashion*, *vase*, *chill*, *wave*, the average amount of saliva was 59%. When it was a homonym, *stile*, *earn*, *frieze*, *serf*, it was a mere 37%.[30] The conditioned salivation response apparently generalizes substantially to a meaning-related word and to a lesser degree to a form-related word.

[30] This was all corrected for "control salivation."

This Pavlovian conditioning experiment by Razran (1939), a Russian at Columbia University, is just one in a flurry of Russian experiments on generalization of the conditioned response, in particular semantic generalization.[31] It had been known for a long time that a conditioned response to a stimulus (such as Pavlov's bell) generalizes to a perceptually similar stimulus (a bell with a different pitch). The more dissimilar, the less generalization: this is the "stimulus gradient." Razran's homophones, visually and phonetically similar to the conditioned stimuli, show this generalization effect. In an immediate follow-up Riess (1940) demonstrated the same phonological and semantic conditioning effects for the galvanic skin response, certainly an easier response to measure than salivation. But how does semantic generalization come about? How does a meaning-related word, such as a synonym, show an even stronger generalization effect than a sound-related word?[32] In Pavlovian terms, these semantic generalizations are due to the *second signaling system.* This is the language system, whose properties are quite different from the first signaling system, which is governed by classical conditioning. This second signaling system makes it possible for us to follow verbal instructions. It takes much effort, for instance, to train an animal to respond to green triangles as opposed to other geometrical objects on a screen. But the second, verbal signaling system shows "abruptness." It is enough to say "Press when you see a green triangle"; no further training is needed. And it is as easy to switch the conditioned response. Just say "Now press when you see a yellow triangle." To retrain the animal would again take much time and effort.[33] We will return to these matters in Chapter 10 on acquisition, which has always been a central topic in Russian psycholinguistics. American behaviorists, however, largely ignored Pavlov's notion of the second signaling system. Cofer and Foley, as we will see, proposed different mechanisms for classical ("physical") stimulus generalization and semantic generalization. Osgood rejected that proposal and adduced all generalization to the same underlying mechanism.

Cofer and Foley's analysis

A *Psychological Review* paper by Cofer and Foley, published in 1942, proposed a theoretical model for Razran's and related semantic generalization effects. No new mechanism was needed for the homophone effect; that was merely the classical stimulus gradient effect. But, following a suggestion by Clark Hull, they added a new mechanism to explain semantic generalization. "Mediated generalization" works as follows. Consider a vase. Over the course of your life you have acquired some overt responses upon seeing a vase. You may grab it or put flowers in it. The stimulus vase, S_v releases some overt response R_v, $S_v \rightarrow R_v$. The word *vase* often appears with the object vase. Still, it does not get

[31] Among them Kapustnik (1930), Kotliarevsky (1935), Nevsky and Levin (1932), Smolenskaja (1935), and Traugott (1935). Razran (1961) provides an excellent review of Soviet research in semantic conditioning. See also Razran (1965).

[32] Later, Riess (1946) demonstrated that 7–9-year-old children showed a stronger effect for homophones than for antonyms and synonyms. This is reversed by the age of 14.

[33] Slobin (1966) provides a detailed review of the Soviet literature on this topic.

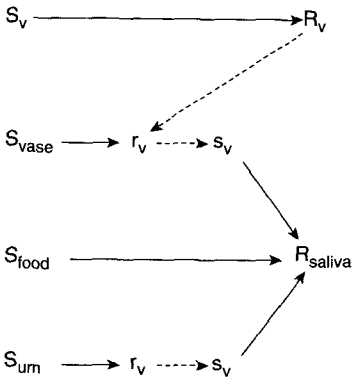


Fig. 8.5. The mediation mechanism of semantic generalization. This figure does not appear in Cofer and Foley's paper, but its ingredients and their relations are all explicitly discussed, in particular the "mediating" $r \rightarrow s$ mechanism.

conditioned to R_v. When you hear the word *vase*, you do not grab in the air. The word may have been conditioned to a fractional, kinesthetic part of R_v, which we can call r_v.[34] This unobservable internal response, a tiny part of R_v, has itself a certain self-stimulating value, called s_v. Hence, when you hear *vase*, S_{vase}, the resulting internal event is $r_v \rightarrow s_v$. This is represented in the top two lines of Figure 8.5. In fact, there is a whole set of such tiny, fractional responses. When you now perform a new Razran-type experiment, conditioning the word *vase* to the saliva response for food, it is the internal s_v that is conditioned to the saliva response. This is represented in line 3 of Figure 8.5. Why does this response generalize to a synonym, such as *urn*? The reason is that some of the fractional responses $r_v \rightarrow s_v$ to *urn* happen to be the same as those to *vase* (for instance the tendency to "put something in"). This is represented in line 4 of Figure 8.5. But if s_v becomes conditioned to R_{saliva}, the stimulus *urn* will also, via the common internal reaction $r_v \rightarrow s_v$, evoke the saliva response. The size of the response will depend on how much of the fractional response to *urn* coincides with the fractional response to *vase*.

Cofer and Foley demonstrate that the same mediation mechanism handles not only synonym type of generalizations, but also other cases observed in experiments and that it made new predictions. Razran, for instance, had found generalization from a word to its translation equivalent in another language (known by the subject). Generalization to associated words (e.g., *doctor—nurse*) was also thought to be due to an overlap of internal fractional responses. And then there are all the "chained" cases. The often observed "physical" generalization to a homophone can be further generalized via the aforementioned type of mediation to a synonym of the homophone, hence from *urn* to *earn* to *money*. Another type of chaining involves synonyms of a homophone, for instance *gate–stile/style–fashion*; and so on for ever longer chains. The authors display a systematic network of such phonetic and semantic word-type relations. To the best of my knowledge this is the first attempt in the literature to illustrate the structure of a lexical network. It is a highly complex mediating structure, a "mental representation" as we would now call it.

[34] The neobehaviorists would have been quite happy with the neuroimaging evidence reported by Mitchell *et al.* (2008).

In subsequent years Cofer and his associates tested and further developed their mediation theory experimentally in a series of studies.[35] The experimental design was, in essence, this: the subject would be visually presented with ten words, one after another. The test list would, for instance, contain words such as *pear*, *right*, or *dead*. Subjects were then asked to recall the words on the list and their performance was scored. Immediately before this recall test, another list of ten words was repeatedly presented to the subject. We would now call this a priming list; Cofer called it the "reinforcement list." That list could contain homonyms of the test list (such as *pair* for test list item *pear*) or synonyms (such as *just* for test word *right*) or an antonym (such as *alive* for test word *dead*). Alternatively, a totally unrelated priming list was used. Cofer found that related prime words usually improved recall performance for the test list. The effect was (very roughly) dependent on the "chain" distance between prime and test word. These were the first systematic "verbal learning" experiments, which would go on to become a cottage industry during the 1950s and 1960s.

It should not be a surprise that Charles Cofer (1916–1998) was to play a "mediating" role in the transition period of the cognitive revolution. He became the organizer of an informal Group for the Study of Verbal Behavior (GSVB), involving most of the American psycholinguists of the 1950s and early 1960s. They met at the fringes of conventions, always with Charles Cofer as the convener (in 1955, 1959, 1961, and 1966).[36] They also established, in 1962, the new *Journal of Verbal Learning and Verbal Behavior*, with Leo Postman as its first editor.

Charles Osgood's theory and measurement of meaning

In Chapter 1 we discussed Osgood's pivotal role in establishing and organizing the "new" psycholinguistics. Together with Sebeok he edited *Psycholinguistics* during 1953, right after the publication of his *Method and theory in experimental psychology.* The previous year Osgood had published his classic paper,"The nature and measurement of meaning." That paper did four things. It first reviewed and evaluated the literature on "measuring" meaning, both experimental approaches, such as Razran's and Cofer's, and statistical approaches, such as word frequency counts and word association norms. They were all deemed unsatisfactory by Osgood. Osgood focused a lot of attention on the experiments of the Cofer team. He correctly noticed a flaw in the design of the experiments, which made them less than conclusive. It is interesting to observe the rapidly increasing technical sophistication of psychological experimentation at this time, which is a lasting contribution of behaviorism to psycholinguistics.

Second, the paper reviewed some dominant theories of meaning. One of them is, of course, the "mentalistic view." Here, Osgood was appreciative of Ogden and Richards' book *The meaning of meaning* (1923), in particular on their claim that the relation

[35] Cofer, Janis, and Rowell (1943), Foley and Cofer (1943), Foley and MacMillan (1943), Foley and Matthews (1943),

[36] See Mandler (2007), pp. 195–203.

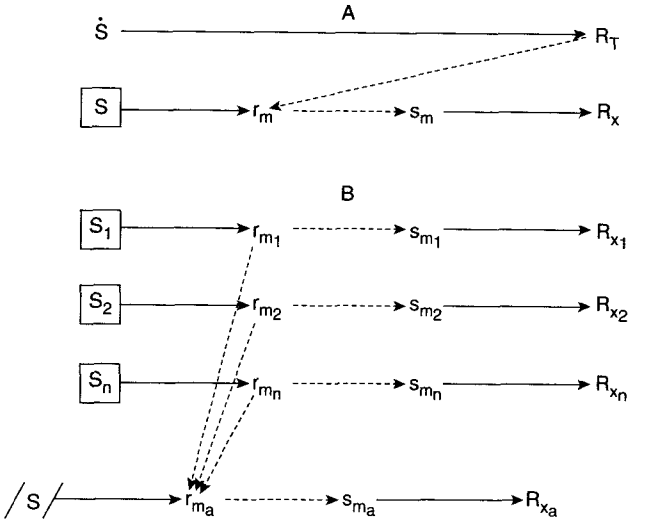


Fig. 8.6. Osgood's mediation account of the *sign* and the *assign* process. From Osgood (1952, p. 205). Reproduced from Osgood, C.E., The nature and measurement of meaning. *Psychological Bulletin*, 49, pp. 197–237 © 1952, American Psychological Association.

between a symbol and its referent is "mediated" through mental thought or interpretation. Another more recent theory also got a positive mention. It was Charles Morris' semiotic "disposition" theory outlined in his article "Foundation of the theory of signs" (1938). Osgood's appreciative summary of Morris's position was "any pattern of stimulation which is not the object becomes a sign of that object if it produces in an organism a 'disposition' to make any of the responses previously elicited by the object." (Osgood 1952, p. 202). But in general none of the theories, including Watsonean behavioristic theories, could meet with Osgood's approval.

Third, Osgood proceeded to develop his own "mediation hypothesis." He depicted it by way of a diagram, which went on to be reproduced time and again. It is shown in Figure 8.6.

The top two lines in Figure 8.6 depict the sign process. The basic "Hullian" notions are similar to Cofer and Foley's theory depicted in Fig. 8.5. The top line depicts the range of overt and covert reactions (R_T) that are, with some probability, elicited by a stimulus object (Ṡ), such as the vase mentioned previously. When some other stimuli that are often associated with the stimulus object (flowers, the word *vase*) occur in isolation, they will elicit some reduced portion of R_T. That is the "mediating reaction" r_m. Mediating reactions that require a lot of energy or that interfere with "goal achievement," i.e., overt portions of R_T, do not condition well; they rapidly extinguish. Only the "light weight," covert, "detachable" ones attain a stable conditioned relation to the associated stimulus. The stimulus [S] which elicits such a stable, lightweight mediated response r_m is a *sign* for Ṡ. The word *vase*, for instance, can become a sign for vase objects. The mediated response r_m is a discriminatory response; the sign (*vase*) is related to the referent, the original stimulus object Ṡ, via

the common properties of r_m and R_T. This relation is the "interpretation" or *semantic* meaning of the sign. In addition, this mediating response provides mediated self-stimulation, s_m. That self-stimulation can, in turn, become conditioned to specific overt behaviors, R_X. When you hear the word *vase*, you may have acquired the response of looking around for a vase. That relation of the sign to a set of overt behaviors R_X can be called the *pragmatic* meaning of the sign. In other words, Osgood conceived of the sign mediation process as a two-step S–R mechanism. The first step is from sign to its discriminatory lightweight "interpretation" response r_m. The second step is from the self-stimulation s_m to the acquired pattern of responses R_X to the sign.

Most of our signs, however, are not acquired by association to some stimulus object. They are rather *assigns*, because they are solely associated to other signs. This is depicted in the bottom part of Figure 8.6. For most children the word *zebra* is acquired as an assign, the /S/ in the bottom line of the diagram. Its meaning is acquired by association to portions of the mediating responses of other, already existing signs (S1, S2, S3). The child is told that zebras have stripes, run like horses, are usually found wild and so on.

Both Hull and Cofer had worked on the assumption that there are two types of generalization. "Physical" generalization (as occurs with homonyms) does not involve mediation; it is pure "stimulus generalization." Semantic generalization, however, always involves mediation. Osgood's more parsimonious claim is that all response generalization is mediated. "Physical" stimulus generalization, for instance from a musical note to its octave, results as much from frequent co-occurrence of the two stimuli as does semantic generalization.

There is, however, a major problem with this mediation account of generalization. According to Osgood, the r_m (he claims) must be *discriminatory* for it to be a sign. That must mean that it is uniquely related to the original total response R_T. It is not enough to just be a part of R_T. If it is a proper part of several Rs, it would very ambiguously refer to all Ŝ's involved. But if r_m is a unique part of R_T, it must differ from all other mediating responses. However, we saw that Osgood's explanation of generalization is based precisely on some *overlap* of mediating responses. They are, in other words, not unique.[37]

The fourth contribution of Osgood's paper concerns his newly invented measurement technique, the "semantic differential." As mentioned, Osgood's judgment of existing methods of measuring meaning was quite negative. There was a need for a general, reliable, widely applicable tool for the measurement of meaning. Osgood's inspiration was synesthesia. Some people experience printed numbers or letters as colored. There is a "shortcut" between one modality and another. Another subject reported pressure sensations about his teeth and cheeks whenever cold spots on his arm were stimulated. We experience a pianist travelling "up" and "down" the scale, feel that hope is "bright" and despair is "black," etc. Such cross-modal meaning relations could probably be used to develop a systematic, standardized tool for the measurement of meaning, thought Osgood. Take any object or concept or piece of art. You can measure its meaning for a

[37] Fodor (1965) used the same uniqueness argument to undermine the formal distinction between single stage S–R theories and mediation theories of meaning.

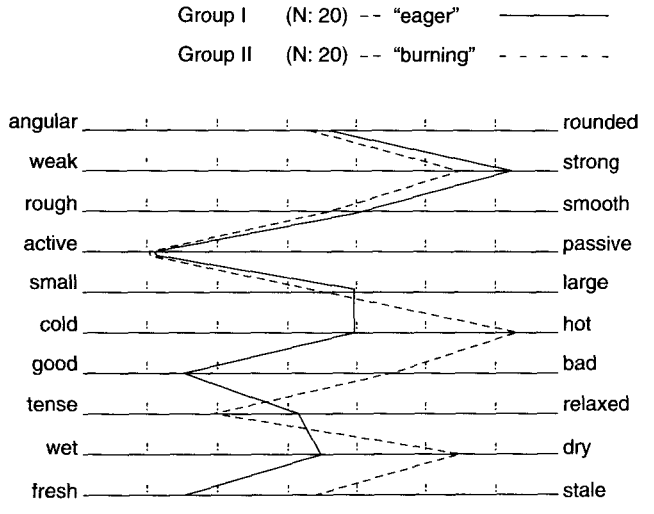


Fig. 8.7. Semantic profile of "eager" and "burning" on the semantic differential. Reproduced from Osgood, C.E., The nature and measurement of meaning. *Psychological Bulletin* (1952), *49*, 197–237. © American Psychological Association.

subject by having it scaled on a set of polar, descriptive scales, such as fair–unfair, kind–cruel, big–small, strong–weak, etc. The pattern of responses is your measurement. Figure 8.7 shows Osgood's measurement of the meaning of "eager" and the meaning of "burning," each scaled by a group of 20 subjects.

The two concepts are clearly similar in meaning, but they also differ in specific ways: "burning" is hotter than "eager," and "eager" is better than "burning." Osgood soon discovered that the various scales he used fell into highly correlated clusters. If something is judged "honest," it will also be judged "good" and "valuable." This invited the application of factor analysis, reducing the variation on 50 different scales to a much smaller number of "underlying" dimensions of meaning. The 1952 paper reports the three dominant factors "evaluation," "strength" (later called "potency"), and "activity." There are few findings in psycholinguistics that have been replicated so often. The semantic differential technique was widely applied in domains such as social perception, advertising, politics, aesthetics, cross-cultural psychology and more. Time and again the three dominant dimensions emerged. The basic "semantic space" turned out to be universal. Of course other dimensions would also emerge, but those were mostly domain-specific.

Osgood was convinced that he had found the Holy Grail: "*A limited number of such continua can be used to define a semantic space within which the meaning of any concept can be specified.*" (his italics). The same conviction is expressed in his 1957 bestseller (together with Suci and Tannenbaum), *The measurement of meaning*. But the various reviews of the book, especially Weinreich's (1958) review, convinced him that what he had been measuring was *affective* meaning, not meaning in general. As we saw in Chapter 6, Osgood recognized the close similarity between his three-dimensional space and Wundt's space of feelings (see Fig. 6.2). Weinreich had argued that these three emotional dimensions

are "an aspect of all experience and behavior."[38] Osgood's retraction was only partial. He recognized that his space did not express a multitude of other semantic features such as ± *human* or ± *sex* (i.e., *male/female*). But he also argued that his three affective features were just like all other bipolar features. In other words, they are real semantic features.[39] In what was no doubt the largest cross-cultural psycholinguistic project of the twentieth century, Osgood and coworkers in many countries convincingly demonstrated that"the three affective factors of meaning are truly pancultural."[40] In later years Osgood began to develop new methods for the empirical determination of (other) semantic features, but these did not catch on in the same way as his differential scales.[41]

Hobart Mowrer: the sentence as conditioning device

In 1949 Hobart Mowrer had lured Charles Osgood to the University of Illinois. They would be close colleagues for many years to come. Mowrer (1907–1982) began his academic life as a student of Max Meyer at the University of Missouri. He designed and distributed the questionnaire on sexual attitudes among students that eventually led to Meyer's dismissal from the university. After receiving his PhD at Johns Hopkins, Mowrer went to Yale University, where Clark Hull was developing his mediation theory. After the war years (largely spent at Harvard), in 1948, Mowrer received his research appointment at the University of Illinois, Urbana.

Mowrer was a man of many talents and interests. He was a clinical psychologist, behavioral therapist, counsellor, neobehaviorist theoretician, and one-time president of the American Psychological Association. In the latter role, he presented his 1954 presidential address on language. The published version of the address, which we will consider here, is Mowrer's main contribution to psycholinguistics.

Mowrer began his paper by noticing that of the 100 American textbooks on psychology, only 27 had any discussion of language. But he also noticed the renewed interest, the now "thriving domain of psycholinguistics." (p. 660). He then introduced mediation theory, largely following Osgood's theory of signs. He suggested applications to children's word acquisition and to the issue of the origins of language. The main original contribution in the paper is Mowrer's conditioning theory of the sentence.[42] "The sentence is, preeminently, a *conditioning device*," Mowrer claimed (p. 665). Using language is not transmitting meanings from person to person, but from sign to sign. A sentence is minimally composed of two units, one functioning as subject, the other as predicate. The predicate

[38] Weinreich (1958, p. 360).

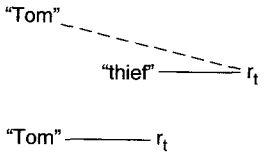
[39] Osgood *et al.* (1975, pp. 404–406).

[40] Ibid. p. 190.

[41] During my 1966/1967 postdoctoral stay at Osgood's Institute of Communications Research, I worked on a method based on the well- or ill-formedness of adjective–noun combinations. Later, my student Leo Noordman wrote a master's thesis on this subject. See Levelt and Noordman (1978).

[42] Later, Skinner (1957, p. 362) took a similar step.

works as the unconditioned stimulus, the subject as the conditioned stimulus. Take the sentence *Tom (is a) thief.* Its mediational structure can be depicted as:



When you hear this sentence, the word *thief* will evoke the mediating reaction r_t. By the contiguity to the word *Tom*, this mediating response, or some part of it, will also associate to *Tom*. In that way, the meaning of the sign *Tom* is changed. Next time you hear the word *Tom*, you will be reminded of thieves. This is what happens in verbal communication; interlocutors change the meanings of signs. Mowrer of course recognized the existence of more complex syntactic structures, such as transitive *Tom robbed the bank.*[43] Here, according to Mowrer, the syntactic conditioning device not only changes the meaning of the subject, *Tom*, but also of the object *bank* (if it was not already known that the bank was robbed).

It is amazing that, among the large audience of American psychologists, Mowrer could get away with this naïve story. If the meaning of the transitive verb gets conditioned to both subject and object, then *Tom robbed the bank* and *the bank robbed Tom* are synonymous. Grammar had been totally lost in the mediation game. In addition, our lexicon is no longer considered to be a stable repository of meaningful words, largely shared in the language community. Every heard or read sentence reconditions the meanings of its words.

Retrospect

Behaviorism was an almost[44] entirely regional but stubborn stance in US psychology and linguistics. Starting with Watson's *Behavior* of 1914, it became for over four decades the dominant, although never exclusive, epistemology in these disciplines. "Objectivism" was its mantra. Everything non-observable was suspect, mental entities in the first place. Even worse, theory became suspect. Mechanisms mediating between observables should be entirely avoided or at least kept to the utter minimum. One consequence was a general reluctance to study complex behavior, language behavior in particular. And if you were brave enough to do so, you were best advised to avoid invoking evanescent meaning as a mediating mechanism.

Still, neobehavioristic mediation theory, with Charles Osgood in the driving seat, ultimately opened the minds of American psychologists to things mental, to meaning, to theorizing about mediating "information processing" mechanisms. It became a jumping board for the "cognitive revolution" of the late 1950s and 1960s. Then it became politically correct again in the United States to adopt William James' definition of psychology

[43] Mowrer happens to call this "intransitive" (p. 688).

[44] There was, of course, the Soviet variant too.

as "the science of mental life." That is what George Miller did in the title of his 1962 book, a text which was a major effort to reconnect psychology to its mentalistic, mostly European, roots. And indeed, behaviorism had effectively eclipsed essential classical notions of mental functioning, such as Wundt's attentional mechanisms, Selz's mental operations, and Meringer's sequential ordering principles. They were soon to be revitalized in the United States. But one heritage of (neo)behaviorism was lasting: rigorous experimental design. Here, American psycholinguistics had clearly gained the upper hand, setting methodological standards that were rarely met in European circles.

Chapter 9

Speech acts and functions

In 1922 the famous British Egyptologist Alan Gardiner (1879–1963) published a paper in the *British Journal of Psychology* in order to address psychologists on their treatment of the sentence. Both Wundt and Paul had defined the sentence as an expression of representations in the mind of the speaker. "But why do we go about troubling our neighbours with our units of thought?" Gardiner asks. "Why do we inflict our 'complete thoughts' or 'meanings' upon them? Ought we not to be content with mere thinking, instead of launching our thoughts upon the air?" And "somehow or other the dynamics of the situation is neglected in such definitions." (p. 353). The situation in psychology had not improved since Wundt and Paul; "a few recent excursions into psychological literature suggests that here too there is more than an inclination to identify speech completely with thought, and so to ignore . . . the attitude to a listener which speech essentially implies." (p. 354). Here Gardiner refers to the recent exchanges with Watson in the same journal. All participants ignored "the proximity of the listener" as crucial stimulus "when the language-habit comes into play. . . . Would professor Watson's infant run all over the house crying 'box' unless there were either actually or potentially present some person accustomed to act upon the suggestion thus given?" (p. 354). (Gardiner just missed Kantor's 1922 paper, which precisely introduced the listener as a crucial stimulus for language "adjustments").

How to improve on this situation? Gardiner begins by making reference to a paper he "had been unable to obtain," "by a scholar whose name I do not remember." But then, just before the final version of Gardiner's paper went to press, a 1918 article by Karl Bühler turned up and was discussed in a postscript.[1] Bühler stated that speech has three functions: "self-expression" (*Kundgabe*), "demand for response" (*Auslösung*), and "description" (*Darstellung*). These functions derive from the purpose (*Zweck*) of the speech act. This is also the essence of Gardiner's position. The principal sentence types fulfill these functions to different degrees. Exclamations are dominantly self-expressive, statements are largely descriptive, questions and imperatives are demands for response. These are different attitudes of the speaker towards the listener. Gardiner correspondingly defines the sentence as "an articulate sound symbol in its aspect of embodying some volitional attitude of the speaker towards the listener." (p. 355). Sentences are "social facts." Speech is "perhaps the most vital of social phenomena." "*Language is the name given to a system of articulate symbols having reference to the facts of experience, whereby speakers*

[1] K. Bühler (1918b).

seek to influence the minds of listeners in given directions." (p. 354). Gardiner mentions, in addition to Bühler, earlier exponents of this view, in particular Marty.[2] But as critically relevant to his position he refers to Wegener's work.[3] Not mentioned by Gardiner, or by Bühler for that matter, were the American functionalists, particularly the school of John Dewey (1859–1952), who first taught at Chicago and later at Columbia. Here is a quotation from Dewey (1910): "The primary motive of language is to influence (through the expression of desire, emotion, and thought) the activity of others; its secondary use is to enter into more intimate sociable relations with them; its employment as a conscious vehicle of thought and knowledge is a tertiary, and relatively late, formation." (p. 179). Here is another one, written 12 years later, in a more behavioristic jargon: "Often and primarily the auditor [of speech] is another organism whose behavior is required to complete the [speaker's] speech reaction, this behavior being the objective aimed at in the speech reaction." (Dewey 1922, p. 565). We will, like Gardiner, ignore this brand of functionalism in the present chapter, because it barely contributed to the psychology of language.[4]

Both Gardiner and Bühler went on to develop their initial papers on the functions of speech into full-blown monographs. Eventually they met and discussed their commonalities and differences before the appearance of Gardiner's *The theory of speech and language* (1932) and Bühler's *Sprachtheorie* (1934). This chapter will review the somewhat fragmented history of ideas on language-in-use, as it developed from Wegener (1885) to Gardiner, Bühler, and the Prague School. These ideas developed very much on the margins of structuralism and outside mainstream psychology. They were also exclusively European developments, which were abruptly truncated by World War II.

Philip Wegener and Adolf Reinach, the pioneers

Wegener's major publication preceded Wundt's *Die Sprache* by 15 years and Wundt must have been familiar with it, because he marginally refers to it in his chapter on syntax. But Wundt ignored the essence of Wegener's perspective. For Wegener speech is by nature dialogical: "The purpose of our speaking is always to influence the will or knowledge of a person in such a way as seems useful for the speaker."[5] Words are, in the first instance, not learned as "sound forms with content," but as means for achieving particular ends (p. 72). There are two classes of ends that speakers intend to achieve. They try to induce the hearer's sympathy or interest in their own states or value judgments. Or they intend

[2] Gardiner (1922, p. 354). The reference is to Marty (1908, p. 22), where he writes "This influencing of another's mental life is the primary purpose of speech." ("Diese Beeinflussung des fremden psychischen Lebens ist die primäre Absicht des Sprechens").

[3] Wegener (1885, 1920).

[4] This also holds for its sequel in the work by Morris (1938, 1946). The "pragmatics" part of his "semiotic" is irrelevant to speech act theory.

[5] "Der Zweck unseres Sprechens ist stets der, den Willen oder Erkentniss einer Person so zu beeinflussen, wie es dem Sprechenden als wertvoll erscheint." (Wegener 1885, p. 67).

to show their sympathy for or interest in the states or value judgments of the hearer. Where Wundt only considered the speaker, Wegener focused equally on the listener. Speech understanding is first and foremost recognizing the speaker's intention in using verbal means. The listener "presupposes that human beings only execute particular actions in order to achieve something . . . We presuppose in the speaker the same psychological processes and the same psychological organism as in ourselves, and the sympathetic mood is precisely the affective state we enter by interpreting somebody else's state after the model of our own."[6]

This is Wegener's "theory-of-mind" approach to speech understanding. It is no different from what Degérando had formulated even more succinctly over half a century before and which we cited and discussed in Chapter 4. Wegener independently gained the same insight, but in addition considered a plethora of means by which speakers can influence their interlocutors' will. In all cases the hearer is supposed to be able to derive the speaker's intention from what is said. There are straightforward cases such as imperatives and questions, but much is achieved indirectly. The merchant, asked if he carries a particular commodity, will normally infer that the questioner intends to buy it. Still, the interpretation depends on the situation. If he is asked the same question by a statistician, he will make a different inference. When the child says *I am thirsty*, this is inviting the sympathetic action of providing a drink. In his posthumous 1920 paper, Wegener reviews actions performed by single words or short phrases. *A coffee* suffices for the waiter to derive the speaker's intention. That is the *function* of the utterance. Dependent on the situation, the child will interpret *nitwit* as an act of chiding. And so on. A word's function and its (etymological) meaning will often be incongruent. In fact, this is mostly true for standard expressions of politeness, such as *good day*. In his book, Wegener extensively argues that we routinely perform such intentional functions by way of largely fixed expressions, whose functions are shared knowledge.

Putting himself in the position of the listener, Wegener exemplifies that almost any declarative sentence requires completion and supplementation by the hearer. What did the speaker intend to express? Here limited verbal cues may suffice, because the hearer shares substantial knowledge with the speaker. The same word *make*, for instance, is interpreted differently in *he makes a trip* and *he makes an error*. We share action schemas which guide our expectations and as speakers we provide cues (such as *but* or *however*) when we intend to deviate from such a schema. Natural order is one such schema. By default, the hearer can presuppose that the order in which the speaker expresses events corresponds to the order in which they occurred. Cause–effect is another schema, as is means–end. Action schemas differ between cultures, which is why it is so hard to understand action sequences in rituals or ceremonies.

[6] "Wir setzen bei dem Sprechenden unbewusst dieselben psychischen Vorgänge und den gleichen psychischen Organismus voraus, wie bei uns selbst, und die sympathische Stimmung ist ja eben der Empfindungszustand, in den uns die Andeutung fremder Zustände nach dem Muster der eigenen versetzt," (Wegener 1885, p. 69).

Wegener applies his dialogical perspective to what we now call the "information structure" of sentences. There he participates in the perennial "subject debate." Every utterance has an implicit dual segmentation. There is the "exposition," the entity or state of affairs talked about, and there is the predicate, the comment made about it. The exposition (or "logical subject"; we would now say "psychological" subject) is often not expressed at all, because it is obvious to the hearer from the shared situation.[7] A final point Wegener makes is that the dialogical use of language has an ethical dimension, precisely because it serves the function of influencing the will of another person.

These ethical, quasi-legal subtleties of speech acts are recognized and extensively treated in modern speech act theory. But a first formulation had already been provided by the philosopher (Husserl student) and lawyer Adolf Reinach (1883–1917) in 1913. A command, Reinach writes, "is an action of the subject to which is essential not only its spontaneity and its intentionality, but also its being directed towards alien subjects and its standing in need of being perceived by those subjects."[8] Here, spontaneity means that "ego" is experienced as initiator of the action. Social acts are defined by Reinach as intended to be perceived. That is true for all of them: "What has been said of commands holds also for requesting, admonishing, questioning, informing, answering, and many other types of act. They are all social acts which are, in their very execution, cast towards an alien subject, in order to pitch into his mind."[9] We now call them "performatives." It is essential to the act that the intention is *registered* by the interlocutor. That is what constitutes its quasi-legal status. Social acts differ in purpose and presupposition. The purpose of an announcement is to inform somebody else about something. Its presupposition is the speaker's belief in what is announced. A command aims to induce some response in the interlocutor. Its presupposition is the speaker's will that the response be executed. A question aims to provoke a social act in the interlocutor, i.e., an answer. Its presupposition is uncertainty on the part of the speaker, and so on. What Reinach calls the presupposition of the act is nowadays known as its "sincerity condition." Here are the beginnings of speech act theory, including the notion of social *commitment* (*Verbindlichkeit*). Regrettably, Reinach's work remained largely unknown to both linguists and psychologists. Even Bühler, a man of wide reading, never cited him. One cause was no doubt Reinach's early death. He died during World War I, as a military volunteer, in Diksmuide, Belgium.

[7] Wundt attacks precisely Wegener's use of "logical" subject; according to Wundt, it only makes sense to use the term "subject" in a purely grammatical sense. Wundt (1900a, Vol. II, p. 260 (footnote)).

[8] Translation from Smith (1988, p. 140). Here is the original text: "Vielmehr ist das Befehlen ein Erlebnis eigener Art, ein Tun des Subjektes, dem neben seiner *Spontaneität*, seiner *Intentionalität* und *Fremdpersonalität* die *Vernehmungsbedürftigkeit* wesentlich ist." (Reinach 1913, p. 707).

[9] "Was hier für den Befehl ausgeführt wurde, gilt auch für das Bitten, Ermahnen, Fragen, Mitteilen, Antworten und noch vieles andere. Sie alle sind soziale Akte, welche von dem, der sie vollzieht, *im Vollzuge selbst* einem anderen zugeworfen werden, um sich in seine Seele einzuhacken." (Reinach 1913, p. 707).

Still, Reinach's insights were not completely lost. In 1937 Hans Lipps (1889–1941), a philosophy professor in Frankfurt, published a paper entitled "Remarks on promising." The paper begins with a reference to Reinach's notion of commitment (*Verbindlichkeit*) in the act of promising, a notion not worked out in detail by Reinach. A promise is not what lawyers take it to be: an expression of will, and even less an engagement or a contract. Lipps defines it first from the hearer's perspective: (i) being informed about the other's intention and (ii) accepting the expressed intention. From the speaker's perspective it is an assurance that one vouches for one's words (the words can be cited in testimony); the addressee "takes the speaker's word" for something. Promising involves a responsibility. The addressee is now entitled to test fulfillment of the given word critically. Lipps adds that the given commitment is not a "moral" one, meaning that it is not ethically derived but an act *sui generis*.

In a subsequent paper, "The binding nature of language,"[10] Lipps (1938) widens the issue to speech acts in general. Each spoken word implicates some commitment. The speaker stands for what his words mean. The addressee "executes" (*vollzieht*) the meaning of the words. Like Reinach, Lipps was a phenomenologist in Husserl's tradition, not a linguist. And like Reinach his fate was to be killed on the German front, in Russia in 1941.

Alan Gardiner: the functions of word and sentence

In 1931, Karl Bühler presented two lectures at University College London, one entitled "Structure of language" and the other "Psychology of speech." This was following an invitation from Gardiner, who had, a decade before, recognized the affinity of his own thinking to Bühler's. Their meeting prompted Gardiner to publish his monograph "without further delay."[11] It became his *Speech and language* of 1932. On p. 4 Gardiner acknowledges that among psychologists "Karl Bühler is the writer on linguistic theory with whose views I find myself most in sympathy. Many of his conclusions, reached along quite different channels from my own, coincide almost completely with those to be expounded in the present book."

The problem linguistic theory has to address is, according to Gardiner: "How does speech work?" And this should be done by considering "concrete, practical examples." Gardiner's method is "to put single acts of speech into their original settings of real life, and thence to discover what processes are employed, what factors involved."(p. 6). This is, as we have seen, almost the program formulated by Kantor (1936). Indeed, Kantor makes repeated, even sympathetic reference to Gardiner's book, at the same time discarding it as utterly mentalistic. For Gardiner the essential factors to consider here are the ones Bühler had referred to in his lectures: the speaker, the listener, the things referred to and the linguistic material. In this view, "speech is a human activity demanding at least two persons possessing a common language and finding themselves in a common situation. The science to which linguistic theory thus ultimately owes allegiance is neither logic

[10] Both papers are now available in Hans Lipps (1958).

[11] Gardiner (1932, p. viii).

nor psychology, but sociology" (p. 7).[12] Here, Gardiner rather pointedly defines the topic of psycholinguistics, but locates it outside psychology. It is, as he puts it himself, "a refutation of the assumption which has given so many books the titles of '*Sprachpsychologie*' and the like. Hardly anywhere is the slightest hint found that the authors know how insufficient psychology is to cover the entire field of linguistics." (p. 104). Gardiner had the *idée fixe* that psychology is *individual* psychology, the study of "subjective states."[13] That should not deter us from considering his "sociological" treatment, which is, in fact, social psychology.

Gardiner dedicated his book to the memory of Philipp Wegener. Indeed, like Wegener, Gardiner's point of departure was the dialogical situation-bound character of speech, speech as a "social act."[14] And like Wegener he focused on the listener rather than on a speaker who "expresses his thoughts." Words are not so much thought expressions of the speaker, but rather hints and clues that are instrumental for the listener to infer the speaker's purpose in the "act of speech." It is sort of a 20-questions game, channeling the listener's guesses toward what is intended. The delivery of such clues is a purposeful activity of the speaker and in that sense an individual act, "the initiative is always his" (p. 64). The "decision to speak invariably assumes a form of *an intention to affect the listener in a particular way*, and it is this intention which ... makes of every genuine act of speech a 'sentence,' not merely the use of words or a word." (p. 74). On this point Gardiner repeats his 1922 definition of the sentence as the "unit of speech," a purposive structure "embodying some volitional attitude of the speaker towards the listener." The sentence is the very act of speech. "The thing meant in the act of speech," i.e., the sentence meaning, is "defined as that which the speaker intends to be understood by the listener." (p. 103). The word, on the other hand, is the "unit of language," an instrument in the tool kit we share for indicating and inferring the intention. Like Wegener, Gardiner also examines the variety of cases where the act of speech is a single word, which is then by definition a sentence.

Following Wegener, Gardiner discusses how repeated performance of a purposeful act of speech can become a habit. That is the *mechanization* of speech. It results in stereotyped formulas such as *Don't mention it!*, in set phrases such as *to hold one's tongue*, or idiomatic uses, such as, in German, combining definite articles with personal names (*die Maria*). Like words, such mechanized phrases perform specific functions in the listener's inferential process. A core process is that of *selective attention.* Upon hearing a word or set phrase in the context of the speech act, the listener is able "to push far into the background all those potential applications of the meaning which are irrelevant to the

[12] Conversational analysis is in the offing here, but there is no inkling in the book of a corresponding empirical approach.

[13] Gardiner had a point, however. Allport's classical *Social psychology* of 1924 does contain a chapter on language and gesture, but it is no more than a review of the traditional physiology and individual ontogeny of speech, plus the obligatory remarks on its role in exerting "social control."

[14] The same term that Reinach (1913) had introduced.

immediate context." (p. 48). This certainly is an important insight, anticipating the modern work on "on-line" sentence processing.[15] Understanding speech is an act of intelligence: "I cannot insist too often upon the facts that words are only clues, that most words are ambiguous in their meaning, and that in every case the thing-meant has to be discovered in the situation by the listener's alert and active intelligence." (p. 50). Regrettably, these useful leads are not tracked down further by Gardiner: "It is not for the philologist to expatiate upon the psychical equipment which enables man to perform his linguistic functions." (p. 48).

All speech involves predication. There is something that is spoken about and there is something said about it. The former is Wegener's "exposition." If it is explicitly brought to the listener's notice, it becomes the *subject* of the sentence. The latter, in course of being said, is the *predicate* (p. 261). These notions would be further developed by the so-called "Prague school" of linguistics. Gardiner's distinction of sentence types naturally emerged from Bühler's three functions: expression (*Kundgabe*), evocation (*Appell*), and representation (*Darstellung*).[16] They correspond to the pure forms of exclamations, demands (questions and requests), and statements. However, Gardiner stresses (more than Bühler himself) that all three functions are involved in any single act of speech. It is, for instance, a rule rather than an exception that a sentence's prosody can accentuate a particular function over and above the "verbal" content. The latter Gardiner calls the *locutional* sentence form, the former the *elocutional* sentence form. The sentence *My account is overdrawn*, for instance, is normally a statement, but with elocutional question intonation (*My account is overdrawn?*) it will be perceived as evocation, functioning as a question. The point here is that elocution brings in the listener's inference. Such observations were, of course, not new, but Gardiner is a master at sketching the larger picture of language-in-use, of speech acting in context. This formed a useful counter-weight to the dominant structuralist approaches in linguistics, which were almost exclusively occupied with form. It also invited back the psychologists who had so dramatically lost ground in theoretical linguistics. Gardiner correctly castigated the psychologists of language for their lack of social perspective. He only trusted Bühler.

Karl Bühler

From Würzburg to Vienna

We left young Karl Bühler in Chapter 7, after his clash with Wundt over the experimental methodology of the Würzburgers. Here we will consider Bühler's mature work on language and its functions, which culminated in his *Sprachtheorie*. This work appeared in 1934, when Bühler had reached the pinnacle of his career as both a general psychologist

[15] See, for instance, Hagoort and van Berkum (2007).

[16] Gardiner rejected the five-fold distinction of language functions in the work of Ogden and Richards (1923). Bühler completely ignored that work.

Fig. 9.1 Karl and Charlotte Bühler. © Bühler Archive.

and a psychologist of language. Major sources of information about Karl Bühler have been provided by Arnim Eschbach.[17]

Bühler had followed Oswald Külpe as his assistant, first to Bonn (1909–1913) and then to Munich, where Külpe suddenly died in 1915 while Bühler was serving his country on the western front in World War I. In the same year he was appointed as Külpe's successor, but nevertheless he remained in military service till the end of the war. Bühler spent the last two years of the war mostly in Munich, as medical superintendant of the motor vehicle department. At the university he taught psychology and also was a colleague of psychiatrist Isserlin, with whom he worked on brain-damaged war victims.

These must have been eventful years for Bühler, the more so because in 1916 he married Charlotte Malachowski, Külpe's bright 22-year-old PhD student, whose dissertation we discussed in Chapter 7 (Fig. 9.1). She had been perplexed by Karl's abrupt proposal only two weeks after they first met.[18] In 1917 the first of their two children was born.

The war years had severely delayed the publication of Bühler's influential, frequently reprinted text *The mental development of the child.* It appeared in 1918 and we will return to it in Chapter 10. From 1919 to 1922 Bühler taught at the Technical University of Dresden, but he then moved to his ultimate position, holding the chair of psychology at the University of Vienna till 1938. The dramatic turn of events in Bühler's life upon the *Anschluß* of Austria in 1938 is covered in Chapter 14.

In Vienna Bühler had to build his scientific environment practically from scratch. It had two components: he directed the university's Psychological Institute and taught at

[17] Among them Eschbach (1984, 1988) and Eschbach and Halawa (2005). It is Achim Eschbach's everlasting achievement to have established the "Bühler Archive" at the University of Essen, to have edited a score of volumes on Bühler's life and work, and to be editing Bühler's collected work. He generously provided me with documents relevant to the present chapter and to Chapter 14, with the portrait in Figure 9.1, and with a host of other information.

[18] C. Bühler (1972), p 18–19.

the city of Vienna's teachers' college. The president of the city's education committee, Otto Glöckel, was a leader of educational reform in Austria. Attracting the Bühlers to the capital's teachers' college was a major coup for the movement, which pleaded for a child-centered, evidence-based approach to school education (instead of the traditional drill system). The combination of responsibilities inspired Karl and Charlotte to develop a broad research program in both basic and applied psychology. Over the course of time the Psychological Institute developed three research groups.[19] The first was in experimental psychology. Karl built up a well-equipped laboratory for the study of perception, memory, thought, and attention. From 1927 this group was supervised by Egon Brunswik. The second group was dedicated to child and youth psychology. Charlotte was the leader of this group, but soon Hildegard Hetzer shared the responsibilities. They developed new methods for educational research, such as questionnaires, interview techniques, statistical procedures, and diary studies. One important outcome of this work was Charlotte Bühler's 1933 book on life-long human development. The third group, directed by Paul Lazarsfeld, concentrated on economic psychology. Last but not least Karl Bühler concentrated on theory development. As he phrased it at the end of a review of the Institute's activities: "Nothing is more practical than a good theory."[20]

There was a natural division of labor here, not unlike what we saw in Wundt's case. During the 16 Vienna years, Bühler hardly ever published experimental work of his own, but he carefully supervised the experimental research of many PhD students. One of them, working on color contrast, remembers: "He meticulously examined the experimental setup. He asked me about every detail, about the reasons for setting it up precisely this way. He was especially interested in my methodological considerations. Nothing should be done accidentally this or that way – he said, but immediately added that sheer accident could lead to the discovery of important connections."[21] Bühler's own work was almost exclusively theoretical. He was always working on one book or another, often in connection with courses he taught or seminars he conducted with his students and visitors. He was a brilliant teacher, almost performing like an actor, lively and humorous. "One got the impression that he took pleasure in finding the precise, though often peculiar linguistic formulation of his thoughts."[22] Some of his lectures were attended by an audience of a thousand. There was a weekly colloquium, where students would report on their research or discuss some paper or another. It was always followed by a joint dinner

[19] See Ash (1988) for further details.

[20] K. Bühler (1924, p. 73).

[21] "Er prüfte die Versuchsanordnung minutiös. Er fragte mich nach jeder Einzelheit, nach dem Grund, weshalb ich etwas gerade so angeordnet hatte. Meine methodischen Überlegungen interessierten ihn besonders. Man dürfte nichts 'zufällig' so oder so machen—sagte er, fügte aber sofort hinzu, dass nichtdestoweniger oft gerade der Zufall zur Entdeckung wichtiger Zusammenhänge führe." (Kardos 1984, p. 38).

[22] "Man hatte den Eindruck, daß es ihm eigens Freude bereitete, die präzise, wenn auch oft eigenartige sprachliche Darstellung seiner Gedanken zu finden." (ibid. p. 33).

in a nearby restaurant. Bühler was "kind and humane,"[23] and had students in great quantities, some 130 in psychology and 40 in philosophy. Among them were many of the future leaders in psychology, philosophy, and biology, including Paul Lazarsfeld, Egon Brunswik, Hildegard Hetzer, Karl Popper, and Konrad Lorenz. There was also a stream of foreign visitors. Karl and Charlotte Bühler had jointly created one of the world's liveliest and most influential research centers in psychology. They also invested substantially in cross-Atlantic relations. Among their many American visitors were Edward Tolman and Neal Miller, and they frequently lectured in the United States (Stanford, Johns Hopkins, Harvard, University of Chicago). For many years their Institute enjoyed a substantial grant from the Rockefeller Foundation. Karl received an honorary degree from Wittenberg College, and in 1930 he was offered William McDougall's former chair at Harvard, with Charlotte receiving a call to a Radcliffe chair. They rejected the offer. Karl was inclined to accept, but Charlotte preferred to stay in Vienna with her booming research team. Only two or three years later, under the increasing threat of Nazism, Karl and his Jewish wife Charlotte might have reacted quite differently. Charlotte called it (in 1972) "a probably fatal decision."[24] We will return to the Bühlers' final years in Vienna in Chapter 14.

Karl Bühler's ten books and numerous theoretical papers range widely from studies in Gestalt perception[25] to child development, school psychology, test standardization, metatheoretical issues, and language. Language was probably his main theoretical concern during the Vienna years, culminating in his *Sprachtheorie* of 1934. His work on language became his most influential and lasting contribution to science. In the following sections we will consider some of its highlights.

The functions of language

Language had been on Bühler's mind since his early habilitation work on thinking. The notion of an imageless sentence frame kept returning in his later work. During the 1908 Congress of Experimental Psychology in Frankfurt, Bühler presented a review of research in speech and language comprehension.[26] It treated the perception of consonants and vowels, the perception of words in isolation, and sentence understanding. There was, as yet, very little to report on the latter topic apart from Bagley's work (discussed in Chapter 5) and the Würzburg "discovery" that the meaning of a perceived sentence was not an image, but an imageless thought. However, Bühler made a virtue of necessity by reviewing some relevant linguistic literature. Here he introduced Wegener to the psychological audience, in particular Wegener's notion of the hearer supplementing and

[23] "er war lieb und menschlich." (Kardos 1984, p. 33).

[24] C. Bühler (1972, p. 27).

[25] K. Bühler (1913), his only experimental book. Bühler did not consider himself a Gestalt psychologist, on methodological grounds, but the notion of Gestalt was central to his thinking.

[26] K. Bühler (1909a).

completing what is said on the basis of the situation and shared action schemas. Much is supplied (*mitgedacht*) by the hearer.

Bühler's views on the functions of language emerged in his 1909 review of Marty's (1908) *magnum opus*. In this book Marty had rejected Wundt's view that language is primarily an expression of thought. Marty aligned rather with Wegener by stressing that the purpose of speech is to affect the listener. The meaning of an utterance, according to Marty, is normally to induce or "insinuate" some particular judgment in the hearer.[27] In his careful and detailed review, Bühler remarks that the hearer perspective is fine, but just as one-sided as Wundt's expressive perspective. And for completeness' sake, Bühler added a third perspective, which had been central in the classical British philosophical tradition, namely the naming or representational function of language (although it was not called a "function" in that tradition). Marty, Bühler claimed, "does not recognize this representing (*Darstellen*) as an independent function."[28] It is in this review that Bühler coins his term "representational function" (*Darstellungsfunktion*), which was to play such a crucial role in all of his later work. He also gave a first indication of the theoretical program he was about to embark on, with his *Sprachtheorie* as the ultimate outcome: "If the distinction between the different functions of language has any value for the psychology of language . . . it will justify the proposed separation of the representional function from the other two. The purpose of a functional doctrine will have to be to systematically approach the issues only touched upon here. The thing is to take up all functions, to understand them in their characteristics, to describe their combination, their super- and subordination and their interplay in a given discourse."[29] Many of the traditional conflicts in the literature are precisely due to disregarding one or another of these essential functions. Bühler intended to be the mediator and sketch the larger picture.

As a spin-off of his 1918 text on child development, Bühler published the paper Gardiner had been so impressed by.[30] That paper opened with the following statement: "Threefold is the performance of human language: expression (*Kundgabe*), evocation (*Auslösung*) and representation (*Darstellung*)." It had by now become his established framework. Bühler uses it to discuss the emergence of functions in the child's speech. He also discusses some of the subtle ways in which these functions interact in discourse. For instance,

[27] "Ein Sprachmittel habe die Bedeutung oder Bedeutungsfunktion z. B. einer Aussage heißt uns also: sie sei in der Regel bestimmt (und in gewissen Grenzen auch fähig) dem Hörer ein Urteilen von bestimmter Art zu suggerieren oder zu insinuieren." (Marty 1908, p. 286).

[28] "Aber er [Marty] erkennt dieses Darstellen nicht als eine selbständige Funktion an." (K. Bühler 1909b, p. 965).

[29] "Wenn die Unterscheidung verschiedener Funktionen der Sprachmittel überhaupt einen Wert hat für die Sprachpsychologie, dann liegt er, so meint der Ref., in der Richtung der angedeuteten Tatsachen, und darum wird das Angeführte die vorgeschlagene Trennung der Darstellungsfunktion von den beiden anderen zu rechtfertigen imstande sein. Aufgabe einer Funktionslehre müßte es sein, all die nur berührten Fragen systematisch zu erledigen. Es wären alle Funktionen aufzusuchen, in ihrer Art zu erfassen, ihre Kombinationen, die Ueber- und Unterordnung und ihr Durcheinander in einer gegebenen Rede zu beschreiben u.s.f." (ibid. pp. 966–967).

[30] K. Bühler (1918b).

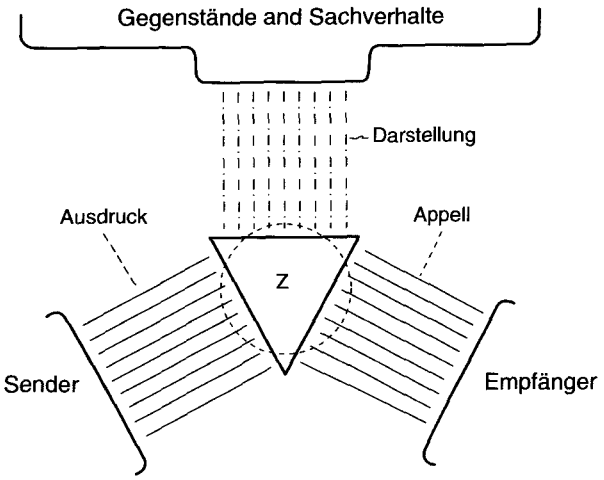


Fig. 9.2 Bühler's Organon Model. The circle in the center represents the speech signal. The sides of the triangle represent the signal's three functional relations. The triangle's left side reflects the signal's function as the expression of a sender/speaker (*Ausdruck*). The right side reflects the signal's appeal to a recipient/hearer (*Appell*). The top side reflects the signal's representing relation (*Darstellung*) to objects and states of affairs (*Gegenstände und Sachverhalte*). From Bühler (1934, p. 28).

when you say *The train pulls in, the doors are opened and the king gets out*, the speaker expresses the three events as they come to mind. At the same time, however, the order in the sentence "objectively" refers to the real order of events. The expressive and representational functions go hand in hand. But this is by no means always the case. A speaker can express a certain conviction by saying *There exists historical justice.* The conviction is expressed, but not explicitly referred to, as it is in *I am convinced that there exists historical justice.* This interplay of functions finds its ultimate formulation in Bühler's so-called "Organon Model."

The Organon Model

Bühler presented his Organon Model (see Fig. 9.2) in Hamburg to the 1931 Congress of the German Psychological Association.[31] Crucial to the model is the representational function. This is precisely what makes language a *symbolic* system. Speech is not just a sign emitted by a sender (a "symptom") and picked up by a receiver (as a "signal"). That is speech as studied by the behaviorists. Animal communication is limited to such sign-signal situations, but human communication is not. This is not sufficiently recognized by the behaviorists, who ignore the semantics of speech signals. Here Bühler refers, with due respect, to de Laguna's (1927) "acute" elaboration of this sender/receiver view on language, its origins and ontogenesis. However, this view cannot suffice. It is the

[31] K. Bühler (1932). Fig. 9.2 represents the ultimate Organon diagram in *Sprachtheorie* (1934).

representational function of speech which makes speech uniquely human. The speech sign is a "symbol," representative of something else, some real or imagined state of affairs "out there."[32] The *Sprachtheorie* is largely about this symbolic function of language as explicitly expressed in the book's subtitle.[33] Before we turn to this central aspect of Bühler's theory, we should spend some time on what Bühler had to say about the sender/receiver character of language.

Bühler was much inspired by recent developments in the study of natural animal communication systems, in particular the work by Wheeler and Wassmann on the communication of ants and by Frisch on bees.[34] These systems are cybernetic in nature, according to Bühler. They are cooperative signaling systems by which animals can affect or "steer" one another's behavior. Just a year before his *Sprachtheorie*, Bühler had published his book *Ausdruckstheorie* ("Expression theory"). In this work Bühler reviewed the intellectual history of expression theories, among them Darwin's and Wundt's. A recurring theme in the book is the functional significance of expressive movements, an issue much underestimated by Wundt and also by Darwin. An example is Darwin's "first principle":

> The first of these principles is, that movements which are serviceable in gratifying some desire, or in relieving some sensation, if often repeated, become so habitual that they are performed, whether or not of any service, whenever the same desire or sensation is felt, even in a very weak degree.[35]

Bühler discusses the case of frowning, which may originally have been an action to protect the eyes, for instance when scrutinizing a distant object in glaring light. By some law of evolutionary inertia, the frowning response was never entirely lost because nothing else interfered with it. The emotion involved in scrutinizing still releases the frowning response, also in cases where it does not serve any of the original protective function. Bühler disagrees. He rather forcefully expresses the notion of "functional shift."[36] The obvious, probably universal, social significance of frowning makes it unlikely that it is, at the same time, a functionless remnant. Rather, the original behavioral response acquired a new function in human evolution, a *social* function of signaling to another member of society that one is scrutinizing something, in thought or otherwise. Such has been the functional course of many facial movements; they became mimical, often symbolic gestures. Although Darwin never denied the possibility of functional shift, it was Bühler who stressed the ever evolving signaling functions of expressive movements. For social animals, expressive movements are constituents of *actions* in the first place, serving some signaling function or other. They are rarely self-contained "useless" archaic remnants.

[32] On this point, Bühler time and again acknowledges his indebtedness to Husserl, who, however, regrettably ignored the expressive and appeal functions of language.

[33] "Die Darstellungsfunktion der Sprache."

[34] See K. Bühler's *Die Krise der Psychologie* (1927), pp. 37–47.

[35] Darwin (1872, p. 347).

[36] Adopted by his student Konrad Lorenz.

Functional shift is at the basis of social evolution. Expressed emotions always function in the cybernetic sender/receiver domain of the Organon Model.

Turning now to language, its expressive function is, in its purest form, apparent in children's first words, according to Bühler. Here Bühler (1918b, p. 2) refers to Meumann, who, as we saw in Chapter 4, was among the first to claim that first words are not representational, but merely expressive of affect or will. The case is more complex in normal adult speech. There are the emotional interjections and imperatives, such as *Ouch!* and *Help!*, which are dominantly expressive in children's first words, but what is expressive in a normal assertion or question? Bühler's views are somewhat floating here. Expression (*Kundgabe*) is everything in speech that characterizes the speaker. Prosodic properties of speech, such as intensity, tempo, rhythm, and melody, often express the speaker's state of mind, over and above what is factually said (p. 9). That is the traditional view, such as formulated by Herman Paul, Wilhelm Wundt, and Otto Jespersen. But Bühler also includes in the expressive function the speaker's implicit or explicit propositional attitudes. In the example, saying *There is historical justice* is performing both a representational and an expressive function. The criterion or litmus test for the representational function (*Darstellung*) is the sentence's truth value. The representational "performance" (*Leistung*) of the sentence is that it is true or false for the state of affairs at issue (p. 3). What it expresses is the conviction of the speaker that this is indeed the case. The conviction is not explicitly referred to in the sentence *There is historical justice*, as it is in the fuller sentence *I am convinced that there is historical justice.* Whereas truth is the litmus test for the representational function of an utterance (*Darstellung*), sincerity (*Echtheit, Wahrhaftigkeit*) is the essential characteristic of its expressive function (*Kundgabe*), (pp. 11–12). Although truth and sincerity often go together, they need not, Bühler argues. The natural relation is, for instance, violated in lying, where the speaker's (tacit) belief is not sincerely expressed.

Statements always perform both functions, representation and expression of propositional attitude, at the same time. The same holds, although in a different way, for questions. When I ask *Who built Rome?* I express the wish to be told about some state of affairs. At the same time I make reference to that state of affairs. In other "belief" questions, such as *Does God exist?*, it is the degree of conviction that is at stake. It is an appeal to the hearer to provide evidence for a sincere belief (or disbelief) in the state of affairs referred to (the existence of God). Questions, according to Bühler, essentially involve all three functions (p. 17). This is the closest Bühler gets to modern speech act theory, or almost, as we will see in the final section of this chapter.

As far as the function of appeal is concerned, Bühler does not add much to what Wegener and Marty said. The signaling function is often carried "by accent and melody" which serve to "impress and steer the hearer."[37] All in all, Bühler gave remarkably little attention to the psychology of expression and appeal in language use. His main business is *Darstellung*, the representational function of language, to which we turn now.

[37] "Beeindruckung und Steuerung des Hörers." (K. Bühler 1932, p. 119).

The two-field theory of reference

Language is a signing system, unique in the animal world because it is symbolic in nature. The signs of language are not just signals. Words and sentences have a representational function. They can stand for objects, events, and states of affairs. The value of these symbols, words in particular, is dependent on the context in which they appear, in particular the syntactic context. Just as the perception of a figure depends on its surround or "field" (a central thesis of Gestalt psychology), the value of a content word depends on the syntactic surround in which it appears. Bühler calls this the "symbol field." The word's case, for instance, depends on the syntactic frame in which it appears. In the typical English (or German) noun–verb–noun sentence frame, as in *Hans hears Gretchen*, the case value of the first noun (*Hans*) is nominative, whereas the second noun (*Gretchen*) has accusative case. These case values are dependent on the *position* of these words in the sentence frame, the symbol field. The symbol field is the *context* of interpretation. But there is still a second field operative in language, which Bühler calls the "pointing" or deictic field (*Zeigfeld*). When somebody says *He hears Gretchen*, the truth or falsity of the sentence depends on the situation in which it is uttered. The word *he* has no fixed reference; its value depends on the situation of discourse. I can literally point to some person and say "he." Terms such as *I*, *you*, *he*, but also *here* and *there*, *now* and *then* (often called "indexicals") are deictic terms whose value depends on the situation in which they are uttered. Pointing is not symbolic, according to Bühler. Deictic terms are not symbols. They are signs; they just point to items in the situation.[38] Bühler's two-field theory is his elaboration of the deictic situation and the symbolic context in which language functions. We will consider them in this order, as Bühler himself does in *Sprachtheorie*.

The deictic field

Bühler by no means *discovered* indexicals. Linguists such as Paul, Wegener, and in particular Brugmann (1904) had devoted much attention to them already, which Bühler acknowledges generously. Bühler's own contribution was two-fold. First, he systematized the field and, second, he recognized a hitherto ignored dimension of the deictic field, which he coined *Deixis am Phantasma* ("deixis in imagination").

Bühler distinguishes three types of deixis. They differ in the kind of situation that forms the deictic field. The first type he called *demonstratio ad oculos*. It is the deixis one can perform in the perceptual environment that interlocutors share. This deictic field has a three-fold origin. It is the *I* of the speaker, his *here* and his *now*. They are the origins for the personal, local, and temporal coordination systems of the speaker. From the speaker's personal origin his interlocutor is *you*. From his local origin the deictic term *there* points to something not-here. And from his temporal origin, *then* makes reference to something not-now. These are only the beginnings. An immediate issue is that the deictic fields of two interlocutors need alignment for a speech act to work. In the simplest case the *I* of the one participant in a dialogue will be the *you* of the other and inversely the *here* of the one

[38] K. Bühler still treats a verb like *to come* as purely symbolic, non-deictic.

participant will be the *there* of the other, but the *now* of both coincides. Bühler did not mention that the origin for the deictic term *we* may include both participants (so-called "inclusive we"). It need not, however, namely when *we* points to speaker plus somebody else. But Bühler does speak, more generally, of the division of *roles* in the speech event.

Similarly, the spatial origin of local deixis, *here*, is not simply the space occupied by the speaker. Is it the particular spot between the two eyes, Bühler asks, which the great vision researcher Ewald Hering called the "cyclopean eye"? Or is it the head of the speaker or the whole standing body of the speaker? The orientation of the deictic field depends on which origin is at play. When the speaker refers to the *left* of his deictic origin, *left* of his head need not coincide with *left* of his body. These are different varieties of "egocentric" orientation. But Bühler also observes that the orientation of the deictic field need not be egocentric. When a vehicle, such as a train, moves forward, a natural front–back orientation results, with orthogonally a left–right dimension. These are different deictic orientation systems, but Bühler happily claims that the "field values" of such orientation systems are mutually translatable. That, however, is by no means the case.[39] Bühler had no idea of the topology of such orientation systems. As everywhere in the *Sprachtheorie*, Bühler's approach is informal, even discursive. His rhetorical style is one of "thinking aloud."[40] Only pages before his body-scheme treatment of *here*, Bühler admits that all Viennese refer to Vienna using *here*, just as all Berliners refer to Berlin using *here*. It is an "easily understandable sloppiness or vagueness" in an extended use of the term.[41] Finally, Bühler elaborates in some detail Brugmann's thesis that the use of deictic terms in many cases requires some kind of factual pointing. Where is *there*? Where is *he*? Such terms can be quite unspecific in a given perceptual discourse situation, requiring some kind of gestural support. Bühler discusses various kinds of pointing in local deixis, such as finger pointing and eye fixation. But a speaker can also use acoustic cues, speaking softly when a nearby *you* is addressed and more loudly when a further away *you* is the addressee.

The second type of deixis Bühler distinguishes is *anaphora.* Here the deictic field is not the perceptual *situation*, but the discourse *context.* The ongoing discourse, still present in immediate memory, forms the deictic field. Anaphoric terms point to particular items or moments of what has been said. Much discussed in the linguistic literature are the relative pronouns, such as in *the man* ***that*** *came.* Here *that* is pointing back to the just mentioned *the man*, and Bühler discusses the long noticed fact that such anaphoric terms stem from the perceptual deictic field. They function there as well, as *that* does, for instance when you say ***that*** *man*, pointing to a particular person. Personal pronouns are another major category of anaphoric terms, as in: *The man came.* ***He*** *carried a bag.* They can point back not only to earlier persons or objects, but also to concepts or states of affairs.

[39] See Levinson (2003, p. 56).

[40] which, of course, it is not. Bühler labored for over a decade to write his *Sprachtheorie*. The chapters were presented and discussed in his seminars. He knew it was going to be his *magnum opus.*

[41] In a paper entitled "Wo ist hier," Klein (1978) generously acknowledges Bühler's contributions to the theory of local deixis (and his "peculiar" style), but, no doubt unintentionally, the paper demonstrates how little Bühler had understood of the lingustics of local deixis.

*Hans talked about the political situation. **It** was tense.* Bühler also mentions the anaphoric connectives, such as *however*, which points back to a just made statement. Bühler rarely produces examples of his own. He cites from the available Indo-European literature. He is not actively construing and exemplifying the cases he discusses, which makes the reading rather abstract. But he is quite explicit about the point he wants to make: anaphora is *deictic* to the core. Anaphora is a pointing device. The field in which the pointing is done is the ongoing verbal context. This includes pointing forwards, to the context-in-the-making, as in *Have you heard **this**: Hans has found a new job.* For this, Bühler introduced the now standard technical term *cataphora.* The pointing context of anaphora is, in fact, the symbol field: the words and sentences produced or under construction. Anaphora is "syntactic pointing" (p. 388). It is "eminently called upon to link the pointing to the factual representing [*Darstellung*]."[42]

The third type of deictic field that Bühler treats had not been recognized earlier as such. His *Deixis am Phantasma* is somewhat like anaphoric deixis, but it is not the current discourse that forms the deictic field. The pointing is not so much to what is in immediate memory, but rather to matters remembered or thought up, some state of affairs in the interlocutors' imagination. When somebody tells you a story or when you read a novel, you are led or guided through some imaginary world. Pointing and being oriented is as normal here as it is in the "real" perceptual world or in the world of discourse. There will be some *here, now, me* origin in this world; you perceive it with your "mental" eyes and ears. You imagine objects in this world of imagination to be in front of you, or behind you, or "over there." Bühler refers to an experiment by Martin who asked subjects to put an imaginary pot of flowers on the real table in front of them. They easily do so. The imagined pot of flowers is then "in front of" the subject. More generally, you can imagine a whole world around you, without changing your "here" and "now." That world has come to you, as the mountain came to Mohammed. Alternatively, you can displace yourself through the imagined world, walk through a fantasized town or museum. This is somewhat like Mohammed going to the mountain. And then, there is a third case according to Bühler: It is the speaker here and now indicating some imagined object. Bühler would ask his students in the lecture hall to point to the (invisible) Stephansdom. They would more or less successfully point in the right direction. They could have said *there, behind the opera.* Here, to complete Bühler's metaphor, neither Mohammed nor the imagined mountain moves. Bühler correctly observes that this opens interesting issues for the psychology of language. Texts induce "games of relocations" in imagination. How is the use of deictic terms, such as *here* or *there*, material in realizing such relocations? Where do protagonists end up in the imagined world? This has become a chapter of psycholinguistics.[43]

[42] "... daß die Anaphora in eminentem Maße gerade dazu berufen erscheint, das Zeigen mit dem Darstellen zu verknüpfen." (K. Bühler 1934, p. 123).

[43] See Tversky (2005).

The symbol field: a two-class system

Language is a very special signing system, according to Bühler. Being bipartite, it is different from most other signing systems, such as signing flags. Bühler called this the "dogma of lexicon and syntax" (p. 75). There is, first, a system or class of denoting symbols. They are the lexical elements, roughly the words of a language. Second, there is a syntactic "field system," which "allows one to represent by means of a limited set of conventions and corresponding syntactic constructions . . . an unlimited variety [of things] . . . The human languages that we know today can all claim to be such '*productive*,' even *universal* symbol systems."[44] Languages share this dual property with our number systems. "We can also symbolize numbers *ad infinitum* with just ten elementary signs and a simple, conventionally agreed syntax."[45] Bühler stresses the psychological advantages of such a productive system. It helps us transcend our "capacity limitations" (p. 77). If a language community only had a lexical system, with no syntactic "field," the lexicon would grow to enormous proportions in order to symbolize whatever the multitude of language users makes reference to. We would have to be "acrobats of mnemonics" (p. 77). "Only field systems of the type 'language' can, under the limitations of material and memory capacity, . . . provide the needed *productivity* and adaptability."[46] This is entirely the modern generative perspective, more explicitly expressed than by most of Bühler's contemporaries. Moreover, his psychological grounding is interestingly to the point. Whereas the early generativists argued that generativity is *restricted* by memory limitations, Bühler claimed the reverse: our limited memory capacity is precisely *overcome* by the syntactic productivity of language.

Before we turn to Bühler's treatment of the two classes, the lexicon and its syntactic "surround," it is important to discuss his views on what makes a linguistic sign a *relevant* sign. It is captured in his "principle of abstractive relevance."

The principle of abstractive relevance

A major influence on Bühler's thinking came from his Vienna colleague Count Nikolaj Sergeyevich Trubetskoy (1890–1938). Trubetskoy taught Slavic philology at Vienna University from 1922 to 1938, exactly the period of Bühler's chair there. He is still recognized as the major phonologist of his era. Trubetskoy fully clarified the notion of "phoneme," phonemes being the *distinctive* elements in a language's sound system. This led him to make a rigorous distinction between phonetics and phonology, phonetics

[44] ". . . was sofort mit einem Feldsystem erreicht wird: mit einem beschränkten Schatz von Konventionen und dementsprechend von Sprachgebilden unbeschränkt Mannigfaltiges hinreichend differenziert und exakt zur Darstellung bringen." (K. Bühler 1934, p. 76).

[45] "Wir können auch Zahlen ins unbegrenzte mit nur zehn Elementarzeichen und einer sehr einfachen, konventionell festgelegten 'syntax' symbolisieren." (ibid. p. 77).

[46] "In unserem Zusammenhang genügte der Nachweis, daß eine praktisch ausreichende *Produktivität* und Anpassungsfähigkeit unter den Grenzbedingungen des gegebenen Materials und der Gedächtniskapazität, die wir fixiert haben, nur von einem Feldsystem vom Typus der Sprache erwartet werden kann." (ibid. p. 78).

dealing with the physics of speech sounds, phonology with the linguistically distinctive or oppositional properties of speech sounds. Trubetskoy (1929) had developed these notions in an essay on vowel systems. Vowels vary along five physical or acoustic dimensions. The properties of brightness (*Eigenton*) and satiation (*Schallfüllgrad*) span the well-known vowel triangle. In addition, vowels vary in duration, intensity, and melody. Any spoken vowel has some value, which can in principle be physically measured, on all five dimensions. That is a fact of phonetics. But a language may not use all five dimensions for distinguishing its vowels. Adyghe, for instance, a Caucasian language, has only three vowels which are distinguished on just one dimension, satiation. But other languages oppose their vowels on more than a single dimension. A dialect of Ostyak, a Uralic language, uses two dimensions to distinguish its seven vowels, brightness and satiation. Other languages (among them some Turkish languages) have vowel distinctions on four dimensions. These are facts of phonology. More generally, phonological opposition is the basis of any relevant distinction of sound elements in a language. It is the vowels that oppose the words *pull* and *peal.* The vowel phonemes are the distinctive elements. They are opposed on the brightness dimension. It is irrelevant whether you also vary them in duration or melody; the relevant distinctive opposition is maintained. These notions are worked out in brilliant detail in Trubetskoy's posthumus *Grundzüge der Phonologie* ("Fundamentals of phonology") of 1939.

Bühler (1931, p. 38) fully adopted Trubetskoy's innovative conception when formulating his own *Principle of abstractive relevance.* For something to be a linguistic sign, it must possess some relevant property on which it is meaningfully distinguished from other signs. It may vary on other, irrelevant, properties without losing its identity as a sign. The system abstracts away from those other features of the sign. Only the relevant property is "diacritic" within the system as a whole. Although Bühler explains the principle from Trubetskoy's analysis of vowel systems, he claims the principle has general validity for all levels of linguistic description, including lexicon and syntax. We will discuss how Bühler applies the principle in his analysis of syntactic case marking.

Bühler explicitly depicted the principle in his Organon diagram (Fig. 9.2). The central circle depicts the speech event in its full physical complexity. The triangle represents what is sign-functionally relevant in the speech event, what makes it a *symbol.* This is, normally, less than the fullness of the speech signal. But it can also happen that some diacritically relevant feature is not realized in the speech event. A signal may need further interpretation (Bühler calls it "apperception") on the basis of the context in which it appears, in modern terms its redundancy. Hence, circle and triangle (signal and symbol) partially overlap, neither one fully included in the other. Let us now turn to the two classes in Bühler's symbol field, lexicon and syntax.

Lexicon

Bühler was, of course, aware of the fact that the two-class system also functions at the level of the lexicon itself. Some 40 diacritically distinct phonemes suffice to produce a great variety of meaningfully distinct word symbols. Between the phonemes and the words are the syllables and Bühler went through the following counting exercise: His German

dictionary contains some 30,000 words, not counting their morphological variants. How many syllables are there? He counted the number of meaningful syllables on successive pages of Goethe's *Wahlverwandschaften*, making a cumulative graph of the number of *different* syllables he came across. The curve approximated an upper limit of some 4000 syllables.[47] Syllables, however, fulfill no particular diacritic function in distinguishing between words, according to Bühler. That is only realized by their "phonemic description." It is the words' phonemic patterns that make the abstractly relevant distinctions or oppositions. They are the linguistically functional Gestalts. Bühler elaborates on this in his 1931 paper, introducing the modern concept of lexical neighborhood. To distinguish among the "few thousand monosyllables," relevant diacritics will be those that can usefully distinguish between similar neighbors (pp. 48, 51). To determine these relevant diacritic dimensions, the best approach is to consider neighborhoods of maximal similarity. What is it that opposes monosyllables such as *balm* and *barm*, *cold* and *gold*, and so on?

The lexical two-class system involves a second class of phonemic and morphological constructions by which the possible word Gestalts can be produced. Bühler is not very clear about this. His discussion of syllables may have been intended for this, but there is nothing substantial on the structure of syllables, except that they tend to be redundantly distinguishable. His discussion of morphology is equally disappointing. Bühler mentions compounding, such as in *housetop*, in which two "symbol values" are combined. This he distinguishes from a case like *house's* (as in *the* house*'s paint*), where a symbol value combines with a "field value," namely the genitive relation (*house's*) to another element (*paint*) in the syntactic construction (*Sprachtheorie* p. 295). But basically nothing is said about inflectional morphology or derivational morphology. Bühler in passing mentions nominalization (*Substantivierung*) and adjectivization, but derivational morphology is still not there, 34 years after Wundt, who had also ignored it. Bühler's treatment of the lexicon, its properties and functional use, shows no linguistic sophistication.

Syntax

How do words perform their representative (*Darstellungs-*) function? Here context plays a crucial role. One case Bühler handles is that of a guest in a coffee shop, who orders: *a black one*. The waiter has no problem understanding this; his apperception supplements what is needed. The client is simply being efficient. The reference could not have been more obvious; "therewith, it seems to me, everything psychological has been said." (p. 157). Bühler in particular rejects the idea, suggested in linguistic circles, that the speaker silently construes a sentence, only uttering part of it. But here the German example is interestingly relevant. The German guest said *einen schwarzen*. Is there really no syntax here? Why then are both determiner and adjective marked for masculine-accusative?

[47] Bühler is unclear about including *bound* meaningful syllables, such as syllabic inflexional suffixes. He probably did not, just counting the number of different monosyllabic *words*. He almost certainly did count as different items cases such as *lieb* versus *liebt*, because he refers to that distinction on p. 35. Bühler is also unaware of the unrivaled Kaeding (1898) counts.

Obviously, the elided noun "Kaffee" (coffee) is masculine and the accusative case further implicates an object role in the tacit syntactic frame. Bühler did not notice any of this. He did not see that elliptical constructions (not just a minor "remainder of cases," p. 166) are just as syntactic as completed ones. He was right, of course, but not terribly original, in claiming that ellipsis requires context to function.

Still, syntax was for Bühler the ultimate battle ground for his two-class (lexicon–syntax) symbol field. The productivity of human languages is the result of their syntactic composition. The notion of syntactic frames had intrigued Bühler since his Würzburg years. In 1918 he reviewed recent theories of syntax, in particular criticizing Wundt's expression theory. In *Sprachtheorie* Bühler calls Paul's and Wundt's definitions of the sentence "driveling and obsolete."[48] The essential issue, according to Bühler, is the "original creative act of the composition,"[49] whatever that means. To do justice to this, Bühler continues, we should consider "the sentence as *a closed and well-occupied symbol field*"[50] One aspect of this special symbol field, as already discussed, is that it allows for pointing within and across sentences, i.e., its anaphoric potential. Another and crucial aspect is its compositional potential and a third essential feature is its case structuring. In the following we will consider Bühler's handling of the latter two syntactic field properties.

Composition

Bühler, as we have seen, was well aware of the generative potential of language and its psychological grounding. He based it in the syntactic "class" of the symbol field. But how should one conceive of this syntactic productivity? On this matter Bühler was as much in the dark as all of his contemporaries. There was no inkling yet of an algorithmic approach to "productivity." That had to wait till the early 1950s, in particular for Chomsky's dissertation. What Bühler did do was to review and comment on the traditional approaches to composition. He cited Hermann Paul, who wrote that "the essence of the sentence is, that it indicates the *act of combining* several parts."[51] He also cited, on p. 174, Paul's listing of seven ways to express the relations among these parts (or in Paul's terms the relations among the *images* that are expressed by the words in the sentence). They are word grouping, word order, the stressing of words, sentence intonation and tempo, the use of conjunctions, prepositions, and auxiliaries, and inflection. Bühler both accepted and criticized Paul's view that composition is *additive.* It holds for the addition of a sentence (*he laughs, she weeps*), of a predicate (*he fell and died*) or of a subject (*Hans and Gretel wandered around*). But according to Bühler composition is more than additive. There is something like *merging*, as proposed by other linguists, such as Paul Kretschmer (1866–1956). Three main cases here are the subordination of a second sentence to a first (as in *I am afraid he will die*), of a first sentence to a second one (*If you follow me, we are in better shape*) or the

[48] "schwammig und veraltet" (K. Bühler 1934, p. 322).

[49] "*Urschöpfungsakt* der Komposition" (p. 322).

[50] "... daß der Vollsatz *ein geschlossenes und wohlbesetztes Symbolfeld* aufweist." (p. 366).

[51] "Denn das Wesen des Satzes besteht ja darin, dass er den Akt der Zusammenfügung mehrerer Glieder bezeichnet" (p. 321).

embedding of one sentence into another (*a dreadful drake, revealed by the Olympian himself, threw itself . . .*).[52] According to Bühler, Paul's additive type of composition is useful for making reference to some complex state of affairs, whereas Kretschmer's merging type is especially fit for forging an intentional unity. In *I am afraid he will die*, the first, "subordinate" sentence *I am afraid* does not refer to a state of affairs, but rather expresses the speaker's attitude to the content of the second, "superordinate" sentence *he will die.* (p. 406).

In discussing the factual composition speakers perform, Bühler time and again returns to his notion of a syntactic schema or frame. Without offering new data, he cites his 1908 conclusions: "When we want to express a difficult thought, we first select the sentence form for it; we first become conscious of a plan of operation, and it is this plan which governs the [selection of] words." What becomes conscious is almost always "what *mediates* between thoughts and words, the knowledge of sentence form and the relations of sentential components among them, something that counts as the expression of the grammatical rules that live in us."[53]

Bühler had no inkling of the ways in which the relations among these sentential components contribute to sentence meaning. How is the meaning of the sentence built up from the meanings of its parts, in particular its words? If a sentence is a representation (*Darstellung*) of some state of affairs, is the representation truthful? Nowhere in *Sprachtheorie* does Bühler refer to Frege's ground-breaking (1892) paper *Über Sinn und Bedeutung*, which marked the beginning of the modern discussions of meaning. But here Bühler is not alone. Over the entire era we are considering in this book, the psycholinguistic study of meaning was by and large the study of word meaning. Closest to a consideration of compositional meaning came Bühler's discussion of case relations.

Case structure

The symbol field is structured by case relations. Bühler had come to realize that the syntactic frames he had studied since his Würzburg years were carriers of case relations. His discussion of case in *Sprachtheorie* begins with Wundt. Bühler not only sketches the "well thought-out and cautious case doctrine" (p. 236) of Wundt, but also adopts "an astoundingly elegant twist" (p. 241) that Wundt had proposed. Clearly, Wundt was not *just* "driveling and obsolete" in Bühler's view.

In Wundt's case theory a two-way distinction was made, the "concrete" and the "conceptual" cases. In *Hans walks to the door*, the concrete, locative goal of Hans's walking is the door. This (oblique) case is marked by the preposition *to*. The case role of *Hans* is

[52] p. 406., Kretschmer (1923, p. 62).

[53] "Wenn wir einen schwierigen Gedanken ausdrücken wollen, dann wählen wir erst die Satzform für ihn, wir werden uns innerlich erst des Operationsplanes bewußt, und dieser Plan ist es dann, der erst die Worte meistert." "was . . . zwischen Gedanken und Wörter *vermittelt*, ein Wissen um die Satzform und das Verhältnis der Satzteile unter sich, etwas was als direkter Ausdruck der grammatischen Regeln, die in uns lebendig sind, zu gelten hat." (K. Bühler 1934, pp. 253–254). Notice that the first sentence expresses a notion very close to Kay Bock's theory of grammatical encoding (Bock 1995).

conceptual; the actor's role is expressed by the nominative case. Bühler called Wundt's observation that the conceptual case, but never the concrete case, can be expressed by position alone "astoundingly elegant." In *Hans sees Gretel* and *Gretel sees Hans*, the conceptual case roles of subject and object are completely transparent. Wundt, Bühler observes, had considered this from the fact that, if there is also overt case marking of these roles, nominative and accusative marking are often identical (such as for feminine nouns in German (*Die Frau führt die Kuh*—"The woman leads the cow") or for all plural nouns (*Die Männer führen die Kuhe*—"The men lead the cows")). If they are identically marked, only position can distinguish them. The quite regular identity of nominative and accusative forms is, according to Wundt, due to a tacit, underlying conception of action, in which subject and object are both potential actors. That is rather trivially the case when both are animate (like *Frau* and *Kuh*), but we can easily conceive of a storm or even a stone as an actor in conceptual events ("the storm/stone ruined the window"). This underlying action scheme is the conceptual model (*Denkmodell*) our cultures apply to this kind of event structure. We would now call this thinking-for-speaking.[54]

Bühler adopts this view, but additionally stresses that positional marking of the conceptual case takes the verb as *origo* of the symbol field. "If there is no verb, there can be no real object case"[55] (which makes Bühler's treatment of *einen schwarzen* the more surprising). Nominative, accusative, dative are all "satellites of the verb."[56] Their position with respect to the verbal origo is the distinctive syntactic feature that is "abstractively relevant" for case.

Bühler used this origo-plus-satellites notion to discuss his thoroughly modern idea of lexical "empty slots" (*Leerstellen*). They govern the mutual syntactic selection among parts of speech, their patterns of unification, as we now call it. Let us cite Bühler in full on this point:

> In every language there exist affinities: the adverb looks for its verb and so do all other [word classes]. Another way of formulating this is that words of a particular class open around them one or more *empty slots*, which have to be filled in by words from other classes.[57]

This is by no means limited to verbs and their "satellites." It is also at work in the formation of compounds. A key, for instance, is a key to something. The word *key* has an empty slot for its target, such as *house*. Their mutual affinity governs the compound formation

[54] Slobin's (1987) useful term for retrieving conceptual features that require obligatory marking in a language, such as tense for English, but not for Indonesian. See also Chapter 13 and Levelt (1989, pp. 103–105).

[55] "Es gebe keinen echten Objektkasus, wo das Verbum fehlt." (K. Bühler 1934, p. 243).

[56] "Verbaltrabanten" (ibid. p. 243).

[57] "Es bestehen in jeder Sprache Wahlverwandtschaften; das Adverb sucht sein Verbum und ähnlich die anderen. Das läßt sich auch so ausdrücken, daß die Wörter einer bestimmten Wortklasse eine oder mehrere *Leerstellen* um sich eröffnen, die durch Wörter bestimmter anderer Wortklassen ausgefüllt werden müssen." (ibid. p. 173).

house key (p. 246). These syntactic affinities are core properties of words. They constitute their "field suitability" (*Feldfähigkeit*).[58]

The sound stream

A speaker's performance involves more than the multiple operations within the symbol field. Speaking is also the operation of a physical device, chest movements that press air through a vocal tract of variable shape. The listener has an auditory system that is highly sensitive to minor variations of a sound stream's sonority, intensity, satiation, and duration. According to Bühler, these "material" properties of the sound stream lead their own life in parallel to the grammatical operations of the symbol field. An interesting case of this "own life" Bühler mentions is now known as *resyllabification*. "It happens for instance regularly that what belongs to two words is captured in one syllable." (An English instance is the syllabification of *I gave it* as *I-ga-vit*, where the last syllable stems from two words). The syllabification runs its own course here, ignoring the lexical segmentation in the symbol field. There is no serial operation here, grammatical encoding preceding sound encoding. But running in parallel does not mean being independent. There are multiple ways in which the two processes can and do interact. However, Bühler does not consider this "cooperation" in any detail. Instead he discusses one essential aspect of speech sound streaming, its syllabification. In doing so he tries to bridge the chasm between two schools of thought.

There is, on the one hand, the acoustic tradition, going back to Helmholz, Stumpf and, in particular, Sievers, Jones, and Jespersen. In Sievers' (1876) *wave theory* of syllabification, the speech sound stream is characterized by a wave pattern of *sonority*. Syllable boundaries are located in troughs between sonority peaks. Sievers observed that the sonority wave pattern is highly correlated with the intensity pattern of the speech signal. The listener's perception of syllabification is probably jointly determined by these two properties. There is, on the other hand, the behavioristic motor theory of syllabification, proposed by Stetson (1928) and his coworkers. Here, syllables are the product of ballistic chest pulses. Ballistic movements are only *initiated*, but not further controlled. A chest pulse is like throwing a ball. After its initiation, the course of movement is autonomous. Ballistic movements, Stetson argued, require minimal energy, somewhat like the effort required to keep a child moving on a swing. What a speaker perceives and experiences is not objectively measurable and hence is irrelevant. Here Bühler plays his characteristic role of critical mediator. Both perspectives on syllabification are indispensable, he states. The auditory system can distinguish much more than the speech motor system can produce. More importantly, the auditory and motor systems always act in cooperation, a much under-researched issue, according to Bühler. During our motor articulation we monitor our own performance by both kinesthetic and auditory feedback. Conversely, when we listen to somebody's speech, we often tacitly produce it ourselves in imitation.

[58] "denn ein Merkmal des Wortbegriffes ist die (syntaktische) 'Feldfähigkeit' der Lautzeichen, die wir als Wörter bezeichnen." (K. Bühler, p. 187).

This is not necessarily *overt* shadowing; it can be entirely internal. We are always senders and receivers at the same time. Bühler's is probably the first motor theory of speech perception.

Bühler's axioms

In 1927 Bühler published his often reprinted *Die Krise der Psychologie* ("The crisis of psychology") in which he tried to break down the "dividing walls" between consciousness psychology, behaviorism, psychoanalysis, and Gestalt psychology. We will not follow Bühler in these constructive, mediating efforts,[59] but limit ourselves to a few remarks on his chosen approach. Bühler considered the foundations of psychology. The essential object of psychology is, according to Bühler, of three kinds: (conscious) experience, meaningful behavior and their relation to mental products/performance. It was of the *Zeitgeist* for Bühler to capture these essentials *axiomatically*. It was very much the same "postulates" approach we saw applied by Weiss and by Bloomfield (although there are no mutual references in this respect). According to Bühler, the domain of language is governed by three axioms. First, social life requires a system for the mutual control of meaningful behavior. Second, to serve the self-interest of society's members, the control system must allow for both expression and appeal. Third, by relating signs to objects and states of affairs they achieve a new meaning dimension, which multiplies their communicative capacity (pp. 50–51). It is not hard to discern Bühler's Organon Model in these axioms. As it should be in an axiomatic account, it should be complete and that was Bühler's conviction: "I am of the opinion, that the three meaning dimensions of expression, appeal and representation constitute the universe of pure language in full."[60] Still, in his *Sprachtheorie* Bühler extends his axiomatics substantially. There a special axiom captures the tripartite functionality of language. A further three axioms have to do with the distinctive ("abstract") signing nature of language, with an extension of Saussure's *langue/parole* distinction and with Bühler's two-class system of the symbol field. We will not go into the details of this axiomatic exercise. Bühler did not have a formal, mathematical mind. Nowhere is there an effort to deduce theorems from these axioms, let alone to provide proofs for such theorems.

Bühler and the Prague school

Bühler, as we have seen, was deeply influenced by his Vienna colleague Trubetskoy. Trubetskoy, in turn, participated in the *Cercle Linguistique de Prague*, which was founded in 1926 by Vilém Mathesius (1882–1945), professor of English at the University of Prague, Roman Jakobson (1896–1982), emigré from Russia, and Trubetskoy. The "Prague school" would for several decades become a unique center for the linguistic study of "information

[59] Although it is hard to suppress the temptation to cite Bühler on psychoanalysis and its founder Sigmund Freud ("the penetrating thinker," "great magician," "undaunted thinker"). Freud was Bühler's colleague at Vienna for 16 years but Bühler never chose to meet him.

[60] "Ich bin allerdings der Meinung, daß die drei Sinndimensionen der Kundgabe, Auslösung und Darstellung den Kosmos der reinen Sprache restlos konstituieren." (K. Bühler 1927, p. 51).

structure" and the organization of topic-comment structure, as pioneered by Wegener and Marty. Through the work of, in particular, Petr Sgall (born 1926) and Eva Hajičová (born 1935) this ultimately became an integral part of modern functional linguistics. The *Cercle* was also, with Trubetskoy and his dear friend Roman Jakobson, the leading European think tank in phonology.

Bühler accepted Trubetskoy's invitation to participate in the 1930 International Congress of Phonology in Prague. There he delivered the paper, in which he developed his principle of abstractive relevance. In his turn, Trubetskoy adopted Bühler's Organon Model. His *Grundzüge der Phonologie* ("Fundamentals of phonology") opens with a presentation of Bühler's three functions, as applied to the sound structure of language. Here Trubetskoy argues that the core of phonology deals with the representational (*Darstellungs-*) function. But Bühler inspired him to also consider the expressive and appeal functions. In only a few pages, Trubetskoy presents a lucid discussion of what this would entail. The starting point is Bühler's specification of the expressive and appeal functions of a speech event. Expressive properties are those that characterize the speaker. Appealing properties are those features of the speech signal that in themselves evoke some emotional response in the hearer. Trubetskoy first distinguishes between the phonetic and phonological properties. Bühler, we saw, had fully adopted that distinction, but he had never applied it to the expressive and appeal functions. Trubetskoy mentions that there is much in the speech signal that is symptomatic for the speaker. It carries information on the speaker's sex, age, health, education, character, and so on,[61] but only those properties that the speech community *conventionally* uses to characterize its speakers are phonologically relevant. These properties are usually language-specific. The same holds for the appeal function. There is much in the speech signal that can evoke an emotional response in the hearer, but phonology is only concerned with the linguistic means that are conventionally used for particular appeals. Trubetskoy then presents several carefully worked-out cases of expressive phonological features in different languages. One example from Tamil is the caste-dependent rendering of the same phoneme as either *č* or *s*. Another concerns systematic sex-related phonological distinctions, such as the way Russian women render *o* as *ua*.[62] Such phonological distinctions occur in many languages, but are always language-specific conventions. Trubetskoy has more trouble finding good phonological cases for the appeal function. His main example is vowel or consonant lengthening, as in *schschööön!* ("beautiful!"), but he immediately adds the subtle observation that this is not conventionally used to evoke a *specific* emotion. It can also serve to engender delight, irony, indignation, and more. The diacritic only says: "have an emotion!" Which emotion is appealed to, the hearer must derive from context.

Strict application of Bühler's Organon Model requires the development of three domains in phonology: representational, expressive, and appeal phonology. But clearly,

[61] Thomas Pear (1931) had already introduced an experimental procedure to test the distinctiveness of these properties in the speech signal (see Chapter 12).

[62] For more precise detail see Trubetskoy (1939, p. 21).

phonology's lion's share will be in the representational chapter. It is more practical, according to Trubetskoy, to accommodate the other two chapters under sound stylistics.

The Prague school did not leave Bühler's doctrine of three functions untouched. We will not discuss the proliferation of functions proposed, except for mentioning the additions Roman Jakobson made, although these were much later, in 1960. Aside from Bühler's original three functions (expressive, conative, i.e., appeal to the hearer, and representation), Jakobson distinguished, first, a *poetic* function. This can be seen at work in our preference for saying *Joan and Margery* as opposed to *Margery and Joan*). Second, a *phatic* function, which is to establish or maintain the channel of communication (such as saying *Hello?* on the phone) and, third, a *metalingual* function, which is the function of making reference to the code of communication (such as in *Let me address you in Dutch* or *What do you mean by "phatic"?*). Jakobson made another, much earlier, contribution to psycholinguistics, which became a classic. His slim monograph *Child language, aphasia and phonological universals* appeared in 1941, after he had left Prague for Copenhagen. We will discuss it in the next chapter, on language acquisition.

Functions and speech acts in retrospect

Although the term "speech act" was widely used in the literature we have discussed in this chapter, in particular in Bühler's work, no serious speech act theory was developed during the first half of the twentieth century. Only Adolf Reinach saw the light, considering speech as a social act with contract-like properties: the speaker's intention to affect the partner's mind in specific ways, the necessity of the partner's registering that intention, and the partner's presupposition of sincerity on the part of the speaker.

Counterfactuals are hardly useful in a historical text, but I cannot help wondering what Bühler would had done if he had seen Reinach's text in 1913. At that moment both Bühler and Reinach lived in Munich, but there is no evidence they ever met. Considering the functions of language might have been a different exercise for Bühler if he had been familiar with Reinach's quasi-legal social act perspective. During the years before World War I Bühler was essentially completing the text of his *The mental development of the child.* In that text, ultimately published in 1918, we find Bühler's most explicit statement on the nature of (assertive) speech acts and it takes the legal perspective. Here is the full extract:

> When somebody, for instance as witness, makes some statement on oath, he assumes a responsibility, though not in first instance for the truth, but for the sincerity of his statement. It is not falsity . . . but perjury which law threatens to punish, and accordingly it is the conviction of the witness the judge is primarily occupied with. Such a guarantee indicates . . . what we call the sense of an assertion: *The assertion expresses a conviction*, precisely the speaker's conviction, which covers the state of affairs referred to.[63]

[63] "Wenn jemand z.B. als Zeuge eine Aussage beschwört, so übernimmt er damit eine Verantwortung, aber nicht in erster Linie für die Wahrheit, sondern für die Wahrhaftigkeit seiner Aussage. Nicht der Falscheid (wenn er nicht etwa durch Fahrlässigkeit verschuldet wurde), sondern der Meineid wird ja vom Gesetz mit Strafe bedroht, und dementsprechend ist die Überzeugtheit eines Zeugen das erste, was den Richter beschäftigt. Eine solche Garantie weist also auf die zweite Seite an dem, was wir den

It would have been a small step for Bühler to consider the functions of language from Reinach's "contractual" social act perspective. He might have developed the insight that the core function of language is to make binding social commitments, rather than just to communicate information about states of affairs. He came very close in this citation and in his 1932 paper. As with counterfactuals, we will never know what would have happened. What we do know is that Bühler left the issue untouched in his *Sprachtheorie* and that the breakthrough had to wait for John Austin's work in the 1950s.[64]

Sinn eines Aussagesatzes nennen, hin: *Der Aussagesatz gibt eine Überzeugung des Sprechers kund*, die sich auf den dargestellten Sachverhalt erstreckt" (Bühler 1918a, pp. 294–295).

[64] Leading to his major work, Austin (1962). See Seuren (2009), Vol. I, Chapter 4, for a thorough historical discussion of modern speech act theory.

Chapter 10

Language acquisition: Wealth of data, dearth of theory

The attentive reader may by now be suffering from acute data deficiency. Being fed on theory alone, be it structuralism, thought psychology, behaviorism or functionalism, does not satisfy the empirical psycholinguist. That holds most clearly for the students of language acquisition during the first half of the twentieth century. If anything characterizes this period, it is the concern for collecting rich and reliable data. Diary keeping, the major source of evidence during the nineteenth century, reached new standards of sophistication. The testing of linguistic skills became an industry, in particular in the United States. The study of individual language development and skill was complemented by large-scale comparative and statistical studies. Educational psychologists began measuring how children fare in acquiring reading and writing skills or second language proficiency. They also performed extensive statistical studies on the effects of bilingualism on intellectual development. When you read these studies, the theoretical denomination of the writer is usually transparent. Still, the data-based exchange of views among developmental psycholinguists was, luckily, less dogmatic than the theoretical exchanges we considered in the previous chapters. In fact, theoretical exchanges were the exception rather than the rule. If anything characterizes twentieth century developmental psycholinguistics, it is a dearth of theory.

This chapter opens with a section on Clara and William Stern, who set the scene, or rather the example, for twentieth century research in language acquisition. We will then introduce the leading developmental psycholinguists or teams thereof, proceeding in chronological order. This is the order in which their most significant monographs or papers in language acquisition were published. The second half of the chapter more specifically discusses three topics that were particularly high on the research agenda throughout the century. The first concerns the growth of vocabulary, syntax, and utterance complexity, a topic increasingly involving large-scale statistical studies. The second concerns the early acquisition of speech sounds. It is the only topic where real theoretical progress was made. The third and last topic concerns child bilingualism and its potential hazards for the developing child.

Clara and William Stern

The foremost text of the twentieth century, and also the first to appear, is *Die Kindersprache* ("Child language"), Clara and William Stern's monograph of 1907 (Fig. 10.1). It transformed the nineteenth-century diary practice into a modern objective database approach,

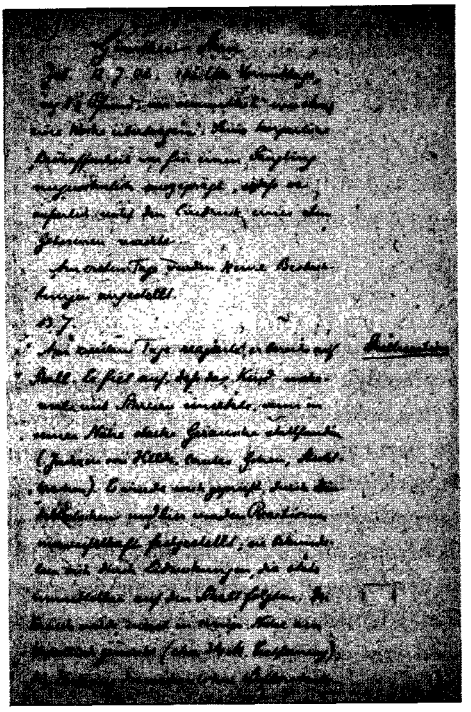


Fig. 10.1 Clara and William Stern, and the first page of the diary on Günther (dated 12 July 1902).

setting new standards for rigorous separation of data and interpretation. It demonstrated how a sufficiently extensive diachronic database on a number of children could be used to systematically test important theoretical issues.

Clara Joseephy (1878–1945) was born in Berlin as the daughter of a banker. It was only 30 years later that German universities would allow women as students on their premises. Clara was without academic degree or training when, at the age of 21, she married William Stern, a university lecturer in Breslau. She defied her parents, who considered him a "poor beggar."[1]

Born as Wilhelm Louis Stern (1871–1938), William studied, under rather restricted financial conditions, in Berlin. There he received his PhD in 1893 under the supervision of Hermann Ebbinghaus. In 1897 he moved to the University of Breslau, where he taught till 1916. It is during this Breslau period that Clara and William designed and systematically executed their grand developmental project. The aim for the project was to produce a series of six monographs on major aspects of child development, including the acquisition of language. The database for these monographs would primarily consist of systematic diaries, kept by Clara, on the development of their children. They were Hilde, Günther, and Eva, born in 1900, 1902 and 1904.

[1] "armer Schlucker" in the words of Eva Stern (Stern-Michaelis 1991, p. 133).

The data gathering started for all three children at birth and continued to the ages of 12, 10 and 7 respectively. Systematic observation was terminated in August 1912. By then the Sterns had collected almost 5000 pages, over a page of notes every single day. They consisted of two types. Clara observed the children all day and noted down whatever seemed relevant to her. This ranged from reactions to sound and light right after birth, wake/sleep rhythms, babbling patterns, new words, first narratives, attempts at lying and signs of moral judgment. The other type of data consisted of systematic recording sessions. Here one of the parents, usually but not always Clara, would sit down with the child and elicit picture descriptions, narratives, and play commentaries. The other parent, usually William, was "at work," but in fact jotting down in shorthand everything the child uttered. This was entirely natural for the child, because the parents had a joint study. At regular times the collected data were scrutinized to establish the *status quo* of a child's language development. The categories of development were largely dictated by the data themselves (e.g., early vocabulary as relating to objects, actions, properties). This assessment was immediately followed by "filling the gaps," observations/tests on what had remained unclear in the analyses.

From the historical perspective, it is interesting to note that this procedure *à trois* was, after some 120 years, the first application of Johann Heinrich Campe's (1785) methodological advice for the creation of diaries: "this work is almost too heavy for one man; one needs two equally acute observers, who divide the business among them, so that the one always stays with the child, while the other one notes down his observations."[2] Also noteworthy is the fact that the Sterns never referred to Jan Baudouin de Courtenay, who had been much ahead of them in establishing a huge systematic database (see Chapter 4). They must have been totally unaware of this work, in spite of the fact that Breslau (now Wroclaw in Poland) was just 200 miles from Warsaw. This work left no trace in the acquisition literature, unlike Baudouin's work on phonology. The 1928 updated edition of *Die Kindersprache* does refer to another diary produced in Breslau, by Ernst and Gertrud Scupin.[3]

An early application of the Sterns' database, still before the appearance of *Die Kindersprache*, can be found in William's monograph *Helen Keller*, published in 1905. Helen Keller (1880–1968) needs no introduction. Blinded and deafened by a brain infection, probably meningitis, at the age of 19 months, she was assigned a private teacher

[2] "... aber für einen Menschen ist diese Arbeit fast zu groß: es würde zwei gleich scharfsinnige Beobachter erfordert, die das Geschäft unter sich theilten, so daß der eine jedes Mal bei dem Kinde bliebe, indeß der andere seine Beobachtungen niederschriebe." (Campe 1785, p. xxiv).

[3] Ernst and Gertrud Scupin published a two-volume diary of their son Ernst Wolfgang, called "Bubi," under the title *Bubis erste Kindheit* (1907, 1910). The father was a doctor in Breslau, the town of the Sterns; they doubtless knew each other. The updated 1928 edition of *Die Kindersprache* makes ample reference to this diary. It is not a text, but just a diary, with short entries every few days. It also reports on the occasional test, such as color naming. It is entirely matter-of-fact, which made it a useful data source for students of child development. It indeed became widely used, not only by developmental psycholinguists, but also by psychoanalysts. The whole diary was also immediately and fully translated into Dutch as an example for dedicated parents.

when she was almost 7 years old. This had been mediated by the Perkins Institute in Boston, where another famous deaf and blind person had been educated: Laura Bridgman. Keller's teacher was Anne Sullivan, 19 years old when the educational odyssey began. Sullivan was a rare pedagogical talent who took Helen in tow, opening the world for her at an amazing speed. The linguistic medium was finger-spelled English, the letter signs tapped on the palm of Helen's hand. This was never "taught," let alone "conditioned." Sullivan's approach was "conversational." She would, all during the day, tap everything interesting or relevant on Helen's hand, while Helen's other hand was touching objects, people, body parts or materials. Helen began to imitate the finger scribbles and fairly soon discovered that "things have names." It was the beginning of language acquisition at an incredible rate. The story has been told often enough. In particular by Helen herself, who published *The story of my life* while she was a Radcliffe student.

After the appearance of this autobiography (which was immediately translated in German), William Stern set out to consider the psychological implications of this unique case. Helen's development provides a rare window on the basic problem of psychogenesis: "What are the mutual shares of capacity and environment, of innate tendency and acquired materials?"[4] The monograph is a lucid analysis of Helen's case in this light. A core part consists of her stages of language acquisition. Here Stern compares 14 landmarks in Helen's early acquisition to the same steps in Hilde's. They go from the first substance-naming stage (objects, persons) to later multi-word stages, first negation sentences, to beginning inflection, pronomina use, and why-questions. It turns out that, with one minor exception, the order of stages is exactly the same for Helen and Hilde. This indicates the existence of genetic laws at work. The rate of acquisition, however, was three times as fast in Helen's case as in the much younger Hilde's case. The final part of this monograph is dedicated to the education of deaf, and deaf and blind people. Here William Stern expresses the, by then traditional, misgivings about the use of sign languages. "As long as the deaf-mute person, as in France, is limited to his sign language, even though artificially enriched, his intellectual development will . . . remain stunted."[5]

Die Kindersprache was largely based on the diaries the Sterns kept; they had run for almost five years by then. But it also referred to other diary data, such as Preyer's and Lindner's. The book consists of three parts. Part I summarizes the stages of speech development in Hilde and Günther, respectively. It provides a summary of the collected data. Part II deals with the psychology of language acquisition. Here the Sterns develop their balanced theory of what is native (such as early expressive movements, "natural symbol formation," rate of acquisition as related to the child's sex) and what is environmental (such as "conventional symbol formation," language-specific syntax, pronoun use as

[4] "Immer mehr bildet sich geradezu als Grundproblem aller Psychogenesis die Frage aus: Wie sich die Anteile von Anlage und Milieu, von angeborener Tendenz und erworbenem Material, zueinander verhalten." (Stern 1905, p. 14).

[5] "Solange der Taubstumme, wie in Frankreich, auf seine, wenn auch künstlich bereicherte Gebärdensprache beschränkt bleibt, muß auch seine intellektuelle Entwicklung, wie wir oben sahen, eine verkümmerte bleiben." (ibid. p. 65).

related to sibling position) in the acquisition of language. The heart of Part II concerns the children's syntactic development. It forms a considerable advance in comparison to earlier work and it was rarely matched during the decades to follow.

The first words, according to the Sterns, are one-word sentences. The authors defined the sentence as the expression of a token position with respect to some state of affairs.[6] What is at stake, even in children's first words, is their taking position. Initially the position taken by uttering a word is affective-volitional, as Meumann had argued. The attitude "I want food," for instance, is expressed as *hap* by Eva Stern. But although first words are not pure denotation, they are also more than pure "cheering" (*jauchzen*) or "moaning" (*jammern*). There is always a "something" which is wanted, evaded, enjoyed.[7] There is a rich range of prosody which goes with these "egocentric" states of mind.

By the end of the second year two- and multi-word sentences arise. Initially each of these words still functions as a one-word sentence. The child expresses a collection or sequence of interrelated "positions" (i.e., one-word sentences), which are independently intonated. They form a "word string." In syntactic terms they can be a subject and an object (*danna kuha*–"aunt cookie"), object and verb (*pichel hol*–"napkin get") or other combinations. The Sterns stress, like Dewey had done, that initially there are no parts of speech, no nouns, verbs, adjectives, etc. The part-of-speech status of words only emerges in sentential context, marked by inflection and word order. This trivial truth is ignored in most of the literature to follow. Part-of-speech data are regularly reported for children in the one-word stage. The word-string character of early speech is often maintained into three- and more-word sentences. When Hilde 1;10[8] wanted her mother to get her pictures from the backroom, she uttered the string of single words (transliterated): *Mamma, picture have, room, have picture, back, there mamma get.*[9] Initially, these sentences are still volitional, affective, or exclamatory. The first negatives appear by adding a loose negation element (*no*), as in *chair no no—lap*[10] (preferring the lap to the chair). The latter utterance is also a proto-form of coordination.

During the third year, sentence coordination begins by expressing two positions in succession, by two independent sentences: *is not there spoon, cannot paint paint.*[11] But then

[6] "ein Satz dagegen ist der Ausdruck für eine einheitliche (vollzogene oder zu vollziehende) *Stellungnahme zu einem Bewusstseinsinhalt.*" (Stern and Stern 1907, p. 164). In 1928 (p. 179) the latter term became *Bedeutungstatbestand.*

[7] "Das erste Sprechen ist zwar gewiss kein blosses Bezeichnen, aber ebenso gewiss auch kein blosses Jauchzen und Jammern, sondern ein Hinstreben zu einem Etwas, eine Abwehr gegen ein Etwas, eine Freude über ein Etwas." (Stern and Stern 1928, p. 182). The volitional/conceptual duality of speech in mental development was a theme for European psycholinguistics, for instance Kenyeres (1927) and Mulder (1936).

[8] It was the Sterns who introduced this, now standard, age notation. "1;10" stands for "one year and ten months old."

[9] *mama, bildä hamele, zimmä, hamele bildä, hinten; dada mama eholn* (Stern and Stern 1928, p. 204).

[10] *stul nei nei—schossel* (ibid. p. 207).

[11] *is nicht da schieben, kann nicht mahle mahle* (ibid. p. 208). (*schieben* refers to "Breischieber," kind of porridge spoon).

(for Hilde) during the fourth year, a variety of subordination, with appropriate connectives, arises, for example *you rub your hands, because it's so cold.*[12] Temporal, causal, conditional, and other connectives quickly follow, together with relative clause formation. In the 1928 edition of their book the Sterns refer to what was probably the first statistical study of coordination and subordination by Albert Huth (1919) among 58 kindergarten children in Munich. His 6-year-old subjects already showed double subordinations. The data show the acquisition order of connectives. *Wenn* ("When") is early, *wo* ("where") and the relatives follow, *weil* ("because") and *wie* ("how") are relatively late. Let me add Busemann's (1925) statistics here. They concern school children's "action" expressions as opposed to predicative "property" expressions. He believed that the ratio of these expressions is a "mirror" of the child's mind. In an "action" period, the child is more extraverted, in a "property" phase, the child is more occupied with his own state or condition. Busemann's "verb/adjective quotient" was going to be picked up by the text statisticians, such as David Boder and Johnson (see Chapter 12).

The Sterns provide detailed treatment of questioning. Early in the third year, where-questions appear, such as *where hat?*[13] This is volitional usage, not inquisitiveness. Who- and what-questions follow soon. Not much later the first yes/no questions arise, as in *That eat may I?*[14] But a real further development in questioning only occurs when the child masters subordination. This consists of why- and when-questions. The Sterns speak of a "second questioning age," which emerges somewhere during the third year (the first questioning age concerned the child's asking for names). The initial why-questions are also affective-volitional. They are often reactive to commands or prohibitions (type *warum denn nicht*–"why not?"), but "intellectual" why-questions soon follow, as in *why can houses not walk?*[15] This analysis of questioning is psychological in the sense of "what motivates the child?"

There is, regrettably, little on the development of syntactic form. But the Sterns do provide a section on word order. Two factors play a role here, imitation and spontaneity. Where imitation has worked and the child adopts conventional word order "there is no need for further discussion."[16] Here, the Sterns are still in the Wundtian world where imitation is the default mechanism. They are not yet expounding Guillaume's and Jakobson's later insight that "The child creates by adopting," considering imitation as an active process of recoding. Still, the Sterns often distance themselves from Wundt by stressing the child's spontaneity in both sentence and word formation. The child, for instance, is often doing word ordering by analogy. When the question is asked *womit hast du gepfiffen?* ("What-with have you whistled?"), Günther answers *mund mit*

[12] *du reibst deine hände, weil's so kalt ist.* (Stern and Stern 1928, p. 208).

[13] Günter's *ä hut?* (ibid. p. 213).

[14] *die essen darf ich?* said Lindner's son at 2;3 (ibid. p. 214).

[15] *warum tönnen häuser nicht laufen?* from Ament's niece at 3;1 (ibid. p. 215).

[16] "Sofern sich die Wortstellung des Kindes an die Konvention anlehnt, bedarf sie keiner weiteren Erörterung." (ibid. p. 218).

("mouth with"). The child's sentence formation, like the adult's, requires attentional anticipation. Here, the perceptually given and the affectively loaded will get precedence, as Wundt had argued. And that is what children do, even if it violates the conventional grammatical order. Again, quite convincing data are provided in support. The mechanisms of analogy and conspicuity-first are major determinants of children's early word order.

Part II also sketches and, where possible, explains the stages involved in acquiring the various parts of speech. It ends with Haeckel's thesis, comparing the child's early responses to speech with the responses of trained dogs and compares the stages of the child's early word and syntax productions to the evolution of human languages.[17] It is no exaggeration to label the Sterns' genetic theory as "modern." In his *Psychologie der frühen Kindheit* ("Psychology of early childhood") of 1914, William developed it further as the "convergence theory." We will return to it in the final section of this chapter.

Part III, finally, concerns the linguistics of child language. It discusses the phonology of babbling sounds and sound imitations, as well as the systematic phonological "mutilations" of first words. It carefully discusses the issue, completely rejected by Wundt, of spontaneous word invention by children. It also reviews the processes of analogical and compound-word formation, such as Hilde's (at 4;1) *kaffrig* ("coffeeish") for a napkin with a coffee stain or Günther's (at 4;1) *gelbrot* ("yellow-red") for the color of fire. And it discusses the children's rather productive etymological thinking. When Günter (4;7) heard that an ocean (*Ozean*) is a very big sea, he immediately commented that that is because you say "Oh! oh!" when you see it. *Die Kindersprache* was destined to become a classic. After three editions, William reworked and updated the book. The fourth edition appeared in 1928 and was reprinted in 1975.

The second monograph by Clara and William Stern appeared in 1909, two years after *Die Kindersprache*. It was entitled *Memory, assertion and lie*. Regrettably, this ended the planned series of monographs. Clara herself wrote one further book (1914) under the pseudonym Tony Meyer, in which she published for a general public fragments from her diaries, although with pseudonyms for the names of the three children. That hailed the end of her involvement with the psychology of child development. In the same year William published his *Psychology of early childhood*, which was again largely based on the diaries. William's work was widely recognized. In 1909 he received an honorary doctorate from Clark University on Stanley Hall's initiative (where he was surprised to find himself together with Sigmund Freud).[18] The honors for Clara were not as forthcoming.

[17] The comparative issue would be picked up a century later by Tomasello and Kaminski (2009).

[18] To his embarrassment he found himself, on the steamer to New York, together with Sigmund Freud, who was going to receive the same honor. William had published a devastating review of *Die Traumdeutung*. Their joint cruise and honor did not help much to improve relations. On many occasions William criticized Freud and warned about the potentially detrimental effects of the psychoanalytical approach to children's treatment and education. He claimed limited success. Freud had accepted for his psychoanalytic monograph series a 'Diary of an adolescent girl', which appeared in 1916. Slowly but surely doubts arose about the authenticity of the diary. William Stern, in 1927, joined

Die Kindersprache is frequently cited, but more often than not it is referred to as William's book. Here is just one example of this egregious behavior.[19] Fritz Röttger wrote a doctoral dissertation on the phonetic development of children (to which we will return). His supervisor was Felix Krüger, Wundt's successor in Leipzig. The published edition of the 1931 dissertation opens in its preface as follows "In his work 'Die Kindersprache', the last important publication on the scientific topic of young children's language, William Stern says . . . ."[20] And it keeps going with "his" and "Stern" throughout. Not even his otherwise careful supervisor had the insight or decency to correct this.

In 1916, in the midst of World War I, the Sterns moved to Hamburg, where William had been offered the chair of psychology in the then "Tropical Institute." It already had a few faculties, but it needed people of William's international standing and organizational power to transform it into a full-blown university. William's urgent plea was to achieve this in the interest of Hamburg soldiers returning from the war. And he succeeded. Hamburg University was founded in 1919. William created a leading center for both theoretical and applied work in human development, personality, forensic psychology, and intelligence (he invented the concept of intelligence quotient). Amongst his American post-doctoral students was Gordon Allport. The productive life of the Jewish Sterns was brusquely disturbed in 1933. That story, including the further history of their diaries, will be told in Chapter 14. But at this point we should gratefully acknowledge the efforts of the late Werner Deutsch in researching and promoting the cause of the Sterns.[21]

Leading twentieth-century scholars and research teams before the "cognitive revolution"

Michael Vincent O'Shea

The Sterns' 1907 publication coincided with O'Shea's *Linguistic development and education.* Michael O'Shea (1866-1932), professor of education at the University of Wisconsin had, independently, taken an initiative quite similar to that of the Sterns. He recorded the language acquisition of four children, Harriet, Vincent, Stanley, and Katherine, up to the age that they "had acquired facility in reading, writing, spelling, and composition." O'Shea is curiously cryptic about this database. He does not even mention whether these children were his own (they were), when they were born (Harriet was born on October 27, 1895 and became a successful developmental psychologist), how long they were observed

forces with Karl Bühler in Vienna to get the monograph withdrawn. Freud, finally, gave in. See Graf-Nold (1991) for a detailed account of William Stern's dealings with psychoanalysis; it reads like a thriller.

[19] Other sinners I spotted are K. Bühler, Bateman, Busemann, Decroly, Deußing, Guillaume, Jespersen, Kenyeres, Lewis, Vygotsky, and Weisgerber. It only shows how advanced the Sterns had been in this male scientist culture.

[20] "In seinem Werke 'Die Kindersprache,' der letzten Erscheinung auf dem Gebiete der Wissenschaft von der Sprache junger Kinder, sagt William Stern einleitend: . . ." (Röttger 1931, p. 1).

[21] See, among others, Deutsch (1991, 1994).

(probably for a decade), and what the observations consisted of. The book was written as a text for students of educational psychology and O'Shea did a very good job. Part I of the book reviews, in a balanced way, the child's linguistic development from birth to school age.

The author is an acute observer; his presentation is entirely "evidence-based." The developmental stages are discussed, from first cries to complex syntax involving inflection, agreement, negation, and subordination, the same stages the Sterns had considered. O'Shea also reviewed some of the recent work on vocabulary development, to which we will return. Part II of the book is entirely educational. It deals with issues such as learning to read (it must be the "whole-word" method), how to promote oral expression, how to train "aesthetic" written composition, and how to teach a second language.

Ivan Gheorgov and studies of self-reference

The "diary wave," discussed in Chapter 4, rolled on during the twentieth century, covering several languages other than German, French, and English. Ivan Gheorgov (1862–1936) published extensive diary data on the acquisition of Bulgarian. Gheorgov, born in Veda, Macedonia, studied at Jena University, where he received his doctoral degree for a study about Montaigne. He then became one of the founders and five-times rector of Sofia University, where he taught till the end of his career in 1933. His diaries cover a three and a half years of rather leisurely[22] observation of his two sons. Both his 1908 publication in German and his 1910 publication in Bulgarian are largely data lists and tables on the development of vocabulary and early syntax. The introductory remarks to the German edition make an interesting plea for cross-linguistic research in language development. The Bulgarian child, for instance, will have to do without the convenient infinitive and must develop other means of expressing tense. Some languages have a much more elaborate tense system than others, and the child's mind will develop differently in these two linguistic environments. Similarly, a child growing up in a language without declensions must develop prepositional means for expressing relevant relations. The issues of psychogenesis cannot be fully handled without the availability of comparative language data.[23]

[22] "I can add, that I made all observations conscientiously myself. And that my occupation as a professor at Sofia University allowed me to almost always be at home during the day, with only short interruptions, and to occupy myself with the children. This way, nothing essential in the observation of the children's language development could escape my notice, the more so because my wife, who is also an academic, could support me in this."("Ich muß dabei bemerken, daß meine Beobachtungen mit aller Gewissenhaftigkeit von mir selbst geführt waren. Und da meine Beschäftigung als Professor an der Universität in Sofia mir erlaubte, mit sehr kurzen Unterbrechungen während des Tages mich fast immer zu Hause aufzuhalten und mich mit meinen Kindern zu beschäftigen, so konnte mir in der Beobachtung der sprachlichen Entwicklung der Kinder nichts Wesentliches entgehen, zumal meine Frau, die auch Universitätsbildung genossen, mich darin unterstützen konnte.") (Gheorgov 1905, pp. 223–233).

[23] This, clearly, has been a main motivation for Slobin's (1985–1997) cross-linguistic research project.

These two monographs were preceded (in 1905) by an extensive paper on the development of the children's self-reference. The oldest Gheorgov boy referred to himself in second and third person (*you*, *baby*, *Vlado*) much longer than the younger son, who rather quickly referred to himself in the first person (*I*, *my*). Gheorgov adduced this to "the stronger will" of his second boy. The Sterns, who observed a similar difference between (oldest) Hilde and (second) Gunther, gave a more mundane explanation. Before mastering the complexities of shifting deictic reference to persons, children "opt" for fixed reference (their own name, *mamma*, etc.). When they hear themselves being referred to as *you*, they also take this as fixed reference and may refer to themselves by *you*. How do children acquire *I*? The oldest child would not gain much from imitation. The adult's *I* is quite unstressed and without affective force. But a second child will observe *I*, or possessive *my*, *mine*, used with great emotional expression by the older sibling in response to questions such as *Who wants to have this?* or when claiming control: *my ball.* That invites imitation, even without full mastery of shifting reference. The Sterns (1907) took Gheorgov's paper as occasion for a comparative analysis of such use by first and later siblings in the diary literature, demonstrating that indeed "later" siblings were faster in acquiring these terms. But there is often a long period of mixed use of fixed and shifting reference to self, for examples (in transliteration): *will I the Eva,—will I pencil have, the Eva's mine.*[24] This issue was also taken up by Cooley (1908) and by the behaviorist Markey in his 1928 book (introduced in Chapter 8). The latter analyzed eight diaries on the successive appearance of pronouns. One general finding was that new pronouns appeared at moments of vocabulary "spurts." Markey's interpretation was that these spurts result from peak moments in personal relationships. Another finding was that nominal *I* precedes object or possessive *me* and *mine*. According to Markey, "it is the designation by the child of itself as an active state" that is the child's "primary self symbol" and points to its "behavioristic content" (p. 78), whatever that may mean. This apparently develops later in first-born children than in their siblings. Here Markey referred to the Sterns' analysis. This issue stays on the research agenda to the present day.[25] Gheorgov remained a much cited author during the first half of the twentieth century.

Jules Ronjat and Milivoïe Pavlovitch

In 1913 Jules Ronjat (1864–1925) received his doctoral degree from the University of Paris for a diary study of his bilingually educated son Louis. On page three of his dissertation Ronjat reports the advice he had received from his friend, linguist Maurice Grammont,[26] right after the birth of his son in 1908. It was "There is nothing to be taught or instructed. When you have to say something to him, it suffices to say it in one of the languages you want him to know. But here is the important point: *that each language is*

[24] *will ich de eva,—will ich bleistift haben, de evas mein* (Stern and Stern 1907, p. 244).

[25] See Deutsch (2009) and the further references given in that paper.

[26] Grammont (1902) had himself published observational data about the phonology of two children based on their early speech.

represented by a different person. You should, for instance, only talk to him in French, his mother in German. *Never reverse the roles.* In this way, he will talk in two languages when he begins talking, without hesitation and without making any special effort to learn them."[27] That advice Ronjat and his native German wife took to heart. In his dissertation Ronjat reports that the early phonetic systems are fully distinct from the very beginning. Also, there is no evidence for a general delay in acquiring the French sound system. Although lexical and syntactic interferences can occur, they are nonetheless relatively infrequent. Louis at 3;10 said in German *moos* instead of *schaum* ("foam") in analogy to French *mousse.* Or he rendered (at 4;4) "for decoration" in German as *für schön machen* after the French *pour faire joli.*

Ronjat also reports the early awareness the child has of using two languages. At 1;8 Louis pointed at a boat and said *fit bató* ("Schiff–bateau"), expressing his awareness of the double name. Time and again he translates what he hears from one parent to the other parent. It is rare for Louis to address a parent in the "wrong" language. There are immediate self-corrections when he drops a word from the other language into his speech. Explicit reference to the two languages appears by 2;4: *mommy can talk like daddy* or *une singe, das heisst ein Äffchen.* And at 3;7 *I have two languages, French and German.*[28] The abstract notion of "a language" is clearly apparent.

The one-person one-language (OPOL) principle has survived to the present day. But Ronjat's claim that bilingual education is not disadvantageous was to be much criticized, as we will see.

Another Paris diary dissertation was that of Milivoïe Pavlovitch (1920). The author reports on the acquisition of Serbian by his son Douchan. The child grew up in France, but both parents spoke Serbian to the infant during the recorded period up to 2;0. The few words of French picked up by the child came from the wider environment, beginning in the 14th month, and probably because of a staff member of the university library, who he met daily over lunch. But this little French was without any accent.

Scandinavian diary studies: Otto Jespersen

Diary keeping also began in Scandinavia early in the twentieth century, with studies by Bolin and Bolin (1916, 1920) on the acquisition of Swedish and by Rasmussen (1913, 1922) on Danish. Otto Jespersen's (1916) diary study of his son Frans' language development attracted most attention in the form of Jespersen's rendering of that work in Book II of his *Language* (1922). Jespersen presents a general review of the child's early language acquisition, not only using his own observations on Danish, but also diary data from

[27] "Il n'y a rien à lui apprendre ou à lui enseigner. Il suffit que lorsq'on a quelque chose à lui dire on le lui dise dans l'une des langages qu'on veut qu'il sache. Mais voici le point important: *que chaque langue soit représentée par une personne différente.* Que vous par exemple veut lui parler toujours français, sa mère allemand. *N'intervertissez jamais les rôles.* De cette façon, quand il commencera à parler, il parlera deux langues sans s'en douter et sans avoir fait aucun effort spécial pour les apprendre." (Ronjat 1913, p. 3).

[28] "Je sais deux langues, le français et l'allemand." (ibid. p. 93).

other languages, such as the Sterns', O'Shea's and Ronjat's. Otto Jespersen (1860–1943) was, for over 30 years, professor of English at Copenhagen University. He was a much respected general linguist, frequently cited in psycholinguistic circles. We will not in detail consider Jespersen's insightful presentation of children's early sound development, which yields their first "little language." Jespersen sketches children's development of a first meaningful vocabulary and their first steps in acquiring morphology and syntax. Especially noteworthy are his original observations on "echoism," "the fact that children echo what is said to them" (p. 135). Examples (translated) from Frans's diary are: *The sky is so blue* – Frans: *So boo* and *I shall take an umbrella – Frans: Take rella.* Children can "repeat aloud what they have just heard," or "as much of it as they can retain in their short memories" (p. 135). The general observation is that it is the end of utterances that is retained. Tiedemann, as we saw in Chapter 4, had said the same of the child's production of polysyllabic words. Jespersen presents examples similar to Tiedemann's: *gar* for *cigar*, *chine* for *machine.* But then he uses the echoic mechanism to explain cases of early syntax, i.e., word order, in children. For example, in English you say *She comes* and *Now she comes*, but this is not so in Danish (or German or Dutch for that matter). In Danish it is *Hun kommer* and *Nu kommer hun.* The word order inverts after the adverb. The young child will echo the latter utterance as *kommer hun* and tend to also use that word order in the adverb-less cases, saying *kommer hun* instead of *hun kommer.* This echo-mechanism makes the child a factor in language change, according to Jespersen. It also works in the shortening of first names, observable in many languages. First names typically shorten to their ends, becoming "stump words," for example *Bert* for *Albert*, *Sander* for *Alexander* and so on. Surnames, however, do not show this type of language change. Their shortening typically preserves the word onset: *Goldy* for *Goldsmith* or *Pop* for *Popjoy.*[29] Children use first names, Jespersen argues, not surnames. Stump surnames are caused by adults and hence do not show the word-end echo effect.

A special pleasure to read is Jespersen's frontal attack on both Wundt and the Sterns, who denied that children invent new words. "Wundt goes so far as to say that 'the child's language is the result of the child's environment, the child being essentially a passive instrument in the matter' – one of the most wrong-headed sentences I have ever read in the words of a great scientist." (p. 152). And: "Thus Stern says that his[30] daughter . . . used some words which might be taken as proof of the power to create words," but then rejects all of them by showing that they "had grown out of normal words." An *example* is *Eischei*, meaning *to walk, go.* This originated from *eins, zwei* ("one, two"), hence was not a newly invented word. But just about anything that is called an invention developed from something already in existence, Jespersen argues from examples such as bicycles, typewriters, and fountain pens. William Stern, in his[31] 1928 revision of *Die Kindersprache* felt urged to respond to this critique (p. 390). Jespersen did not distinguish between

[29] But also the stressed syllable.

[30] Yes, "his."

[31] Yes, indeed "his."

playful, one-time "inventions" and lasting ones that become part and parcel of the child's vocabulary. All of Jespersen's examples and just about all others in the literature are of the former kind, Stern argues. This, however, is no longer an argument about the child's inventive capacity, but about the language community's willingness to accept the child's innovation. Jespersen did have a point.

Jacques van Ginneken

Jacques van Ginneken (1877–1945) was a Dutch Jesuit of unbridled ambition. While he was a student at Leiden University, he published, in 1905, a two-volume book on the foundations of "psychological linguistics." His supervisor, Christianus Uhlenbeck, accepted a slightly adapted French version of this study, *Principes de linguistique psychologique* (1907), as his dissertation. Van Ginneken's intention for writing this work was similar to Wundt's for *Die Sprache*, namely to lay the psychological foundations for linguistics; Wundt's influence is apparent throughout the book. But van Ginneken wanted to improve on Wundt. Wundt had only provided linguistic illustrations for his claims, where *proofs* were needed. Much richer linguistic justification was required. The reader of van Ginneken's book is indeed showered with linguistic facts from Indo-European, Basque, and American-Indian languages. The psychological base was also in need of a new formulation. Here van Ginneken eclectically "shopped" in the psychology of his days, drawing from Herbart, Wundt, Lipps, Ebbinghaus, James, Charcot, and in particular from Janet. The latter's analysis of automatisms in behavior greatly appealed to van Ginneken. In addition, van Ginneken formulated some new psychological notions of his own. To the traditional contents of consciousness, perceptions, images, and feelings he added "adhesion" (*beaming* in Dutch), which means something like "take as a fact" or "this being the case." This was his rendering of Binet's "thought without images." It was meant to replace the old notion of apperception. Linguistic units emerge from specific interactions of feelings and adhesions, linguistic units being "constructions," roughly like the "tone groups" defined by the phonetician Sievers. (If you cannot immediately follow this, you are excused.) Van Ginneken also formulated four *laws* of automatic processing. Here is one, the *ideodynamic law*: "every motor representation tends to realize its movement."[32] It is, among other things, used to explain the cause of speech motor movements from the speaker's sequence of motor images. Another law is one of *inertia*. It explains perseverations in speech errors, as discussed by Meringer and Mayer.

There is no doubt that van Ginneken's *Principes* impressed his linguistic colleagues. Roman Jakobson remembered, in 1978: "By the way, my first linguistic interest in the new trends in psychology was awakened by van Ginneken's book of psychological linguistics (1907), which was lent to me by Trubetzkoy in 1915."[33] Others were less impressed. Sapir (1921), for instance, wrote that there are "certain writers on the psychology of language [footnote: 'E.g., the brilliant Dutch writer, Jac van Ginneken'] who

[32] "Toute représentation motrice tend à réaliser son mouvement." (van Ginneken 1907, p. 246).

[33] Cited by Elffers (1996, p. 73) in an excellent paper on van Ginneken's *Principes*.

deny its prevailingly cognitive character but attempt, on the contrary, to demonstrate the origin of most linguistic elements within the domain of feeling. I confess that I am utterly unable to follow them." (pp. 40–41). There were many linguistic reviews of van Ginneken's book, including one by psychologist Karl Bühler. It is polite, but very critical. Bühler wipes the floor with van Ginneken's experimental "proofs" that each word image is always four-fold: auditory, visual, articulatory-motor, and graphically-motor. He criticizes the author's use of "adhesion," an ill-defined notion, as the real content of imageless thought and says that van Ginneken's "proofs" are no more than introspective and of "doubtful" value. He also criticizes the fact that van Ginneken proclaims the notion "construction" as all-important for explaining language change, but then does not use it for that purpose, and so on. Bühler concludes by advising the author to "free this grandly designed work from this psychologically entirely untenable foundation."[34] As a matter of fact, van Ginneken followed this advice. During the 1920s he almost fully shifted to providing a biological base for linguistics, an equally controversial project as we will see.

Why discuss van Ginneken in this chapter? There are two reasons. His "genetic" approach to language evolution (to which we will return) had a direct bearing on the innateness issue, which was ever present in the acquisition literature. In addition, van Ginneken contributed to the diary literature in an unusual but interesting way.[35] He had a sister who kept a diary of her son's language acquisition. She provided her brother with access to the diary and he used it for a special purpose. He was writing a high-school text on Dutch and conceived it in such a way that, following the child's speech and language development, he gradually introduced the properties of Dutch phonology, lexical structure, and syntax.[36] The text appeared in 1917. It was entitled *Novel of a toddler* and indeed it reads as a narrative, or at least there is the developmental thread which keeps the text together, with rich and well-chosen examples from the diary. There are also many other pedagogical tricks, one of which deserves our special attention. Van Ginneken devised a new way of displaying syntactic structure to his high-school pupils, which is shown in Figure 10.2.

The words in a sentence correspond to images in consciousness. They pass by as on a stage. (Here van Ginneken adopted Herbart's model of consciousness.) The diagrams display the words as live actors, who enter into various (syntactic) relations. The left figure displays, for the example sentence *Kees goes sleeping*, the (upright) subject and the bent-over predicate attaching to it. The little standing auxiliary attaches to both. Many more attachment or dependency relations are involved in the more complex sentence diagram on the right for the (transliterated) sentence *Mother, will you now not go crying?*

[34] "Es wäre . . . dringend zu wünschen, daß sich G. . . . frei machte von dieser psychologisch gänzlich unhaltbaren Grundlage" K. Bühler (1909c, p. 294).

[35] I am grateful to Gerard Kempen who "rediscovered" and discussed this work, in Kempen (1996).

[36] Roger Brown, over half a century later, took the same perspective in his *A first language* (1973). His stated intention was to present an "overview, or general plan, of the design of English. . . . This overview is in five parts ordered in what I believe to be the development in children of the knowledge in question." (p. 3).

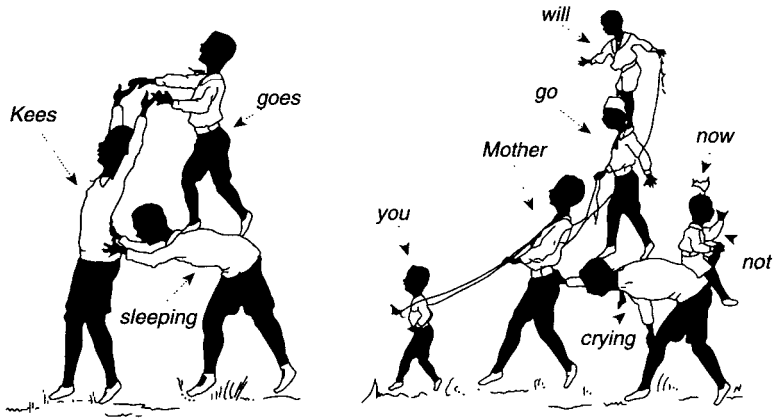


Fig. 10.2 Two phrase structure diagrams from van Ginneken (1917). The English word inserts are mine.

Van Ginneken doubtless knew Wundt's phrase structure diagrams. They may have been a source of inspiration to him. Still, van Ginneken's diagrams are more like the (much later)[37] dependency graphs in linguistics than Wundt's hierarchical inclusion graphs. At any rate, van Ginneken's (1917) curious but inventive phrase structure diagrams are almost certainly the only phrase diagrams published between Wundt (1900) and Nida (1949).

In 1923 van Ginneken became the first professor of linguistics[38] at the newly founded Catholic University in Nijmegen, the Netherlands. He continued to be a man of action, producing a stream of quite controversial publications, and teaching and preaching extensively. He founded three new convent orders and played a prominent role at international linguistic congresses. He was, for instance, invited by Henri Delacroix to the 1933 congress on the psychology of language in Paris, where he presented his biology of language (to be discussed later).

Emil Fröschels

Fröschels (1884–1972) was the founder of logopedics (and coiner of the term) in Vienna, where he occupied a chair in the Medical Faculty from 1924. Long before Jakobson, but, as it were, recapitulating Preyer's approach, Fröschels (1918) had pointed to the parallels of speech acquisition in children and loss of speech in aphasics. This, Fröschels claimed, is due to the layered structure of language. Its building-up or genesis can be observed in the child, its deconstruction in the aphasic. Children go through phases of "dysarthria," "sensory aphasia," "agrammatism," and so on. The book is full of case studies and lively observations. Here is one, underlining the priority of the sentence over its constituents.

[37] Tesnière (1959).

[38] Including comparative Indo-Germanic linguistics and Sanskrit.

An experienced speaker inhales just enough air at the beginning of a sentence to complete it without interruptions. Since this precedes the selection of words, the sentence schema is primary. This is quite different to the early multi-word utterances of children. As the Sterns had described, children pronounce each successive word with sentence intonation; there is no sentential planning yet.[39]

Fröschels was a powerful, unremitting promoter of research and therapy in child and adult speech pathology. His Viennese Newmann Klinik was probably only matched by the ENT clinic in Berlin. He also published, in 1925, an introductory psycholinguistics text, *Psychologie der Sprache* ("Psychology of language"). This small book is, however, hardly a text. It competently introduces the medical student to the traditional typology and anatomy of aphasias. It discusses aspects of developmental aphasia and in a further chapter introduces some core notions of Wundt's *Die Sprache.* There is an unreadable additional chapter by Ottmar Dittrich, including a pages-long story about how the compound word *Hirschkäfer* ("stag beetle") has come about in the human mind.[40] The final, invited, chapter is on Adler's *Individual psychology* and its therapeutical applications to children. In 1938 Fröschels was dismissed from the University of Vienna and built up a new career in the United States. We will return to Fröschels in Chapter 14.

Jean Piaget

Jean Piaget (1896–1980) was born in Neuchâtel, where his father was a professor of history and medieval French literature. Piaget received a 1918 doctoral degree in biology from Neuchâtel University. His life-long interest in epistemology brought him to Zürich, where he spent a year with Bleuler and Jung. He then moved to the philosopher Brunschvicg in Paris. He also became acquainted with Janet's notions of "preverbal intelligence." Henri Simon, who had together with Binet developed the first intelligence tests, asked Piaget to assist him in the construction of intelligence tests for children. This led to Piaget's first notions and publications on epistemological stages in the child's intellectual development. It was this work which triggered Claparède to offer Piaget a position in his Institut Jean-Jacques Rousseau, a pedagogical research institute at Geneva University. We will not consider Piaget's further impressive scientific career in detail. After 1929 he always remained connected to the University of Geneva, with honorary positions in Lausanne and at the Sorbonne in Paris.

The first book written by Jean Piaget, published in 1923 when he was 27 years old, was entitled *Le langage et la pensée chez l'enfant* ("The language and thought of the child"). It was an innovative, fresh approach to children's language behavior. Here was the biologist running systematic ethological observations on the spontaneous speech of 4–7-year-old

[39] This "sentence-precedes-constituents" notion was inspired by Pick (1913), who subscribed to it in his stage theory of language production (see Chapter 11). Fröschels makes due reference to him.

[40] The example had been Wundt's in *Die Sprache.* Dittrich keeps surprising you. The paper begins with a page-long citation from Fröschel's two-page preface to the same book. It further contains a footnote (p. 142) with bibliographical references to all of Dittrich's own publications, surreptitiously accusing Arnold Pick of 'promiscuously presenting my theory.'

children in a classroom. This new paradigm immediately appealed to students of language acquisition, both in Europe and America, especially after English and Russian translations of the book appeared. Curiously enough, this monograph was at the same time the *last* book Piaget wrote on the language of children. Piaget's later groundbreaking contributions to the study of child development were in the first instance epistemological. How do the child's conceptions of the physical environment develop from the initial sensorimotor stage? How does the child, interacting with the world, construct representations of objects, physical causality, movement, space, geometry, and number? How does the child acquire the elementary operations of logical reasoning in these cognitive domains? This became the core of Piaget's grand approach to genetic epistemology. In Piaget's conception, language had only a secondary role to play. It was never *causative* in the child's construction of reality. The child can, in principle, acquire his logical operations without command of language. Language is rather one of the outcomes of epistemological development. It emerges on the basis of general mechanisms, not specific ones. The child's growing semiotic functions allow him to increasingly represent absent things, to operate on entities that are not in sensorimotor reach. That is cognitive growth. Language emerges from there. It eventually becomes a "tool of intelligence," with representation (Bühler's *Darstellung*) as its main function.

Piaget's former student Karmiloff-Smith (1979) clearly sketched Piaget's limited approach to language. There were in particular two aspects of linguistic development that Piaget left untouched in his later work. First, Piaget never approached language the way he approached the child's construction of physical reality, namely as a problem space *per se*, as a cognitive domain with its own characteristic rules, structures, functions—all to be discovered in attentive interaction with the linguistic environment. Second, Piaget, in his later work, did not go beyond considering the representative function of language. Language as a tool for communication and social action was not on his agenda, neither was the child's construction of social, inter-human reality. It was not until the late 1960s, in particular the work of Hermine Sinclair-de Zwart (1919–1997), that language was given its proper place in the Piagetian framework. That development is, however, beyond the scope of this book.

Piaget's *The language and thought of the child* remarkably focused almost exclusively on the expressive and appeal functions of children's spontaneous speech. There was very little concern with what and how the children were representing by means of their language. Piaget's first subjects were two 6-year-old boys, Pie and Lev, in the Maison des Petits of the Institut Rousseau. What are these children using language for? Each child was observed for a month by a dedicated observer. The two observers wrote down everything their child said. The children were observed while playing freely in the classroom with other children. By the end of the month 1500 of Lev's utterances had been collected and 1400 of Pie's. Each utterance was then scored on two main categories: *egocentric* and *socialized.* Utterances in the first category are not directed towards anybody in particular. Among them are random repeats (a teacher explains the term *celluloid* to some child and Lev utters *Luloid, le le loid. . .*), monologues (for instance, the child talking to himself while drawing), and "collective" monologues, where the presence of others is just assumed

(Lev, sitting with others at a table: *I say, I am the captain on horseback*, where *I say* supposes the others to be listening, although nobody in particular is addressed). Socialized utterances do appeal to others; they function to influence an interlocutor in some way or another. Among them are, in Piaget's own terms, "adapted information," where one child helps or instructs another child, "criticism and derision," and "commands, requests and threats." A final socialized category is questioning.

The initial statistics based on these observations show that 43% of Pie's and 47% of Lev's utterances were "egocentric." In a follow-up study the same type of data were collected from a group of 20 children ranging from 4 to 7 years old. For each child a sample of 100 utterances were collected. On average, these children exhibited 45% egocentric utterances, confirming the earlier findings from Pie and Lev. Piaget further analyzed a rather small subset of conversations among children. It is, as it were, the first attempt at conversational analysis in children. From this analysis, Piaget suggested that children successively acquire three conversational types. The first is the "collective monologue" (with the presence of others assumed), which can occur in the form of a quarrel (33% of conversations). The next type to emerge is interactive (i.e., "adapted"), either in the form of helpful collaboration in action or exchanging "primitive" arguments (63%). Finally, there is the stage, rarely reached by children in this age range, of "collaboration in abstract thought" or using genuine argument (3%).

Piaget then reports on experiments in children's understanding of texts. If an adult tells a child a story, the 6–8-year-old child's reproduction is not too bad; on average 70% of a story's content is correctly reproduced, according to Piaget. But if another same-age child tells the test child a story, reproduction drops to 48%. Similar numbers appear if a same-age child explains the working of a pictured device to the test child. The latter is often unable to reproduce such an explanation.

How can this failure of communication be explained? According to Piaget, the thought and speech of children under 7 years old is largely "egocentric," which means that children do not differentiate between their own perspective and the perspective of others. It is an "epistemic attitude." Children in this age range are not aware of the relativity of their own point of view. We would now say they have no theory of mind. The child does not take the listener's perspective when telling a story or explaining a device. It is a state of ignorance, according to Piaget. Piaget turns what had started out as an "objective" scoring category, namely "egocentrism," into a theoretical epistemological concept.

Piaget's term created much misunderstanding. In the third (1959) edition of the book, Piaget added a chapter in an attempt to forestall misconceptions such as the notion that the child was selfish or was not socially adapted. Two features characterize egocentric speech, Piaget explains. First, egocentric speech does not have the purpose of influencing the other; the other's verbal or non-verbal response is irrelevant. Second, egocentric speech does not differentiate between the own and the hearer's point of view. It is only after the age of 7 that children acquire "uncentering" themselves.

Piaget's new observational paradigm, which he called "clinical," was quickly adopted and adapted by others, both in Europe and the United States. Here was a new, non-diary

approach to the functional study of speech development in the child.[41] We will shortly discuss its (modified) application by McCarthy (1930) and many other followers for securing statistical norms of language development. In this context a rather exceptional experimental study should be mentioned, which was closer to Piaget's epistemological objectives. Grigsby (1932) tested 83 children in the age range of 2;8 to 6;4 on their conceptual development, in a high-tech experimental setting. It was probably the first study in which children's speech was electrically recorded. Each child was individually tested using a series of questions for conceptual development in six different domains. For instance:

Time: "Is this morning or afternoon?"

Causative: "Why does a ball roll?" or "Why does an airplane stay up in the air?"

Discordance: "You get your clothes muddy, in spite of . . ."

Number: "How many marbles?"

and so on.

The child's responses were recorded by microphone and transmitted via a telephone line to the laboratory a few blocks away. There the speech signal was played on loudspeakers and recorded on a "standard cylindrical wax dictaphone" (using two of them, to keep recording when one cylinder was almost full). Finally, a typist transcribed all recorded speech. The data allowed Grigsby to formulate phases of conceptual development. For causality, for instance, children's accounts begin by providing volitional or intentional explanations ("I want the plane to stay up"), then develop to just mentioning the order of events, and finally to real causal explanations.

In the following we will see several responses to Piaget's "egocentrism" claim. One of them is Vygotsky's.

Lev Semenovich Vygotsky

Vygotsky's impact on psycholinguistics outside of the Soviet Union was quite limited during the period considered in this book. There were two reasons for this. The first was his untimely death. Vygotsky was born the same year (1896) as Jean Piaget, but died (from tuberculosis) in 1934. His contributions to psychology had been largely theoretical, particularly for his predominantly "genetic" work in the psychology of language. Vygotsky used the term "genetic" in the same sense as it had been used in the nineteenth-century psychology of language, meaning both "ontogenetic" and "phylogenetic." His main publication in this area, *Thinking and speaking,* appeared a few months after his death, but remained untranslated in English till 1962. The book is an effort to integrate a set of earlier studies into a coherent text, but shows signs of haste. The second reason for his limited influence was that, although he was a convinced Marxist, Vygotsky soon did not fit the developing Stalinist party line of the 1930s. Denying the feasibility of a psychology

[41] Although it did not replace the diary method. Jean Piaget and his wife Valentine Chatenay kept extensive diaries on their three children, Jacqueline, Lucienne, and Laurent.

without consciousness, he was at odds with the materialist philosophy. Stalin only allowed a Pavlovian brand of psychology and banned Vygotsky's "paedology," even in the context of educational testing. Only after Stalin's death in 1953 was there a slow Vygotsky revival in the Soviet Union, which went on to become an all-out fad in the United States. In 1962, an excellently edited English rendering of the 1934 book, entitled *Thought and language*, appeared in the United States, and this is our first focus here.

Vygotsky had studied medicine and law at Moscow University, but his heart was in philosophy and psychology. He was a self-made man in these fields, absorbing both Russian and international literature. Vygotsky's absorbing of this literature often took the form of critically commenting on it. That is also the style of his *Thought and language*. It is sharp, analytic, but also apodictic.

Vygotsky's approach to language and thought is genetic (just as it was for Wundt). To understand thought and language in the human mind one has to consider their development, in evolution, in human culture, and in the child. Genetically, thought and language have different roots. Köhler had demonstrated in his tool-using experiments with chimpanzees, that there can be thoughtful tool-use without language. The same holds for pre-speech children, as Karl Bühler demonstrated by repeating Köhler-type experiments with babies. These thinking abilities are genetically independent of the substantial vocal communication abilities of chimps, many other animals, and, preeminently, babies. Vygotsky referred to Charlotte Bühler's experiments (to be discussed) showing babies' explicit reactions to the human voice when they are 3 weeks old. These vocal communications, however, initially do not serve thought or tool use. They are emotional, expressive forms of social communication. It is only late in the child's second year that these two genetic lines come to cross, language becoming a medium for thought, a system of meaningful signs, "words" for Vygotsky.

Vygotsky also discusses Piaget's 1923 book quite critically. He rejects Piaget's idea that the child's overt monologues are just an expression of egocentrism, bound to disappear as soon as the child learns to de-center his perspective with the acquisition of "sustained social life" after age 7. Such monologues do not disappear, Vygotsky claims, but go "underground." Monologue gone underground is nothing other than *inner speech*, which keeps running, day in day out, in the adult's mind. Both the child's "egocentric" monologue and the adult's inner speech can serve thought, imagining things absent, planning actions, and so on.

Piaget did not see Vygotsky's criticism until the early 1960s, but he did respond[42] to a remark Luria, Vygotsky's dear friend and colleague, made on the same issue. Luria, Piaget admits, is right in equating the functions of egocentric monologue and inner speech, but the two differ structurally. The child projects his monologue onto the real world of objects and entities around him. We do not do that in inner speech. Instead we definitely experience it as subjective. Ultimately, Piaget (1962) did react to Vygotsky's original text, which

[42] In the fifth (1959) edition of his book, p. 263, Piaget refers to a Russian paper by Luria, but Vygotsky and Luria (1929) had similar comments in the Proceedings of the 1929 International Congress of Psychology at Yale, where Piaget also had a paper.

had become available by then. There is full agreement now that the child's monologues are functionally like the adult's inner speech. Both can also serve more ego-related or more communicative purposes. Still, they are not "egocentric" in the same sense. There is no rational (or epistemic) *reciprocity* in the child's monological speech. There is no decentering of perspective, whereas shifts of perspective are quite normal in inner speech. Piaget's response is conciliatory and elegant, but it cannot hide a deep divide. What Vygotsky and Luria had been stressing is that a major function of both "egocentric monologue" and inner speech is one of verbal planning, a "specific organizing function."[43] Their empirical evidence was that the rate of "egocentric speech" went up sharply when "the child was confronted with a difficult situation" (p. 456). Speech content showed the effort to solve the problem. Piaget, however, could not accept the notion that language is a tool for thought already for the young child.

One further contribution of Vygotsky should be mentioned. He initiated, together with his co-workers, the experimental study of children's language acquisition. But it was only after Stalin's death in 1953 that this approach really took off. Luria (1961) describes some of this work. Much of it concerns the question of how speech can affect the child's organization of perception and action. There are, for instance, the experiments by Martsinovskaya and Abramyan. You can train a child over 3 years old to squeeze a balloon in his left hand when seeing a red disk on a gray background, and to squeeze one in his right hand when presented with a green disk on a yellow background. Control experiments show that the child is only attending to the color of the discs—the backgrounds are irrelevant. But you can make them relevant by means of verbal instruction. Just instruct the child to squeeze in response to the background colors; children aged 5–7 easily pick that up, but not the younger ones. However, the younger ones can be taught the background response by making the stimuli more "meaningful." This is done by replacing the colored discs with colored airplanes and giving the instruction to squeeze the right-hand balloon when the plane's background is yellow, because "the plane can fly when the sun is shining and the sky is yellow," and to squeeze with the left hand when the ground is gray, because "when it's rainy the plane can't fly and has to be stopped." This worked fine; it demonstrates the "rule of force" exerted by speech associations, the "second signaling system" on the perceptual organization, according to Luria. It is hard to evaluate these studies from Luria's concise descriptions. At any rate, they did not contribute to the late 1950s emergence of an experimental approach in the United States.

Elemér Kenyeres

A classic Hungarian diary study is that of Kenyeres (1926). Elemér Kenyeres (1891–1933) was trained as a pedagogue and language teacher. On receiving his diploma in 1914, he volunteered for military service and was repeatedly decorated for his courage. After the war he obtained a PhD in education at Budapest University and became professor at the École normale in Budapest. He also spent a post-doctoral year (1926–27) in Geneva. Kenyeres' diary reports on his daughter Eva's language acquisition from birth (in 1919)

[43] See Vygotsky (1934) and especially Vygotsky and Luria (1929, p. 456).

to the age of about 7 years. Kenyeres (1927) provides an account IN French of Eva's first words. It carefully relates the stages of Eva's development to the Sterns' (and others') data and discusses their relevance for the ongoing discussion on the functions of first words. Kenyeres rejects, for instance, Meumann's position that first words do not express concepts, but desires exclusively. He rather aligns with the Sterns[44] in that first words can have both volitional and "objective" functions. Kenyeres reviews Eva's acquisition of grammatical categories, such as plurality, person, possession, case, etc. Kenyeres, like Gheorgov, but in a more explanatory way, pleads for a comparative, cross-linguistic approach to the analysis of developmental stages. Eva, for example, uses the accusative already at 1;3, which is 7 months earlier than the appearance of the accusative in the Stern diaries. Kenyeres provides the obvious explanation: forming the accusative is quite simple in Hungarian, just adding *t* to the relevant noun. This is how Kenyeres formulates the comparative principle: "The grammatical forms of different languages differ much with respect to the ease of their realization, as well as to their value, their use and their frequency. The appearance of such forms will therefore not necessarily coincide for two children at the same stage of development, but speaking different languages." It nicely foreshadows the modern cross-linguistic approach to language acquisition.[45]

David and Rosa Katz

Both Gestalt psychologist David Katz (1884–1953) and Ukranian-Russian Rosa Katz-Heine (1885–1976) had been doctoral students of G.E. Müller in Göttingen. They married in 1919 and went to the University of Rostock the same year, where they stayed till 1933, when they were dismissed from their positions. They had two sons, Theodor born in 1920 and Julius born in 1922. The special legacy of the parents to developmental psychology was their book *Gespräche mit Kindern* ("Conversations with children"), published in 1928. It is an extensively annotated data base of 154 conversations they had with their children during one year, from 1925 to 1926. They felt strongly that Piaget's data needed supplementation with conversations between children and adults. Most conversations were relaxed bedtime exchanges. Here is an example (and note that in German *Frau* means both "woman" and "wife"):

Julius: *I want to be Daddy and have a motor cycle.*
Mother: *If you want to be Daddy you will also need a Mommy.*
Julius: *I have you.*
Mother: *No, you will need a wife, I am Daddy's wife.*
Julius: *I don't want a strange woman.*
Theodor: *But then she isn't a strange woman.*
Mother: *Bubi* [i.e., *Theodor*] *do you want to have a wife?*
Theodor: *No.*[46]

[44] Or rather with "M. Stern."

[45] Pioneered by Slobin and colleagues (Slobin 1985–1997).

[46] J. Ich will Papi sein und ein Motorrad haben. M. Wenn du Papi sein willst, mußt du auch eine Mami haben. J. Ich habe dich. M. Nein, du mußt eine Frau haben, ich bin doch Papis Frau. J. Ich will keine

Their data deviate strongly from Piaget's. "We go so far as to claim that the utterances children produce in the presence of their parents, hardly ever show egocentric character; that even holds for utterances in the stage of one-word sentences."[47] In addition, when the two boys played together, their utterances were dominantly non-egocentric, even when the youngest was only 3 years old. Egocentricity only appears when they are together with less familiar children. Piaget (1959, p. 240) acknowledged these findings without noteworthy comment.

Gespräche mit Kindern concerns many aspects of child development, but is regrettably quite limited on aspects of speech and language. The analysis of question types is interesting, however. Why-questions abound, confirming earlier work (such as the Sterns'), but the boys also often checked whether parents or friends know what they knew themselves, for instance whether a new friend knows their father's name. The authors do not comment that this involves some degree of perspective shift.

Yosikazu Ohwaki

A remarkable paper was published in 1933 by Japanese psychologist Yosikazu Ohwaki (1897–1976), professor at Tohoku University in Sendai, Japan. Ohwaki spent two years (1929–1931) with Narziß Ach in Göttingen. He published widely in psychophysics and visual perception. The 1933 paper, however, is a diary report and detailed analysis of the acquisition of Japanese by his two daughters, Yoriko and Sonoko, born in 1926 and 1928. Language acquisition in both children was followed up to the age of 1;10. Ohwaki reviews earlier diary analyses of Japanese researchers Ishikawa, Kubo, and Kido, from as early as 1910 (Ishikawa). Ohwake's analyses take their orientation from Stern and Stern (1928). Like the Sterns, Ohwaki admits to not being sophisticated in phonetics. His analyses of the children's babble period are, unavoidably, limited, but nevertheless focused. The core issue he covers is whether babbling shows any language-specific features. For that purpose he compares reports on babbling from the literature (Preyer on German, Hoyer and Hoyer on Russian) with the production of his two daughters. His main conclusion is that "with respect to the stages and the essential structure [of babbling] there is hardly any essential difference between the babbling monologues of children of different human races."[48] Still, he observed one noticeable distinction between his Japanese subjects and Indo-European ones. His children produced a preponderance of *ch* and *j* sounds (and this had also been the case in Kubo's observations), whereas *d* and *r* are dominant in the published Indo-European studies. One remarkable aspect of Ohwaki's paper is the

fremde Frau. T. Dann ist sie aber keine fremde Frau. M. Bubichen, willst du eine Frau haben? T. Nein. (Katz and Katz 1928, p. 46).

[47] "Wir gehen so weit, zu behaupten, daß die Äußerungen, die ein Kind in Anwesenheit der Eltern hören läßt, so gut wie nie egozentrischen Charakter tragen, das gilt selbst schon für Äußerungen im Stadium der Einwortsätze mit ihrem affektiv-volitionalen Character" (ibid. p. 4).

[48] "Nach dieser Gegenüberstellung lässt sich wohl annehmen, dass hinsichtlich der Termine und der wesentlichen Struktur ein grundlegender Unterschied bezüglich der Lallmonologen zwischen Kindern verschiedener Menschenrassen kaum besteht." (Ohwaki 1933, pp. 85–86).

reported speed of syntactic acquisition of these two children. The size of their vocabularies at 1;10 is 72 and 128 words respectively, which is in the normal range. But two-word sentences appear as early as 1;4 and 1;3 (for instance *gâgâ énén*–"ride in buggy"). Past and future tense marking arise at 1;8 and the same holds for multi-word question sentences. Ohwaki applies the Sterns' so-called "threshold" analysis to the emergence of different parts of speech. Their order of appearance is essentially the same for the Japanese and German children, with one noticeable exception. For the Stern children adjective-like words appeared some eight months after verb-like words. For the Japanese children they appeared at about the same moment. Why was this? Here Ohwaki refers to analyses of Japanese linguists, who have argued that Japanese adjectives behave very much like verbs. They are essentially "inflection" words, with predicative power like verbs and similar syntactic properties.

A final analysis concerns the intentions expressed by the children's early utterances. They range from strongly volitional, where the addressee is supposed to act (appeals), to "situational," where the child comments on his situational experience, as in narrations and play comments. The statistics on both children show that appeals dominate at 1;3 and 1;4, but give way to "situationals," which strongly dominate at 1;10. Ohwaki very politely disagrees with Piaget; his children did not get lost in egocentric monologues.

Ovide Decroly

An excellent French language textbook on child language development appeared in 1934. Its author, Ovide Decroly (1871–1932), a doctor, became Belgian's leading educational psychologist. As a professor of child psychology at Brussels University he had been an active, charismatic, and successful innovator of education, while at the same time keeping close to the laboratory. A wasting disease prevented him from completing his two-volume text. His wife, together with a dedicated former student, added the finishing touches. The book provides a comprehensive and sharp review of the literature. The first volume deals with the stages of acquisition from the first cries of the infant to the acquisition of phrases; the second volume reviews the growth of vocabulary. Especially useful is Decroly's discussion of speech/language comprehension, a much neglected topic in child language development. He was among the first to study neonate sound perception by means of film. A 2-hour-old baby, for instance, reacted violently to loud noise. There are expert reviews of the habituation-to-voice experiments by Charlotte Bühler and her colleagues, and of the perceptual implications of imitation data and another unusual section is on bilingualism. It is probably the first comprehensive review of the rapidly increasing literature on the advantages and disadvantages of early bilingualism. It became and would stay a hot topic, to which we will return in the section on bilingualism.

The Institutes of Child Welfare

Increasingly, language acquisition research in the United States became focused on securing normative data. How does the normal, average child develop from the babbling stage to the skilled language user at school age? This was often part and parcel of more encompassing developmental studies, such as that of Arnold Gesell (1880–1961) at

Yale University on developmental schedules. An obvious further topic of study was to investigate how children of different categories deviate from these norms. The contribution of factors such as sex, socio-economic class, bilingualism, intelligence, and perceptual handicaps became systematic topics of research. Educational applications came next. The Laura Spelman Rockefeller Memorial Fund established, during the 1920s, no fewer than six Institutes of Child Welfare. Among them were institutes at the universities of Iowa and Minnesota. Both got deeply involved with normative studies of language development. At Iowa, the home university of Albert Weiss and of Leonard Bloomfield (till 1927), Madorah Smith (1887–1965) did her PhD research, published in 1926, on the sentence and vocabulary development of some 273 children, to which we will presently return. She went on to test the effects of bilingualism on language development.

The Minnesota Child Welfare Institute was directed by John E. Anderson. There Dorothea McCarthy (1906–1974), in her 1930 dissertation, pioneered carefully balanced, and at the same time quite practical, norm setting studies of word, sentence, and functional development. Her intention was to move the study of language acquisition beyond the diary approach. "While the body of [diary] material is stimulating and suggestive, it is of little scientific value, for these isolated observations on children have been recorded by different methods, the records have been taken under different conditions, and most of them are subject to the unreliability of the parents' reports" (McCarthy 1931/33, p. 330). The introductory section of her dissertation laments the lack of attention paid to language by the average psychologist or psychology textbook, which is despicable given "the important rôle of language" in the civilized world.[49] In her dissertation study she took inspiration from Piaget's approach, but rejected both his methodology ("not adequate for scientific purposes," p. 23) and his conclusions on egocentrism. She transformed Piaget's approach into a well-standardized procedure. Children (age range 18–54 months, 20 children from seven age groups each) were visited at home and presented with toys and/or picture books. They were observed till they had produced 50 utterances, which took, on average, some 20 minutes. A child's sample of (transcribed) utterances was then scored on a host of "constructional" variables, such as number of different words, their part-of-speech roles in the utterance, utterance length, and a variety of syntactic measures (such as completeness, presence or omission of different types of parts of speech, phrases and clauses). In addition, a carefully modified version of Piaget's functional categories was used to score the utterances, with egocentric and socialized speech as main categories. The study set new standards for establishing developmental norms in language development.

Three further dissertations from the same Minnesota institute applied essentially the same procedure. The first was a study by Ella Day (1932) on the language development of twins (identical, fraternal, same and different sex) in the same age range as McCarthy's study. The main finding was that twins were markedly retarded on just about

[49] Remarkably she writes: "Another fact that brings out the important rôle of language is the meager communication and corresponding dearth of culture and civilization among primitive peoples. The greatest contrast in intellectual development between them and the civilized world is essentially a matter of language." (McCarthy 1930, p. 2).

every language measure and that identical twins showed much higher correlations on these measures than fraternal twins. The second study, by Edith Davis (1937), applied McCarthy's procedure to older twins (80 pairs—imagine the logistics of organizing such a stratified sample!), plus as controls children with siblings and only children, making a total of 436 children. A main purpose of the study was to check whether the retardation of twins would disappear with age. Hence, a subsequent age range, from 5;6 to 9;6, was chosen. Indeed retardation diminished over age, but it did not entirely disappear. Davis also scored the quality of children's articulation. Here also twins did worse than the controls. The third study was by a 23-year-old student of Skinner at Minnesota, John Carroll. For his 1939 paper in *Child Development*, Carroll used the records collected by Goodenough (1938), who had used McCarthy's method to study the development of pronoun use. Carroll analyzed the use of pronouns, determiners, and adjectives for the age range from 2;6 to 4;6. One of his findings was a relative increase in definite determiners (especially *the*) over indefinite ones (*a*). There is a dutiful but minimal Skinnerian interpretation of these findings.

The Minnesota studies are surprisingly a-theoretical. The only bit of explanation I came across was the suggestion by Davis that the retardation of twins may be caused by their low-level language model, namely the other twin. Still, McCarthy had defined a new, objective approach to testing productive language abilities in children. It was definite progress. In the year following her dissertation McCarthy wrote a chapter on language development in Murchison's *Handbook of child psychology.* It provides an excellent window on the state of the art from the American perspective. She wrote with an open mind about the historical roots of developmental psycholinguistics and her approach was by no means narrowly behavioristic, but that chapter also reflects her predilection for objective measurement over developmental theory. In 1932 McCarthy became a professor of educational psychology at Fordham University, New York, where she stayed till the end of her academic career in 1971. One of her major achievements there was the construction of the widely used McCarthy Scales of Children's Abilities.

Michael Morris Lewis and his sources

Michael Lewis's first book, *Infant speech* (1936), became a much cited treatise on infant's speech, in particular phonetic development over the first three years of life. The book is based on Lewis's phonetics dissertation at the University of London. After London, Michael Lewis (1898–1971) moved to the University of Nottingham, where he stayed till his retirement in 1963 as director of the Institute of Education, with one interruption: from 1940 to 1947 when he acted as vice-principal at Goldsmith's College in London. *Infant speech* was followed by several further monographs on language, thought, and personality development in children, including children with learning disabilities and, especially, deaf children. *Infant speech* was, for a long time, the most thorough review of phonetic development in infants, positioning it in the wider context of the child's affective and functional development. Lewis's main thesis was that there is phonetic and functional continuity from the infant's first expressive cries to the sound form of the child's first "conventional" words. This was by no means the standard view. For Wundt, the

baby's first cries are natural, affective expressions, whereas first speech sounds are imitative. Karl Bühler further sharpened the divide. It is the principle of phonological oppositions which governs the child's acquisition of speech sounds, whereas first cries are non-linguistic and purely expressive. Lewis followed the infant's development step by step, time and again testing his developmental hypotheses on appropriate data sets. Only a few diary studies provided enough phonetic and situational detail for his purposes. The Sterns' data on Hilde formed a main source. Deville's phonetic transcriptions were also useful, as were some of Preyer's, Ament's, Meumann's, and Guillaume's data. Importantly, Lewis added a database of his own on a boy "K," which was rendered in phonetic script. We will return to Lewis's analyses in the section on sound development.

Antoine Grégoire

The Belgian linguist Antoine Grégoire (1871–1955), professor at the University of Liège and founder of its phonetics laboratory, was seemingly a late representative of the "diary wave," which had emerged from Taine's and Darwin's 1877 papers in *Mind*. The two volumes of Grégoire's diary appeared in 1937 and then, after the war, in 1947. However, the diary data themselves were recorded quite early in the century. They concern Grégoire's two sons, Charles and Edmond, born in 1903 and 1905, respectively, i.e., they were contemporaries of the Stern children. It is remarkable that these publications appeared some 30–40 years after the period of observation. Volume I covers the children's first two years, Volume II is largely about their third year. Not surprisingly, Volume I is almost exclusively about the infants' first vocal productions and Grégoire's approach here is thoroughly phonetic. It is sheer coincidence that Lewis's *Infant speech*, covering the same infant period and the same topic, had appeared a year before. Grégoire acknowledges that in his bibliographical note, remarking that Lewis devoted a large part of his book to the earlier psychological theories on language acquisition. But then he adds that, "regrettably," Lewis "hardly contributes to satisfying the desiderata that we will be forced to express on several occasions." That was his last word in both volumes on Lewis and we are left in the dark about his misgivings. More generally, Grégoire is quite sparing with references to the English language literature. We will return to Grégoire's work in the section on sound development.

Roman Jakobson

In the previous chapter, we met Roman Jakobson (1896–1982, Fig. 10.3) as co-founder, together with Mathesius and Trubetskoy, of the Prague Linguistics Circle. Jakobson, born in Moscow, left Russia in 1920 (initially as a member of the Soviet Embassy in Czechoslovakia), joining other Russian emigrants in Prague. He took his PhD there in 1930. When the Nazis invaded Czechoslovakia in 1939, Jakobson, being of Jewish descent, left Prague for "years of homeless wandering from one country to another," as he described it. He first went to Copenhagen and then on to neutral Sweden. From there he went to New York in 1941, after finishing the brilliant book we will consider in this chapter.[50]

[50] On the same ship as Ernst Cassirer, see Chapter 14.

Fig. 10.3 Roman Jakobson. © Private collection/Archives Charmet/The Bridgeman Art Library.

Initially he took teaching positions in New York, but then, in 1949, he was awarded the chair for Slavonic languages at Harvard, followed by an MIT professorship in 1957. We will return to Jakobson's career in Chapter 14.

Jakobson's major contribution to psycholinguistics was in the area of phonology. Both Russian expatriates, Jakobson and Trubetskoy, were quite familiar with Baudouin de Courtenay's work in the linguistics of sound, which was partly published in Russian. That work became foundational to structuralism, as discussed in Chapter 7, largely mediated by the Prague Circle. Baudouin de Courtenay had substantially elaborated Saussure's notion of "phoneme" as an abstract mental representation of the speech sound, subject to regular "allophonic variation" (for instance phoneme */d/* being realized as *d* in German *Rades* and as *t* in German *Rad*). He had also developed, in great detail, a theory of articulatory features, the way in which speech sounds vary along dimensions such as place and manner of articulation. But he had stopped short of developing the notion of "distinctive feature." Of all the variations in speech sounds only some are "distinctive" in a given language, for instance "voicing" in English, which realizes the functional distinction between words such as *tell/dell* or *power/bower*. This notion was fully apparent in Bloomfield's *Language* of 1935.[51] The "distinctive feature" also became a core notion in the phonology of Trubetskoy and Jakobson, who developed it in much more formal detail. Each language has a small, finite set of binary distinctive features and phonemes are bundles of such features.

In a work of genius, *Kindersprache, Aphasie und allgemeine Lautgesetze* ("Child language, aphasia and phonological universals"), published in 1941, Jakobson used the new

[51] "The difference between distinctive and non-distinctive features of sound lies entirely in the habit of the speakers. A feature that is distinctive in one language may be non-distinctive in another language. . . . we can recognize the distinctive features of an utterance only when we know the meaning, we cannot identify them on the plane of pure phonetics." Here Bloomfield works out the distinctive difference between *man* and *men* (Bloomfield 1935, p. 77).

phonology to unify three empirical domains. The first, which is our main concern in this chapter, is the child's acquisition of speech sounds. The second is the aphasic patient's loss of phonological skill, which is discussed in the next chapter. The third is the "typology" of sound structure; languages differ substantially, but also systematically, in their phonological repertoire. You wonder how he managed, while being on the move from the Nazis, to make such detailed references to a rich, multilingual literature[52] in child language and aphasia. We will discuss Jakobson's applications to the child's phonological development in the section on sound development. Chapter 11 will return to the applications in aphasiology.

The initial reviews of Jakobson's book were positive, but missed the "classic" nature of the work. Werner Leopold's (1942) review in *Language*, for instance, states (p. 252): "My fundamental conviction, however, that we have not reached the stage when trustworthy generalizations concerning children's language learning can be made is not shaken by this book," although "this should not distract from the fundamental merits of this book." But only a few years later Leopold (1947, p. 262) writes: "Jakobson's study is admirably suited to become a stepping-stone leading forward to more valid generalizations on child language than have prematurely been attempted before." History has made this true; Jakobson's slender book, in particular its English translation, became one of the most cited books from the period we are considering—together with the Sterns' *Kindersprache.* The reason was not so much that Jakobson was right, but rather that he had presented a new, challenging theory. This was no luxury. The child language literature had become "objective" in the course of the twentieth century. In psychological and educational circles this meant large-scale statistical studies, with limited (mostly behavioristic) theory. In linguistic circles being "objective" meant rich, minute phonetic detail and "postponement" of theoretic interpretation. Jakobson defied this approach and put explanatory theory back on the agenda, as we will see.

Aleksandr Gvozdev and Werner Leopold

The last great diary studies of the period we are considering were Gvozdev's (1948, 1949) and Leopold's (1939–1949). Both were deeply, although not exclusively, concerned with sound development.

Aleksandr Nikolaevich Gvozdev (1892–1959) was professor of Russian linguistics at the University of Kuybyshev (now the State University of Samara). Gvozdev followed his son Zhenya's language development over a period of nine years (1921–1929). He published two books on the analyses of his extensive material: *Children's acquisition of the phonetic component of the Russian language* (1948) and *The child's formation of the grammatical structure of the Russian language* (1949),[53] both presenting the corpus "in several

[52] Even to the Dutch diary data, which van Ginneken had used in his previously mentioned school book.

[53] A digitized version of the two diaries is available by Magdalena Smoczyńska at: http://www.universite.ru/material/works/filolog/gvoz/gvozdev.html.

cross-cutting topical arrangements, with continuing intensive and insightful analysis of the material."[54] Gvozdev used phonetic notation throughout. The two books were republished together, along with two earlier papers, in Gvozdev (1961). That book was, sadly, dedicated "To the bright memory of my son Zhenya, fallen on the front in the Great Fatherland War [World War II]." We will return to Gvozdev's work in the section on sound development.[55]

Werner Leopold (1896–1984) was a native German (although born in London) with a (German) degree in English literature. In 1934 he became professor of German at the University of Wisconsin and moved eventually to Northwestern University. His diary study, published in four volumes from 1939 to 1949, largely concerns the language development of his daughter Hildegard, who grew up as an English-German bilingual. She was not a "balanced" bilingual, however: her German never became "native," unlike her English. Leopold's diary records are all in phonetic (IPA) script. The four volumes provide conveniently arranged overviews and tabulations of the diary data, a noticeable advantage over Grégoire's presentation. Volume I deals with the developing vocabulary over the first two years. It not only lists the successive word acquisitions in both languages, but also their "mortality," i.e. their disappearance, before 2;0 (no less than 36%). Volume II is on sound development during the same period, Volume 3 on grammar and general issues. Volume 4, finally, presents the full diary data from later years. We will return to Leopold's work in the section on sound development. Here we should mention one more, and quite useful, contribution of Leopold to the study of language acquisition: his amazingly comprehensive *Bibliography of child language* of 1952. Slobin provided a new, augmented edition in 1972.

This completes the parade of scholars in language development from the Sterns early in the twentieth century to Gvozdev and Leopold in the 1940s. Most of these scholars kept and analyzed diaries of early language acquisition. Others, in particular members of the Institutes for Child Welfare, developed large-scale normative studies of language development. A major theoretical impetus was provided by Roman Jakobson's monograph. In retrospect, it was "cognitive revolution" in *statu nascendi.* Indeed, Jakobson's influence on the developments in the late 1950s and early 1960s was substantial.

Let us now turn to the three topical areas which drew most attention from these scholars. They are the development of vocabulary and utterance structure, early sound development, and the vicissitudes of child bilingualism.

The growth of vocabulary and utterance complexity

The growth of vocabulary in children has always been the most conspicuous phenomenon in developmental psycholinguistics. All diary studies since Tiedemann (1787) report on the children's early vocabularies. The Sterns (1907) provide statistics on Hilde and Günther, which they compared to the data from Deville, Preyer, and Major (1906).

[54] Slobin (1973), in Ferguson and Slobin (1973, p. 565).

[55] For his work on grammatical development see Slobin (1973a, pp. 565–583).

They mentioned two characteristics of this data, which got confirmed, time and again, in the decades that followed. The first is that the rate of acquisition differs greatly between children. Around the second birthday, for instance, Hilde had 275 words, Günter 50, Deville's daughter 668, and Wertheimer's daughter (discussed by Preyer) 435. The second is that rate of acquisition is at first quite slow, but then dramatically speeds up towards the end of the second year. This is the age at which the child makes "one of the most important discoveries of his whole life," namely "that everything has a name."[56] Indeed, most children soon reach the stage of *What's that?* questions. The precise nature of this discovery became a recurrent theme in the literature. Karl Bühler (1918a, p. 293) doubted whether this was, in its first manifestations, an expression of symbol awareness, of a incipient representational (*Darstellungs*) function. Instead, the child acquires the habit of uttering a specific word while attending to a specific object. When there is no such word around, he becomes conscious of a task (*Aufgabe*), for which he has a solution schema: ask *What's that?* and the adult will fill in the gap. This is still very much the Würzburg approach. Gestalt psychologist Koffka (1921, p. 232) suggested that, initially, children conceive of names as specific *properties* of entities. These properties are "detachable" from the perceptual Gestalt; you can say "mamma" in her absence. At the same time, they are *essential* properties. Here Koffka refers to the anthropological literature on word magic.

Back to the statistics. The first major tabulation of vocabulary statistics was done by Edwin Doran (1907) of Belfirino College, Mississippi. It was published in Stanley Hall's *Pedagogical Seminary.* The paper presents a rich collection of data for subjects of all ages and categories. It opens with a table of the available statistics on individual children, over a hundred of them, in the age range of 8 to 72 months. At the age of 2;0 the average number of different words used by these children is 456, ranging from 10 to 1121.[57]

All diary writers knew that it becomes just about impossible to keep track of the child's growing vocabulary. Still, heroic and often convincing attempts to collect complete vocabularies of individual children kept coming like a torrent. Here are a few outstanding examples. Gale and Gale (1902) published the vocabularies of three children at ages 2 and 3. The Sterns (1907) provided vocabularies for Hilde and Günther at different ages, coded for semantic categories. Pelsma (1910) reported the vocabulary development of his daughter Elisabeth at ages 1;0, 2;0, 3;0, and 4;0. The paper provides comparative data from 73 reported children of different ages and provides a good discussion of the forces that drive vocabulary growth (such as imitation, interjection, and invention). Bateman (1916) presented vocabularies and part-of-speech classification for three children at 28 months. Their average vocabulary size was 491. Bateman (1917) skimmed the

[56] "Es hat hier eine der wichtigsten Entdeckungen seines ganzes Lebens gemacht, dass zu jedem Gegenstand dauernd ein ihn symbolisierender, zur Bezeichnung und Mitteilung dienender Lautkomplex gehöre, *d.h. dass jedes Ding einen Namen* habe." (Stern and Stern 1907, p. 175).

[57] Reports of extreme cases kept coming. Rigg (1938), for instance, reported on a child that had precisely 13 words at that age. That fact was neither predictive of her vocabulary size at the age of 4;0, which with 1019 was within the normal range, nor her IQ at age 6, which was 139.

literature for reports of the very first word of infants. Nice (1917) presented full data on the vocabulary development of her daughter from 1;6 to 6;0, like the Sterns grouping them into semantic categories and coding them for part-of-speech. She also reviewed much of the earlier work. Nice (1920, 1925) extended this work by recording 1-hour conversations with a 4-year-old child (the procedure later used by Smith). This provided a more reliable basis for parts-of-speech analysis, because the syntactic roles of the words used could be inferred from the sentences in which they occurred. The data also provided the possibility of "determining" sentence complexity (in terms of words per sentence) and determining the development of sentence types, such as declaratives, interrogatives, negatives, etc. Bean (1932) reported in great detail on a blind child's speech development. At the age of 3 years, the child's vocabulary had 1149 words, very much in the normal range. The stages of the child's development were no different to those for hearing children and Bean could not refrain from drawing the larger picture: "Children, like primitive peoples, have at first not more than one pronoun and no distinctions of number, of gender, or of person." (p. 192).

Deußing's (1927) dissertation at Jena University provided statistics on parts-of-speech and sentence complexity in picture scene descriptions of 20 8–14-year-old children. He listed the relative frequencies of nouns, verbs, etc., but then divided these by the number of *different* nouns, verbs, etc., that is, the relative frequency of the average token in a word class. This demonstrated the very high frequency of function words, such as articles and conjunctions. Next frequent were verbs, nouns, and adjectives. Such statistics, we saw, were further developed by Smith and McCarthy. However, that was no longer based on studies of individual children or small samples, but on sizable group comparisons.

The major methodological innovation in the measurement of vocabulary size was to *test* children's (and adults') vocabularies. The basic procedure was, to the best of my knowledge, conceived by Kirkpatrick (1891). In a *Science* paper he reported estimating the size of his own vocabulary: "I first turned to Webster's unabridged dictionary (edition of 1870), and counted the words on every twenty-fifth page, and found the percentage of them whose meaning was known to me. Then by calculation I found that if the same percentage holds for the other pages I must know the meaning of nearly seventy thousand of the words given in that edition of the dictionary." Here the "sampling factor" was 25. Doran (1907) applied exactly the same procedure as an oral or written definition test: "I selected all the words on each page [of some standard dictionary] except such unusual words as none would know, and selected the pages either at random, or more often, in certain order, as every twenty-fifth or fiftieth page . . . The result was arrived at . . . by multiplying the average number of words defined on each page by the whole number of pages in the dictionary." He applied the written version of this method to sixth- to ninth-grade children in different schools (with rather hugely different results in different schools), but the bulk of the paper consists of case studies. Here is an example of a case study of a bilingual girl: "Pearl Beale, a little girl almost five years old (59 months), knew on 100 pages of the Student's Standard Dictionary 171 words, an average of 1.71 words per page. This, multiplied by 814, the number of pages in the book, gives 1,391 words.

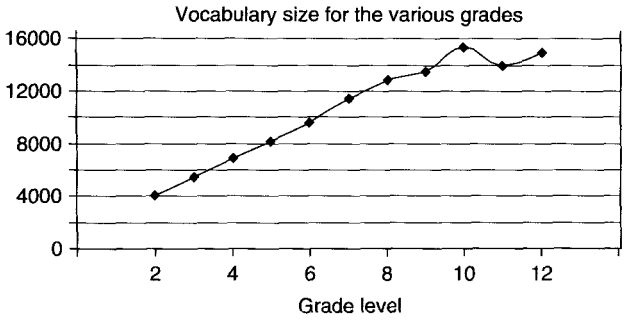


Fig. 10.4 The growth of vocabulary size in 2000 pupils, grades 2 to 12. Data from Brandenburg (1918).

While a very intelligent girl, the opportunities for securing a large vocabulary had not been good. She spoke German, but I did not test her German vocabulary." (p. 411).

Kirkpatrick's method was soon further improved. Terman and Childs (1912), in preparing the first version of Terman's famous intelligence test, had picked the last word of each sixth column in an 18,000 word dictionary, a total of 100 test words. Hence, the sampling factor was 180. Brandenburg (1918) of Perdue University composed the test list by picking each 140th word from Webster's Academic dictionary, a total of 200 test words. This written test list was presented to no fewer than 2000 pupils from grades 2 to 12 in 16 schools. Their task was: "In the blank space after each word you know, write a sentence using the word correctly. Place a cross before each word you do not know." Multiplying a child's number of appropriate responses by the sampling factor 140 gave an estimate of the child's vocabulary size. In Figure 10.4 I have plotted Brandenburg's average numbers for grades 2 to 12. Such graphs were to be published for decades to come.

Further refinements and variants followed. One much used procedure was introduced by Smith (1926). She took inspiration from the language test developed by Descoeudres (1921) in Geneva. Her test list consisted of every 20th word from Thorndike's (1921) list of the 10,000 most common words. Where possible, objects or pictures were presented whose names were words on the list and the child was asked to name them. In all other cases questions were asked which might elicit the word as an answer; if that did not work, the child was asked for a definition. This test could be applied to children over 2;0. By testing children in different age cohorts, up to 6;0 (a total of 273 children), Smith could establish age norms, which she presented in tables and graphs. Smith also studied these children's utterance complexity. For that purpose, she observed each child for an hour, roughly following Piaget's procedure, and recorded all their utterances. She then computed several measures, such as the number of sentences produced in the hour, the number of words per sentence and so on. She also coded parts of speech (i.e., nouns, verbs, adjectives, etc). She acknowledged Dewey's (1894) misgivings about this for the youngest children. Still, this became common practice in statistical studies (as it had been in the individual vocabulary studies, in spite of the warning of the oft-cited Sterns).

Williams (1937), also from the Iowa Institute, published a short version of the Smith test, reducing the number of words tested from 203 to 84.

Three years after Smith's monograph, a moment of alarm arose when a paper by Gillette (1929) was published. He discussed many of the problems in devising such tests. Some had been considered before. For instance, what do you do with a word's inflectional variants? Are *work* and *worked* different words? What about *go* and *went*? What about multiple meanings, for instance *green* meaning a color, but also "not ripe"? But the really disturbing fact was that he had tested himself twice, once with Terman's test (based on an 18,000 word dictionary) and once by a self-test based on Webster's Standard Unabridged dictionary (containing 450,000 words). His vocabulary size was estimated at 16,833 words[58] with the first test, but 127,800 with the second test. In other words, the estimates varied with the size of the dictionary. This is, of course, a trivial truth. If you happen to know *all* the words of your language, any test based on a sampling factor will estimate the size of your lexicon *exactly* at the size of the dictionary used. The same holds, *mutatis mutandis* for fractional knowledge. The best way of approaching this problem is to go for the largest lexicon you can find. That was, basically, what Seashore and Eckerson (1940) did in devising their test, which became the standard for many years to come. Their dictionary contained 371,000 words, a huge increase in comparison to Terman's test. This new test was used in 1941 by Mary Smith (NB *not* Madorah Smith) for testing pupils of grades 1–12. Grade 7 children (12-year-olds) had an average vocabulary size of about 55,000. Brandenburg's (1918) number had been 11,363, based on a 28,000-word dictionary. Dictionary size explains part, but not all, of these differences. Many other factors are involved, but this is enough for our historical purposes. Let us now turn to a last major publication in this area.

Templin's (1957) monograph completes, in the period we are considering, the work of the Minnesota Child Welfare Institute, which began with McCarthy's (1930) dissertation. Over a quarter century later, the approach had remained essentially the same: to secure statistical norms of language development. Even the outer appearances of the monographs are very similar. Mildred Templin (1913–2008) set out to sample data on four language skills, testing 480 children in the range 3–8 years old. She used an articulation test, which was a further development of Wellman *et al.*'s test (to be discussed). Children had to produce words with critical consonants, consonant blends, and vowels. There was also a new two-fold perception test. Children had to choose a picture that matched a presented word, and they had to respond "same" or "different" to a spoken pair of nonsense syllables. The children's vocabulary was tested using the Seashore–Eckerson scale and others. Finally, there was a "verbalization test." This was a variant of McCarthy's 50-utterance test. We will limit our attention to the results of the latter test, because it concerns utterance "complexity." Studies of utterance complexity in this tradition meant establishing norms for the development of sentence length and completeness, of sentence "constructions" (simple, compound, subordinate), of sentence types

[58] Curiously, this number is not a multiple of 180.

(declarative, interrogative, imperative, exclamatory), of phrase types (noun, verb, adjective, adverbial phrases or clauses) and of parts of speech (noun, verb, adjective, pronoun, etc.). Children's utterances were often also scored for "grammatical inaccuracies." Templin performed such intensive, detailed scoring for no less than 24,000 utterances collected in this mega study. The findings, presented in a host of tables, largely confirm the earlier findings of the Child Welfare Institute following from McCarthy (1930). The Institute's earlier vocabulary and speech sound data were also replicated. This is remarkable stability over a quarter century. There is one difference, however. Templin's pupils produced, at comparable ages, less simple and incomplete sentences, more complex constructions, and sentences of greater length. Their language was more "mature." Why would that be the case? Probably the increased talkativeness of these children reflects "an increased amount of adult language in the child's environment whether as a result of increased viewing of TV, more inclusion in family activities, general permissiveness towards the children's behavior, or other factors." (p. 151). That is probably the only theorizing in this book. It is characteristic for the entire major research endeavor of the Child Welfare Institutes: be objective, measure in representative age cohorts, establish developmental norms, and keep away from theorizing or hypothesis testing. This is a highly respectable aim, but dull.

Studies in speech sound development

Sound development has always, since Tiedemann (1787), been a topic in the study of child language. The different speech sounds are acquired successively. Hall (1896) had recorded this acquisition daily from day 1 to day 500. Children also "delete" speech sounds in their early words, and they "replace" target sounds by other ones. Schultze (1880) had formulated his law of least effort: easier sounds are acquired earlier and are likely to replace more difficult sounds. He defined, on physiological grounds, an order of difficulty. Tracy (1894) derived an order of difficulty from his data, partly confirming Schulze. Explanations had been proposed (by Wundt, the Sterns, and others) for assimilations in children's early speech. Progressive assimilations, such as in *tot* for *topf*, would be due to inertia in the generation of sound images (i.e., the *t* image would keep lingering and the *pf* image would emerge too late). Much more frequent in children is regressive assimilation, such as in *kucker* for *zucker* (example from the Sterns). Here the preferred explanation used to be one of anticipation. The "image" of the second consonant (*k*) is already there before the child's sluggish motor movement begins, interfering with the delivery of the first consonant (*z*). Another factor discussed in the early literature, in particular by Wundt, was the less-than-perfect speech perception of the child. When a word's sound image is defective, so will be its delivery by the child.

The child's sound development became a less active topic during the first decades of the new century. Although phonetic detail was occasionally reported in diary studies of linguists, such as Ronjat's (1913) and Holmes's (1927) short report on her daughter Mollie's phonological development, one looks in vain for new explanations of sound development. Vocabulary, syntax, and functions dominated in the diary and cross-sectional

test-type studies. At the same time, however, phonetics/phonology developed as a quite sophisticated branch of the language sciences. It is beyond the aims of this book to discuss these developments, but the names of Daniel Jones (1881–1967) in London, Nikolaj Trubetskoy (1890–1938) in Vienna, and Harvey Fletcher (1884–1981) of Bell Telephone Laboratories stand for impressive developments in the linguistics and (psycho-)physics of speech sounds. The 1930s showed a corresponding revival of interest in the child's speech sound development. Röttger (1931) noted the insufficiency of "William Stern's" approach to early speech, his use of concepts such as "mutilation" and "tampering" as characteristics of that speech. Lewis, in the preface to his 1936 book on infant speech, remarked that the literature was all but silent on the question of how the flowering of speech during the second year relates to experience and behavior during the first year. Grégoire (1937) noted in his preface, that "Mr. and Mrs. Stern" had deliberately ignored the initial phase of speech development, "under the pretext that it had already been subject of extended studies and that they acknowledged their incompetence in matters of phonetics."[59] The new approaches were quite divergent in method and theoretical perspective. In the end, it was Roman Jakobson (1941) who proposed a general genetic theory of sound structure. It became the classical reference for many years to come. Finally, two major diary studies, by Gvozdev and Leopold, involved substantial further treatments on the child's speech sound development and phonology.

In this section we will follow these developments during the 1930s and 1940s. We will first consider the work by Lewis and Grégoire on the initial phase of children's sound production, from first comfort and discomfort sounds to babbling, and van Ginneken's curious "Mendelian" theory of the heredity thereof. We will then move to the next phase in sound development, age range from 1;6 to 3;6. Here the phonology of the child's initial vocabulary is at issue. We will discuss Röttger's dissertation, which is a first attempt to theoretically account for children's deviations from the adult standard, in Gestalt-psychological terms, and then turn to Jakobson's (1941) general genetic theory of sound structure. Next we will review the large-scale norming studies of the child wellfare institutes, and finally turn to Gvozdev's and Leopold's diary-based studies of phonological development of the late 1940s.

From first cries to first words: Lewis, Bühler, and Hetzer

The main thesis of Lewis (1936) was that there is continuous phonetic and functional development from a baby's first cries to the sound form of first words, contrary to the dominant view. Lewis carefully developed this thesis, beginning with a functional analysis of a baby's first sounds. He tested whether infants' first sounds can be phonetically distinguished as "comfort" versus "discomfort" sounds. This work was inspired by the studies of Charlotte Bühler and her team in Vienna.[60] Their elaborate observational and experimental study of 69 infants in their first year of life had provided substantial evidence for

[59] Grégoire (1937, p. 5), making reference to (Stern and Stern 1907, p. 11).

[60] Lewis repeatedly refers to C. Bühler (1930) and C. Bühler and Hetzer (1928).

expressive vocalizations of discomfort (screaming, crying) and for the subsequent development of comfort sounds. Lewis analyzed their phonetic properties. Discomfort sounds are earliest, innately appearing right after birth. Initially they are loud expulsions of air with tense open-mouth vowels in the range from *a* to *e*, with interspersed consonants such as *l* and *h*. Later the nasals *m* and *n* are added, preparatory to feeding, when the child is hungry. It is only later that these (and other) frontal consonants are uttered in a state of comfort, namely in the view of the pleasant expectation of food. Initial comfort sounds are predominantly guttural, such as in *gaga*. In the post-feeding state the baby is typically lying on his back. The oral musculature is relaxed; the only occasional restriction of the vocal tract is of a "belching" type, back in the throat.

Lewis also discussed, with great approval, Bühler and Hetzer's work on speech and gesture perception in infants. This was, indeed, the first experimental "baby lab" work, which deserves special attention here. Two series of Bühler and Hetzer's experiments involved vocal stimuli. The first, now classic, experiment was to test an infant's response to the crying of another baby. Eighty quiet babies, in the age range of a few days to 11 months, received as "stimulus" another crying baby, which was put or held quite close to them. Of the neonates 84% started crying within about 3 minutes. For 1-month olds this was 60%. This response diminished to, on average, 24% for babies 3–11 months old. Interestingly, for infants over 1 month old the crying response diminished substantially when the crying "stimulus" was separated from the "subject" by means of a screen. The authors concluded that for the 2-month-old infant, another baby's crying is primarily a "social expressive movement."[61] The other experimental series tested infants' reactions to friendly versus threatening speech, to inviting and threatening gestures, and to friendly and angry facial expressions. Twenty-five babies were tested at each month of age level between 3 and 11 months (for gestures from 6 months on). The data are interesting, particularly because they do not show the usual monotonically increasing refinement or improvement characteristic of most developmental data. Three-month-olds respond positively to a face, whatever the expression. This soon changes. From 5 to 7 months the infants' response is exclusively positive to positive expressions and negative to negative facial expressions, but then there is another turning point. Increasingly the infants start responding positively to angry facial expressions, dominantly so at the age of 11 months. The authors' interpretation is that babies are now able to interpret the angry face as an invitation to play. The same type of "curvilinear" development occurs in the response to the voice stimuli (where the face is never in view). The negative response to angry voices, which begins at about 4 months, largely disappears in 11-month-olds; infants then take "angry voicing" as play. The authors conclude that 2–8 months old infants respond suggestively to social stimuli, but from 8 months on they start understanding it in the larger situational context. Lewis modified these conclusions somewhat. The experimental procedure may have diminished the infant's ability to "interpret" the voice stimulus, because

[61] "während auf das Neugeborene bis zu 2 Monaten das Schreien lediglich als akustischer Reiz wirkt und Schock auslöst, wirkt es auf das Zweimonatkind so gut wie nur noch als sociale Ausdrucksbewegung." (C. Bühler and Hetzer 1928, p. 61).

the talking face was not in view. Most likely, such situational understanding of the human voice develops several months earlier.

Lewis provides an extensive data-based analysis of babbling and its causes. Here the author makes considerable reference to the detailed study of Hoyer and Hoyer (1924) about their sons. In this case the mother tongue was Russian. The children did not show any language-specific effect before they "adopted" the first meaningful words in their babbling patterns. The authors found no earlier evidence for an influence of the mother's language on the syllables produced in babbling. They also found little evidence for affective expressive values of particular consonants. What their analysis did show was an increasingly rhythmic organization of babbles. After single consonant–vowel syllables and monotonic repetitions of a syllable, bi- and tri-syllabic "words" arose, with stress on the first syllable and specific "word intonation."

According to Lewis, however, babbles are a progressing mix of pure expression (i.e., exclusive use of the "comfort" frontal consonants), imitation, and "training" (i.e., being reinforced for particular babbles). With respect to imitation, Lewis (but also Grégoire, Jakobson, and other authors) referred to Guillaume's (1925) book on imitation in the infant, in particular to the chapter on vocal imitation, which was partly based on the data of his own children, Paul and Louise. The short summary of that chapter is that there are three conditions required for vocal imitation to develop. First, there should be sufficient perceptual differentiation for "setting up" a model. Second, the child should take the stimulus word as meaningful in some way or another, as a sign. And third, the child should be disposed to social interaction. These conditions are not fulfilled before the end of the first year. Sometimes the third condition is not met for a long time. The child stays mute and then suddenly speaks. The child has perceptually acquired the language without overt imitation. Guillaume's most perceptive remarks on the child's imitation of speech, however, appeared two years later, in a paper we will presently turn to.

Lewis also analyzed the child's phonetic rendering of the first "conventional" words. Children "mutilate" words in various ways. What are the "sound laws" followed here? Take sound substitution as a case. Hilde Stern said *didda* for *tik-tak* and Deville's daughter rendered *ballon* as *badon*. The first point noticed by Lewis was that the consonant replaced or "attempted" could be produced by the child; it was already in the repertoire in no less than 81% of the 355 cases Lewis could lay his hands on. Lewis then tested and confirmed the hypothesis that the substituting sound is one that had been acquired earlier than the target sound. This was true for 77% of the cases. Finally, Lewis demonstrated that the replacing consonant usually preserved the place of articulation, as in *d* for *t*, or *k* for *g*. In other words, given the place of articulation, the more established habit tends to take over. At this point, Lewis referred to Head's work on aphasia (see Chapter 11) where a similar pattern was observed: "more recent aptitudes suffer in excess of those acquired earlier in ontological history."[62]

[62] Head (1926, Vol. 1, p. 476).

Michael Lewis was regrettably not familiar with the work of comparative linguist Wilhelm Oehl, rector of Freiburg University in Switzerland. Oehl (1933) produced an attractive little booklet on babbling words in the world's languages, words that have become part of the lexicon. Not only *mama* and *papa*, but a surprising wide range of words, with similar phonology and semantics, show up as statistical universals in totally unrelated languages. Michael Lewis would no doubt have loved this demonstration of continuity from babbling to speaking.

Physiology, environment, and heredity in early sound formation: Grégoire and van Ginneken

Like Lewis, Grégoire considers the very first phase of vocalization as physiologically determined. For instance, the *erre* sound of the first month seems to be universal. The uvular *r* arises naturally when the child is lying on his back while the air is passing from the lungs through the strictures of the pharynx. Similarly, the early labial sounds, *p* and *b*, probably originate in the innate sucking response. But the child is really immersed in speech from birth on and the crucial question is, according to Grégoire, when and how the infant's vocalizations begin to show language-specific influences. This is the issue of "phonetic normalization." According to the literature, this occurs only at the end of the first year, but Grégoire argues that the evidence is insufficient to show this. He would expect a much earlier environmental effect. However, there are two problems here. The first is the almost total lack of detailed phonetic data on children from different language backgrounds. Even a comparison between German and French children is not possible, because of the lack of phonetic detail. For instance, what about the degree of aspiration (i.e., little puffs of air) of stops such as *p* and *t*? They sometimes sound as p^h and t^h in the child's early vocalizations. Such aspiration is a property of (Bavarian) German, but not of (Parisian) French. Do German infants aspirate more than French ones? The Sterns (and others) do not provide the relevant phonetic detail for a systematic check. Grégoire strongly pleads for comparative phonetic research.

The second problem is that, if such differences are found in the infants' early vocalizations, they could still be innate. Aspiration or other phonetic properties may have become hereditary over the generations in a language community. Here Grégoire makes reference to the bold theory proposed by van Ginneken.[63] That theory has two related components. The short summary of the first component is that every speaker has a hereditary preference for particular kinds of speech sounds. This is because the phonemic systems of languages are the outcome of Mendelian heredity. When speakers of different languages intermarry, the phonemic features of these languages, such as nasality or labiality, or the different vowel systems, enter into Mendelian combinations. If both of the parent languages have nasal sounds, all children will inherit a preference for nasality. Mendel's laws are followed for all other cases of dominant and recessive phonological features.

[63] To van Ginneken (1932), a communication to the Royal Netherlands Academy of Sciences. Van Ginneken's contribution to the Paris symposium on the psychology of language (Delacroix 1933) provides a clear review of the theory.

This then predicts the frequency distribution of phoneme use in the resulting hybrid, but stable language community. Van Ginneken tested his theory by way of elaborate statistical counts in texts; he felt they confirmed his predictions, "Eureka!"[64] The other component concerned the heredity of the "articulation base." Different races have different shapes of the vocal tract. Shape, in turn, induces a predilection for certain types of speech sound. And again, van Ginneken provided iso-gloss maps and language statistics to prove his point. The dominating articulation base in a language community could in fact be the material substrate of the inherited sound preference system.

Grégoire was simultaneously intrigued by these proposals and doubtful. Would early aspiration, if it can be demonstrated in German infants, be due to their "articulation base"? Positive evidence was missing, but negative evidence abounds, according to Grégoire. It has time and again been reported that the early speech sounds of children are, as it were, multilingual. Grégoire's own boys produced, in their babbling, totally un-French aspirated consonants, inspired vowels, clicks, and so on. In addition, there is the well-known fact that foreign babies acquire as natives the language of their new environment. Within a language community the anatomy of the vocal tract shows variations much larger than the average differences between races, Grégoire argues. Still, the members of a language community succeed in "normalizing" their speech sounds, in spite of these variations. Grégoire was not the only sceptic here. Trubetskoy (1939) called van Ginneken's theory "aprioristic." The theory claims that if a particular phoneme is frequent in a language, the cause must be the inherited racial properties which facilitate the articulation of that phoneme. But that is a circular approach.[65] It does not predict anything. "When the Negro languages do not show the same phoneme frequencies as the Indian languages of North-America, that is far from being a proof of the racial dependency of phoneme frequency, because the Negro languages differ from Indian languages not only in phoneme frequency, but also in phoneme inventory and grammatical structure."[66] Only an experiment can provide evidence, Trubetskoy suggests. You could for instance test the phoneme frequencies in two subjects of equal education and same mother tongue, but of different races. Van Ginneken's theory did not fare well.[67] Sturtevant (1940) called it a "pseudo-biological theory. It doesn't predict anything."[68] Spitzer, in a pointed, critical paper,[69] spoke of "biological mysticism." Still, the idea of a genetic account of phonetic

[64] Van Ginneken (1934, p. 141).

[65] a *petitio principii*. (Trubetskoy 1939, p. 234).

[66] "Wenn die Negersprachen nicht dieselbe Phonemfrequenz wie die Indianersprachen NordAmerikas aufweisen, so ist das lange noch kein Beweis für die Abhängigkeit der Phonemfrequenz von den Rassenmerkmalen, weil die Negersprachen sich von den Indianersprachen nicht nur durch die Phonemfrequenz, sondern auch durch die Phoneminventare und die grammatische Struktur unterscheiden." (ibid. p. 234).

[67] See especially Van der Stroom (1995).

[68] Sturtevant (1940, p. 235).

[69] The paper also deals with Hitler's hyper-correct pronunciation of German, pronouncing *Volksgenossen* as *Valksgenassen* (Spitzer 1943, p. 422).

differences between languages did not fully disappear. Fouché (1952) in France and the British geneticist Darlington (1949) flirted with the idea. Eventually, Dediu and Ladd (2007) demonstrated the existence of a relationship between a genetic mutation and the occurrence of linguistic tone.

I have one final remark on van Ginneken's unchecked, "bold" theorizing. In 1939 he published the still bolder theory that the course of language evolution had been as follows. Primordial man first communicated by means of gesture. These gestures, in turn, led to hieroglyphical signs and writing systems, such as primordial Chinese, expressing meaningful gestures. Only then did the first stage of oral language develop, a system of "click-words," consonantal sounds produced at different places in the vocal tract, whose meaning relates to the written signs. These single-phoneme words soon did not suffice to represent all visual signs and multiphonemic words developed. "Hence, our review demonstrated that oral languages didn't appear in the history of mankind until about 3500 B.C. at the earliest."[70]

Back to Grégoire. His detailed phonetic analyses of early speech often made it possible to become more precise on important theoretical issues. A few examples must suffice here. Is it indeed the case that the babbling child is "able to produce all imaginable sounds"?[71] Grégoire demonstrates how the developing physiology of the infant's vocal tract, such as the increasing control of oral aperture and the increasing ability to protrude and round the lips, leads to specific extensions of the babbling range. He could then trace both the disappearance and reappearance of specific sounds. For instance, Charles produced the vowel *i* during his third month, while uttering *gih*. Then it disappeared until month ten, after which it was occasionally used as a sound imitation (*titi*, *mimimi* in response to the chirping of sparrows). Grégoire could further show that where babbling and first words overlap in time, the two sound repertoires are quite different. The same holds for the first onomatopoetic sound imitations and first words. Still, babbling increasingly "adapts" to the speech in the environment. That "conforming can begin early," Grégoire (p. 103) concludes. These and many other observations in Grégoire's diaries were used by Jakobson in his famous monograph.

Sound assimilation in children's early words: Röttger's dissertation

Fritz Röttger was born in 1897 and died during the two days the Red Army entered and plundered his home town Meissen in 1945.

Wundt's ghost is much around in Röttger's (1931) Leipzig dissertation. The author picks up the theoretical discussions about the genesis of speech sound patterns where Wundt had left them in 1900. But then he moves ahead in two ways. Empirically, Röttger does away with the relative scarcity of phonetic data on children's early speech. His quite effective approach was to collect spontaneous, mostly one-word utterances of 28 different

[70] "Or notre revu vient de montrer que les langues orales n'apparaissent dans l'histoire de l'humanité qu'environ l'an 3500 av. J. Chr. (. . .) au plus tôt." (van Ginneken 1939, p. 124)

[71] "l'enfant est à même de produire tous les sons imaginables." (Grégoire 1937, p. 101).

children, on average some 140 utterances per child. He would sit together with the playing child (mostly other familiar persons being present too) and phonetically transcribe each utterance and the intended "target" word(s). The children were in the age range of 1;6 to 3;6. These almost 4000 phonetically transcribed utterances formed a data base unmatched by any earlier study. They are all listed in the dissertation and would still be a quite useful data set for students of phonological development. One obvious limitation of this approach is that it could not reveal any diachronic patterns. Another one is that the amount of data per child is too small for an analysis of an individual child's phonemic system. The tacit, but contestable assumption is that they are essentially alike. Theoretically, Röttger had absorbed the "Ganzheits" psychology of his time (and of his supervisor Felix Krüger), the "primacy of the whole over its parts." This, he hoped, would free him from the "isolating" approach to the patterning of speech sounds, which had been characteristic of Wundt's and the Sterns' treatment of sound change.

Röttger's extensive analyses largely concern the children's rich range of assimilations (e.g., *sauft* → *fauft*), but also their relatively rare dissimilations (e.g., *lampe* → *lamde*) and permutations (e.g., *brille* → *bline*). Let us consider some of his analyses and explanations. The dominant forms of assimilation are the "regressive" ones, such as just mentioned, *sauft* → *fauft*. Here, at word onset, the child "anticipates" the later consonant (*f*). More generally, an earlier consonant or vowel shows a qualitative change in the direction of a later speech sound, in particular its place of articulation. The traditional explanation is that the later "sound image" appears before the earlier one became expressed. The interference is not physiological or articulatory, but "mental," as Meringer had put it.[72] Röttger admits that many of his assimilation data do fit this pattern. It is a good starting point for determining the relative "strength" of speech sounds. Some sounds are more often replaced than others and some sounds are stronger inducers of assimilation than others. A thorough data analysis shows that the relative "strength" (*Akzentuiertheit*) of children's consonants follows this pattern (from strong to weak): labials (*b, f*)—back tongue (*k, g*)—front tongue spirants (*sh, s*)—front tongue plosives (*t, d*)—nasal *n*—liquid *l*. Hence, liquid *l* is the weakest consonant. It is not salient (has no *Prägnanz*). It is often replaced, but it never replaces. There are many smaller variants of this pattern, which we will not discuss here.

So far so good, but many assimilations are more complex. Take the example *sube* → *fube*. Here assimilation changed *s* into *f*, but there is no *f* around in the word to be anticipated. What happens is that the labial quality of *b* is anticipated, while the fricative quality of *s* is maintained. The result is labial fricative *f*. Here Röttger is about to propose a *feature* theory of assimilation. That, however, is not exactly what he does; it would be too "isolating" to his taste. Rather, he argues that the sound pattern is not just a successive structure, but that it is experienced by the child as a diffuse *simultaneous* complex. This "whole" has its specific "Gestalt" quality. This quality is largely determined by the

[72] Röttger's (1931, p. 82) own citation: "Nicht in den Sprachwerkzeugen wird die Sprache gemacht und verändert, sondern in der Seele." (Meringer 1908, p. 241).

consonant following the stressed vowel. In *sube* that is the *b*. Its most "pregnant" quality is its being labial. This then "colors" the sound complex as a whole, in particular its second most complex region, the pre-vocalic consonantal area. That spirant now "adopts" the labial property, becoming realized as *f*. Similar explanations are proposed for various cases of "contact assimilation." A frontal vowel, for instance, often adopts the "back" quality of a following "back" consonant. That happens in *bernd* → *band*.[73] Here the "back tongue" *r* becomes totally fused with the consonant, which marks the diffuse Gestalt quality of "backness." A consonant is often "colored" by the quality of an immediately following consonant, such as in *braf* → *graf*, and so on. Röttger provides an abundance of detail.

What is gained by this Gestalt theory of children's early speech? Not too much. One strength of Röttger's treatment is his detailed analysis of feature spreading, including a characterization of feature strength or dominance. Frontal tongue position, for instance, is weak. It is easily "colored" by a neighboring back tongue position. The notion of "markedness" was not around yet, but that is in the background of Röttger's analysis. Another strength is Röttger's detailed argument against traditional "least effort" or economy explanations of the child's sound renderings. The spreading laws often create forms that are *more* complex than the target, or at least not more pronounceable. Here are two such examples: *sürdse* → *dthodthe*, *vorwärts* → *fofodth*. (NB: Here *th* stands for *th* as in *that*.) The weakness is that the theory explains too little and too much. Too little because it fails to explain how a diffuse, "pregnant" feature can "jump" over an intermediary region. For example, when the child pronounces *badewanne* (bathtub) as *banewanne*, the nasal quality of *n* jumps over *w*, which is not affected: it does not become *banenanne*. Röttger's ad hoc explanation is that *ade* and *anne* have a great "affinity" (p. 89). Similarly, in *enefant* for *elefant*, global nasality jumps over *f*. The ad hoc explanation is that *l* is labile and in an unstressed syllable. Gestalt theory also explains too much. Röttger computed that the order or position of (remaining) sound elements is preserved in 97.7% of his observations. Why? Because the quality of each element is "co-determined" by its position in the sound complex as a whole (p. 179). How then should one explain the 2.3% exceptions, such as *brille* → *bline* or *stuhl* → *dlu*, where *l* wandered out of its post-vocalic position? Because other "whole" qualities (*Ganzheitqualitäten*) of these elements, i.e., other than their position, are in effect here. "Such wholeness qualities are always strongly emotional, which also accounts for their extreme shaping power" [*Gestaltungskraft*][74], which sounds quite convincing indeed. It was of the *Zeitgeist* to make Gestalt formation the *deus ex machina* of psychological explanation.

[73] Röttger's is possibly the first detailed treatment of children's consonant-vowel place assimilation. See C. Levelt (1994).

[74] "Solche Ganzqualitäten sind immer stark emotional; daher auch ihre hochgradige Gestaltungskraft." (Röttger 1931, p. 180).

Jakobson on universals of phonological development

Jakobson's thesis in his slim 1941 monograph *Kindersprache, Aphasie und allgemeine Lautgesetze* ("Child language, aphasia and phonological universals") is that universal sound laws govern both phonological development in children and phonological decay in aphasics. Any speech sound system is hierarchical in nature. For instance, no language, no child, no person with aphasia uses distinctive velar consonants (such as *k* or *ng*) without also possessing the corresponding frontal consonants (such as *t* or *n*). But the reverse is often the case. There are languages with frontal, but without velar consonants and most children go through a phase in which they make no velar distinction, only dental/labial ones. The frontal consonants are more "basic" than the velar ones. Jakobson's claim is that there exists a universal phonological hierarchy. Individual languages, individual children or people with aphasia "pick" a subset from this hierarchy, always obeying the rule that a higher level of distinction can only be adopted if the lower levels are also there. No superstructure without substructure.

Children's speech obeys this pattern as soon as their vocalizations become *speech* sounds, i.e., when they become intentionally distinctive. From then on they have a "phonemic value." During babbling this is not the case. At that point the only restrictions on sound formation are physiological. With increasing control over the vocal tract, the infant will produce just about any possible speech sound. There is no further system in either the infant's chosen "repertoire" of babbling sounds or in their order of appearance. This had also been observed by Jeanne-Andrée Feyeux (1932, p. 127), the French specialist in children's speech disorders, to whom Jakobson makes due reference. The transition to distinctive speech is quite dramatic. Its first stage involves an utter minimum of sound distinctions. The child all but loses its complete sound-producing capacity, including the ability to produce the speech sounds of the native language. Even more remarkable, the two systems can coexist for some time, as we saw in Grégoire's analysis. This in itself is a strong argument against traditional "least effort" accounts of sound acquisition.

Jakobson's further claim is that children by and large follow the same pattern in building up their phonological system. Children can differ enormously in the rate of acquisition, but they follow the same sequence of stages. The sequence is determined by the hierarchical structure of the sound system. It is acquired level by level. A fine corollary of this perspective is a reassessment of the creative or "spontaneous" role in the acquisition of speech. In Jakobson's own terms: "The child creates by adopting." It is, initially, impossible for the child to imitate in the Wundtian sense, more or less "parroting" the sound it hears. This "active" view on child imitation had earlier been expressed by Guillaume (1927a), a publication Jakobson apparently missed: "But no imitation is completely passive. There is first a selection; the child does not indifferently reproduce everything he hears: but in particular he starts to *construct* with the resources of his vocabulary, on the model of the expressions he knows how to produce."[75] Even earlier, Bloch (1921) had written: "a new word

[75] "Mais aucune imitation n'est complètement passive. Il y a d'abord une sélection, l'enfant ne réproduit pas indifféremment tout ce qu'il entend: mais surtout il se met à *construire* avec les ressources de son vocabulaire, sur le modèle des expressions qu'il sait produire." Guillaume (1927a, p. 24).

modifies itself after an old word."[76] Jakobson puts it this way: "Each imitation requires a selection and hence a creative deviation from the model."[77] In other words, the child will recode the presented word in terms of its own, still quite limited, phonological system. This may both involve ignoring features in the model and adding features not in the model. Neither ignoring nor adding features is arbitrary. A noticeable consequence of this recoding, Jakobson explains, is homonymy. Bloch's child, for instance, used *avé* for *laver*, *lever*, and *trouver*; it used *asé* for *casser*, *chercher*, *marcher*, and *ramasser*).[78] We should also mention Velten's (1943) study of his daughter's phonemic development, which was much inspired by Jakobson's work. Little Joan, in her acquisition of English, also showed large-scale homonymities (such as *bat* standing for "black," "pat," "bark," "spot," "block," "pocket," "bought," "button," "bent," and "bite"). She also exactly showed Jakobson's staged acquisition of the phonological hierarchy, to which we now turn.

The infant's very first speech sound distinction, according to Jakobson, is between vowel and consonant, between opening and constriction of the vocal tract. It is the universal basic level in the phonological hierarchy. The open vowel is *a*-like, the initial constriction is labial and usually unvoiced (*p*). The contrast is realized *in succession*, or "paradigmatically," establishing the first model of a syllable, such as *pa*. The next stage is the addition of a consonantal distinction, by opening (versus closing) of the nasal tube. This makes it possible to meaningfully contrast the syllables *ma* and *pa*. At this stage there is as yet no distinctive use of the vowel. In a next stage a first vowel contrast is made between "colorful" and "colorless" vowels,[79] initially between colorful *a* and some more frontal vowel, such as *i*. This allows the child to make a meaningful contrast between *pipi* and *papa*. With every contrast added, every new "distinctive feature" is binary. One pole of the opposition is "neutral," the other one is "augmented." Non-nasal is neuter, nasal is augmented; *a* is neutral, *i* is augmented. This is the beginning of "markedness theory." The general observation is that the neutral pole of a contrast tends to be more frequent in the words of a language than the marked pole. That should also hold for the child's initial speech. The next consonantal contrast to develop is within the frontal pole of the consonants, dentals versus labials, which allows for meaningfully contrasting *pa* and *ta*, or *pi* and *ti*, or *ma* and *na*. And so on. We will not follow Jakobson's detailed analyses of children's acquisitions in a variety of languages.

Another noteworthy feature of the monograph is the short shrift Jakobson gives to a persistent, still widespread misconception in (psycho-)linguistics. It is the application of "Haeckel's law" to language development: "Child language is to standard language as the languages of primitive peoples to those of cultured peoples."[80] However, phonemic

[76] "un mot nouveau se modifie d'àpres un mot ancien." Bloch (1921, p. 702).

[77] "Das Kind schafft, indem es entlehnt. . . . jede Nachahmung bedarf eine Auslese und somit eines schöpferischen Abweichens vom Modell." Jakobson (1941, p. 8).

[78] Ibid. p. 118, Bloch (1921, p. 704).

[79] Here Jakobson adopts Stumpf's (1926) analysis of the vowel system, which was designed in analogy to the "color space."

[80] Here Jakobson cites the Sterns, 1928 edition, p. 113: "Es *entstehen* dann die Gleichungen: Kindessprache

contrasts are as much present or absent in "cultured" languages as in the languages of "primitive" peoples (*Natursprachen*). Latin or Russian, for instance, have no back nasals, but "primitive" Gilyak does have them. The "unification" of child language and typology is of a quite different kind, according to Jakobson. It concerns what we now call "relative" universals of language of the type: all languages with velar consonants also possess the corresponding frontal consonants. There is a hierarchy of contrasts which applies as much to so-called "primitive" languages as to "cultured" languages.

Jakobson's refreshing theoretical approach forms a remarkable contrast to the exclusive data-gathering approach of the Child Welfare Institutes.

The Child Welfare Institutes on early sound development

Wellman *et al.* (1931) of the Iowa Child Welfare Institute developed a method for measuring the child's speech sound production at different age levels. It is a follow-up to the Smith (1926) vocabulary study. They tested 204 children in the age range from 2 to 6 on their articulation of a set of test words containing 66 critical consonants, 48 consonant clusters, 15 vowels, and 4 diphthongs, 133 different speech sounds in total. This was done by presenting pictures to the children, which might elicit the critical words, with appropriate help from the experimenter. The children's elicited renderings of these test words were phonetically transcribed for further analysis. How well do children do at different age levels? Figure 10.5 shows the percentage of correctly produced sounds at age levels 2 to 6 for consonants, consonant clusters, vowels, and diphthongs respectively.[81]

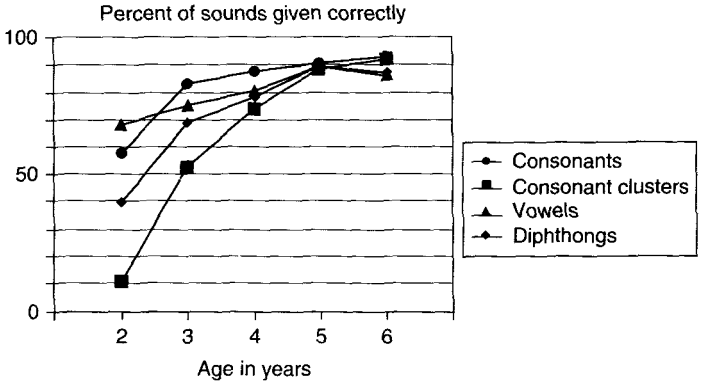


Fig. 10.5 Percentage of speech sounds produced correctly at different age levels for consonants, consonant clusters, vowels, and diphthongs. Data from Wellman *et al.* (1931).

zu Vollsprache wie Sprachen der Naturvölker zu denen der Kulturvölker, oder wie frühe Phasen der Kultursprachen zu ihren Entwicklungshöhen."

[81] The corresponding graph in the monograph (Figure 2) is faulty. Here the Group B data are used.

The authors also presented detailed overview tables on the order in which the 133 speech sounds are acquired, in frontal, medial, and final positions. If anything gets close to statistical norms for speech sound development in children, it is this publication.

A decade later, Irwin (1941a) of Iowa University presented a short and quite incomplete review of studies in the early vocalizations of infants, among them his own unpublished work on 15 babies in their first half year. "Contrary to reports in the literature" (p. 283), he found that the labials *b*, *p*, and *m* do not occur, but rather *h* and occasionally *k* and *w*. Also different from most reports the first vowels are frontal, not middle or back. There is a great need, he says, for adequate samplings of high reliability, but there is no word on a need for developmental theory. The more adequate samplings are what Irwin with his coworkers then developed during the 1940s. They found that inter-observer reliability can be quite high, if the infant's expiration is taken as the unit of observation. For each such unit the observer would score the number (usually 0 or 1) and the character of vocal sounds produced. They are immediately transcribed in IPA format, not recorded on wire or phonograph. During their first 10 days, babies produce vowels exclusively. Irwin and Chen (1941) collected a total of 1200 breaths on 40 infants under 10 days of age and then computed vowel "profiles" or frequency distributions over the 10 different vowels that were rated. They showed the preponderance of front vowels. This was further confirmed in Irwin and Curry (1941) on a similar sample of newborns. Their most frequent vowel was æ, followed by ɛ and this was the same for boys and girls. This preponderance of front vowels diminished over the first 18 months of life. At 1;6 infants produced substantial numbers of back vowels (Irwin 1941b).

The main aim of this research was to establish norms to create a diagnostic tool for speech development. Irwin (1941b, 1942), for instance, measured vowel and consonant profiles in children with learning difficulties up to 5 years old. They "performed" at the level of 1-year-old children. Brodbeck and Irwin (1946) compared infants in families to infants in orphanages up to the age of 6 months. The orphanage children were disadvantaged on both the frequency and the variety of vocal sounds produced. Irwin (1948) compared infants without siblings to those with siblings. The latter vocalize more, but only marginally. Spiker and Irwin (1949) related their speech diagnostics to Kuhlman IQ tests on children up to 30 months of age. The correlations were pretty weak, not good enough for diagnostic use. But one should have no doubts that this was real science. Irwin (1947) and Irwin and Chen (1947) published curves on phonemic frequencies and number of phonemic types over the age range 0 to 30 months. They could be captured by simple mathematical equations. Clearly, it would not have mattered if the various speech sounds had been acquired in reverse order. The enterprise was deeply a-theoretic. Nowhere there was any reference to Jakobson (1941), which was widely cited by others.

Dorothea McCarthy of the Child Welfare Institute at the University of Minnesota published a major review paper on the vocalization of infants in 1929. This quite comprehensive historical review goes all the way back to Tiedemann. She also reviews the tools for recording or transcribing infant vocalizations. This work provides a nice glimpse of the technological innovations such as wave-tracing devices (with rotating cylinders), phonographs, dictaphones, and wire recorders, none of them good enough to be used on infants' vocalizations, according to McCarthy.

A few years later, the Minnesota Institute was involved with a large-scale longitudinal study of 25 babies since birth. These children were followed on an array of developmental measures, one of which was speech. Mary Shirley's (1933) three-volume account of these data includes a chapter on the beginnings of speech. The infants' speech was scored on such features as "vocal grunt," "singing tone," "monosyllabic response," "first imitative response," "first word," and "first pronoun." When you read these pages, you start sympathizing with the repeated complaints of developmental linguists, such as Grégoire and Leopold, that the educational and psychological literature is unsophisticated on the subject of early speech. Shirley (Volume II, p. 47) writes that early vocalizations were "noted by jotting down 'coos,' 'babble,' 'gurgles,' 'sings,' and 'scolds'." We read among the first words (median age 60 weeks) not only *mamma*, but also *pretty*, *oh dear* and *stocking*. There is a table (on p. 55) of vocal reactions reported by the children's mothers. It contains such categories as "Invent words to express wants" and "Say childish word." Clearly, studies of more linguistic sophistication were needed. The work by Wellman *et al.* and by Irwin and his team at least introduced new standards in the world of educational testing by using phonetic (IPA) notation.[82]

Sound development in Gvozdev's and Leopold's diaries

Gvozdev (1948) combined his original 1927 study of his son Zhenya's sound development with a new, 1948 follow-up analysis. Both publications are in Russian, but Magdalena Smoczynska helpfully provided me with extensive notes on both of them, from which I may cite the following. With respect to the 1927 study:

> There is a very detailed description of acquiring the sound system of Russian. In chapter 1, Gvozdev presents the development of the growing inventory of phones: consonants, vowels, as well as the rendering of consonant groups – in the following two-month sections of Zhenya's life, starting with 1;7–1;9, with the last period discussed being 2;10 to 3;2. He analyzes the appearance of the given sounds in different positions in the word, also related to the (mobile) stress patterns typical for Russian, and gives numerous dated examples of all the phenomena he discusses. He also discusses the omissions and substitutions. For consonantal groups he proceeds in a similar way, but he also gives a developmental analysis in a larger time scale than the short two-month sections, which he says to be more adequate, as he gets the possibility to get enough examples of particular consonant groups.

With respect to the new 1948 text, Smoczynska notes: "In publication nr 2, which is addressed to kindergarten teachers (but is also a detailed linguistic study, not a popular simplification), he looks not only at his son's data but also other published Russian child language data dealing with phonetics." Gvozdev makes reference to some quite early, turn-of-the-century publications on the acquisition of Russian. The text systematically reviews early phonetic development, from the initial sound inventory to the emergence of consonantal groups and complex word structure. There is a treatment of sound substitution, assimilation, and exchange, and a discussion of the acquisition of intonation and sentence stress. The text is completed with pedagogical conclusions. All in all, however,

[82] We (irresponsibly) avoid that notation in the present section.

"there is practically nothing on phonology as such in these works, no trace whatsoever to Jakobson's theory, no bibliographic reference to it either."

Gvozdev's (1948) publication coincided with Shvachkin's (1948) experimental study of Russian children's stages of phonological development. Shvachkin constructed a test list of words containing all Russian phonemes. He then taught children these words as names for objects and tested their comprehension of the words in a longitudinal study (ages 0;11 to 1;11). Slobin (1973) provides a detailed review of Russian phonological development based on these and several other early studies.

Werner Leopold, in his analysis of his daughter Hildegard's phonological development (in Volume II of his major work) does take issue with Jakobson. Volume II presents full tables of vowels, diphthongs, and consonants as they are successively acquired. It is striking how often Leopold finds Jakobson's predictions confirmed (pp. 197–206), but this is not always the case. Leopold claims that Jakobson's theory of contrast should be supplemented by the old economy principle. Children simplify articulations. In her earliest, syllabic words, Hildegard always voiced the initial consonant, as in *baba* for *papa*, accommodating the consonant to the following vowel. For Jakobson consonant-vowel is the first contrast, which should be maximized, not minimized. This "economizing" on articulatory effort is more generally apparent in Hildegard's assimilations, where articulatory features of one speech sound are accommodated to those of neighboring ones. Leopold also discusses Hildegard's sound assimilations and substitutions, which had been the main theme of Röttger's book. The data are similar. Neighboring sounds can show partial assimilation, simplifying articulation. This is still rare during the first two years, because Hildegard does not yet produce consonant clusters, they are simplified by deleting the second consonant, for example *cracker* → *gaga*, with voicing accommodation to the following vowel. Long-distance assimilation, i.e., sound perseverations or anticipations, also rare, tend to be full substitutions, as in *wauwee* for *Milwaukee*.

Is bilingual education harmful? Leopold values precision: "An achievement test . . . at the beginning of the fifth grade showed her [Hildegard's] English vocabulary as being equivalent to the seventh or eighth grade standard, the English reading comprehension as equivalent to sixth grade. Obviously the exposure to two languages had not affected adversely her record in the school language." (Vol. II, p. vii).

This completes our review of work in sound development during the 1930s and 1940s. It shows increased sophistication in the study of early sound production in infants. It also shows, at least in Europe, important theoretical innovations in the explanation of phonological development, with Jakobson's 1941 monograph as a historical landmark. The United States Child Welfare Institutes continued their grand scale norming studies, rich in detail, poor in theory. Werner Leopold's major diary study was a breath of fresh air on the American scene.

Language acquisition in bilingual environments

There is a noticeable discrepancy between linguistic and educational studies of bilingual language acquisition. There are just three linguistic diary studies of bilingual acquisition

during the period we are considering. They are the dissertations by Ronjat (1913), Pavlovitch (1920), and the four-volume work by Leopold (1939–1949). They concern three different language pairs: French–German, Serbian–French, and English–German, respectively. Both Ronjat and Leopold concluded that bilingual education is not disadvantageous to intellectual development. Pavlovitch was silent on this matter. He reports on the first two years only, during which Serbian was entirely dominant.

The huge educational literature is more negative than positive on the effects of bilingual education. Before turning to that, however, one paper deserves special mention. It is Madorah Smith's (1935) paper on the diary records of a family of eight children in a bilingual situation. This is the family she herself grew up in as second child.[83] Her parents were missionaries who lived in China from 1881 to 1894, with a year's leave during 1890. Five years after their return they moved to Iowa. That is where Madorah Smith, whose further work on bilingualism we will turn to, got her MA and (in 1925) her PhD. The mother of the family kept detailed diary records on the physical and mental development of her eight children over a period of 14 years from the birth of the oldest child in 1886. In her 1935 paper, Madorah Smith analyzed the language records of all eight children. One of her core analyses compared the development of the oldest three children (including herself) to the youngest five. Her reason for this was that the elder children grew up in a transparent linguistic situation: both parents and all the whites around spoke English to them exclusively, whereas the nurses used Chinese exclusively. This was quite different for the five younger children. After their one year leave of absence, the parents used either language to their children. In other words, this was a natural experiment in OPOL (although Smith did not put it in those terms). Does Grammont's OPOL principle make a difference for the child? Smith compared the records on sentence length, number of errors per hundred words, number of inflected forms in English, the percentage of code-switched ("mixed") sentences, and the number of Chinese words used. The data were exceedingly clear: "at every age level the older children excel the younger children by all criteria." (p. 21). There was, in particular, far less code switching in the older children. Good news for OPOL adepts.

However, OPOL was no issue in the flood of educational studies on bilingualism in children. These were statistical studies, often very large scale, on special groups. In the majority of cases the "target" language was English, either British or American English. The other (usually) native language was, in the majority of cases, an immigrant language, such as Italian, Spanish, Chinese, or Japanese, a minority language such as Hopi, or a language along a language border, such as Welsh. A first major study, which set the tone, was by Saer (1923).[84] He compared 828 Welsh–English bilingual and monoglot children in either language and 569 university students with Welsh as a native language. The conclusions were dramatic: "monoglots" showed considerable superiority in intelligence and vocabulary. The bilinguals were, moreover, quite "confused" in dexterity.

[83] See also Baldwin and Smith (1925).

[84] See also Saer *et al.* (1924).

In short, there exists great mental confusion in bilinguals. This greatly impressed educationalists and policy makers. Conferences were convened, such as the 1928 one in Luxemburg, organized by the International Bureau of Education,[85] and research funding boomed. Over the 30 years following Saer's study there was, on average, one major statistical study every year.

One excellent review of this work appeared in Arsenian (1937). He discussed both small and large studies in no less than 14 countries, including Czechoslovakia, Mexico, the Philippines, China, and Russia. He was quite critical of most of them and reported a neat study of his own on the mental abilities of 2778 New York children (age range 9–14) of Jewish and Italian background. He found no negative effects of bilingualism on mental development.[86] Natalie Darcy (1953) presented a further, though less critical review. She grouped studies in three categories, showing bilingualism to be favorable for intelligence, unfavorable, or without effect. There were many more unfavorable than favorable studies (as was the case in Arsenian's review). That was indeed the dominant mood during the period we are considering. Madorah Smith, for instance, performed a mega-study of bilingualism in Hawaii, in which she extensively tested 1000 2–6-year-old children on their spontaneous, productive language usage. The children came from seven different language backgrounds: Chinese, Filipino, Hawaiian, part-Hawaiian, Japanese, Korean, and Portuguese. They were tested by an observer from the same background and were free in their language use. Responses were dominantly in English, although with much code-switching. They were scored on a wide range of syntactic and morphological measures. Smith (1939) concluded that the island children "are seriously retarded in the use of the English language, a retardation which is not compensated for by a greater advancement in other languages used" (p. 266). "The retardation is due to two handicaps: the prevalent use of pidgin English, and the bilingualism of many homes." (p. 271). Such conclusions were not an exception in the literature reviewed by Arsinan and Darcy. Smith's 1949 advice was: "It would seem unwise to start any but children of superior linguistic ability at a second language unnecessarily during the preschool years." (p. 309). Madorah Smith has also (repeatedly) been cited to have expressed the view "that second-language learning in childhood is arduous, handicapping, and fraught with problems."[87] That, however, she never wrote. Smith's style was always entirely matter-of-fact.

A major problem in these statistical studies was the number of factors to be controlled, which is indeed hair-raising. Always or often considered were age, sex, language used at home, and verbal or non-verbal intelligence (one consistent finding was that the negative

[85] Reference in bibliography: Bureau International d'Education (1928).

[86] Margret Mead (1927) tested American and Italian children, 6–10 years old, in a small New Jersey town. The Italians were inferior on the standard intelligence test used. But Mead did not conclude they were less intelligent. Rather these tests do not provide "a just evaluation of the child's innate capacity" (p. 468). A similar conclusion had been reached by Colvin and Allen (1923): "the Italian children are suffering from a language handicap, hence their intelligence as determined by their scores in the verbal group Tests, is rated decidedly too low" (p. 5).

[87] Minami (2002, p. 732).

effects on intelligence were obtained with verbal IQ tests, but not consistently with non-verbal tests). Factors that were less or never considered were socio-economic status (although this had been stressed by McCarthy 1930), mental abilities other than (school) intelligence (such as metalinguistic, attentional, and social abilities), age and intensity of first contact with second language, second-language ability when the child entered school, whether the child's bilingualism is balanced or unbalanced, relatedness of the two languages, the situations in which the two languages are used, contact persons for these languages, status of both languages, OPOL or no OPOL, and so on. And there was hardly ever a linguist involved in these studies.

Indeed, there was still a long way to go before being able to sort out these complex interactions, and the discussions would never be entirely free of political pressure. It should be noted that William Stern added a discussion on bilingualism to the 1928 edition of *Die Kindersprache*. In it he criticized the dominant negativism in the literature, choosing as one target the old book by Epstein (1900). Epstein had stressed the harmful interference of languages caused by bilingualism, but, even if true, the Sterns commented, would this multilingual situation not also "lead to a powerful mental challenge"? It would stimulate the making of active comparisons and distinctions, the precise coverage of concepts, becoming conscious of subtle meanings of words, and so on. A real "understanding of the mother tongue will result just because of the foreign language and the contrast of these languages."[88] This issue is still active today (see de Groot 2011).

Retrospect: data, theory, and method

Language acquisition research was booming during the first half of the twentieth century. Its most striking characteristic was the avalanche of new data. They were largely of two kinds: diary data from both psychologists and linguists, and large-scale psychometric data, almost exclusively gathered by educationalists and psychologists. Clara and William Stern's monograph set the agenda for the study of the child's acquisition of word and sentence. It was an integrated study of form, meaning, and function. The Sterns demonstrated this integrated approach in great detail in the area of word acquisition, from first volitional-affective word use to more representational uses in sentence context, where the parts-of-speech functions of words emerge, together with the corresponding inflectional and derivational morphology. They also analyzed the acquisition of syntax in far more detail than had ever been done before.

It was, in a way, an empoverishment for Piaget to ignore both form and meaning of his children's speech. His interest was in the "egocentric" usage of language, the younger child speaking for itself most of the time. But he did not even consider the function of this monological speech. Vygotsky and Luria had to stress that it is a proto-form of adult's inner speech, which serves conceptualization, planning, and decision making. The most

[88] "einen mächtigen Anstoss," "dass das Verständniss der Muttersprache gerade erst durch die Fremdsprache und die Verschiedenheit beider Sprachen herbeigeführt wird." (Stern and Stern 1928, p. 298).

remarkable influence of Piaget's early work was the adoption of his empirical method, in modified version, by McCarthy and her many followers, but for a rather different purpose: the establishment of norms for the development of vocabulary and sentence complexity. Securing norms, however, inevitably drastically limited the range of developmental phenomena that could be handled. Measures of sentence complexity, for instance, involved sentence length, completeness, coordination, subordination, parts of speech, and the like. But they did not reflect the conditions and functions of use in context, which the Sterns had unpacked. William Stern commented in his 1928 updating of *Die Kindersprache*, when discussing Smith's statistical work on syntax development, that Smith's main results "could, to be sure, have been obtained without such an excess of work."[89]

New, data-intensive approaches to sound development appeared later, during the 1930s. Careful phonetic transcription became the norm and the focus lay on the first two years, where babbling was giving way to first words, real speech. Although Lewis correctly stressed the noticeable functional and phonetic continuity of this transition, the growing consensus was to consider babbling and early speech as structurally *discontinuous*. In babbling practically any sound could emerge from the vocal tract's random movements. In early speech this whole repertoire evaporated and a slow, stepwise process of acquiring the native language's speech sounds and their combinations started. This two-stage view had been expressed during the nineteenth century, but now received substantial support. The real breakthrough, however, was Jakobson's explanatory theory of this stepwise acquisition of the native sound repertoire.

Developmental theory barely kept pace with the data explosion. The classical issues in acquisition research, the balance of nature and nurture, the reflexion of phylogeny in ontogeny, and the specific mechanisms of learning received most attention early in the century. The Sterns proposed their convergence theory, which replaced the question "Which functions or properties are innate and which are acquired?" with "What are the contributions of nature and nurture to *any* function or feature?" William Stern further developed this approach in the framework of his "personalism," the view that any function must be considered as part and parcel of the individual's unique personality, with its instinctive forces of homeostasis and developmental drive. Karl Bühler, in his 1918 book on child development, discussed the three major views on development: maturation, drill, and insight, and argued that a complete theory would involve all three. He stressed, in particular, the role of Mendelian inheritance of psychological functions and the function of play in the child's development. Still, the application of such general notions to the child's concrete language development was lacking. More concrete was Stern and Stern's discussion of imitation and spontaneous analogy formation in morphological and syntactic development. These two mechanisms received much attention from Guillaume

[89] "Die Hauptergebnisse – die freilich auch ohne solchen Aufwand von Arbeit festzustellen sind - lauten: ..." (Stern and Stern 1928, p. 198).

(1927a,b), who analyzed the roles of phonological, morphological, and phrasal models as schemas in the child's recoding and "regularization" of language.

What struck me most, however, in working through this daunting literature, was the absence of behaviorist theory. Worth mentioning is a triad of theoretical papers in *Psychological Review* by Latif (1934). According to him, words in general, and in particular children's first words, "are conditioned responses to action." He cites approvingly from Holt's *Animal drive*: "the meaning of a word lies entirely in that which the word as a stimulus makes one *do*," and "all mental contents are actual motor responses."[90] For instance, "The words 'Yes' and 'No', for example, symbolically represent opposite motor attitudes of the speaker. Negation is a reversal of movement. And the difference between these words is manifestly behavioural." (p. 246). ". . . Mood *is* behaviour, motor attitude." (p. 263). Latif announces that he will apply this "physiological interpretation of consciousness" to linguistic development, but in fact he does not. He rather reviews the developments from babbling to first words and holophrases, to Stern's "chain-sentences," by citing longish texts of various authors. He also adopts recapitulation theory: "it is interesting to note that the holophrastic use of sounds is found not only in children, but also among primitive men and in primitive languages" (p. 168). It is behaviorism as a belief system, rather than a testable theory of linguistic development.

Indeed, hardly any research was explicitly designed to test behaviorist predictions of language development. This does not mean that behaviorism was without effect on the scene of language acquisition, on the contrary. Its main effect was methodological. It was the mantra of going for objective, replicable data, no less, no more. That characterized the extensive output of the Institutes for Child Welfare in Ohio and Minnesota. Their texts were neatly non-mentalistic, but also systematically a-theoretical. Nobody would have raised an eyebrow if the data had turned out differently. Here the often mentalistic linguists signed a pact with the devil. Leopold, Grégoire, and others stressed the absolute priority of gathering precise and rich data before going into "generalizations." It was only Jakobson, in 1941, who demonstrated the force of theory.

Certainly positive was the wide range of languages studied.[91] There was, moreover, a beginning of language-comparative approaches to child language. Gheorgov, Kenyeres, Grégoire, and above all Jakobson set the scene for what would become core business in modern acquisition research.

The almost total lack of attention to the child's linguistic input is remarkable. It was apparently hard enough to jot down the child's own utterances; recording dialogues was beyond the technical facilities of the dedicated diarist. But as a consequence, the role of input, of "nurture," was never systematically assessed or even studied. Its crucial role in behaviorist theory remained pure dogma. Kenyeres' mere mentioning the possible role of frequency in the child's mother tongue was already a real innovation.

[90] Holt (1931), Vol.1, p. 16.

[91] We could not present a complete review, but there was also continued work on Polish (for instance Wawrowska 1938, Skorupka 1949) and on Italian (for instance Frontali 1943–1944).

Equally remarkable was the sheer absence of experiment before the 1950s. There had always been elicitation studies, to provoke the child's response. As early in the century as 1904, for instance, the Chamberlains systematically asked their 3-year-old daughter *What is* X *for?* When X was *clock* the child answered *why it 's to wind it up*, when X was *water* she responded *to throw 'tones in.* Similarly, much of the work in the Child Welfare Institutes involved systematic elicitation procedures. The norm studies were sometimes called "experiments," but they were no more than manipulated data collection. Hypothesis testing by experiment, where the effect is studied by manipulation of some independent variable, was absent. This is, no doubt, related to the mentioned absence of a theoretical stance, but the notion of doing psychological experiments with infants and young children was probably not widespread. Charlotte Bühler's experiments in Vienna formed a noticeable exception and we mentioned the limited Russian work of the 1950s. In Chapter 15 we will consider the new experimental approach to child language, which emerged by the end of the 1950s. It is probably also due to this lack of experimental thinking that one fascinating field of child language was completely absent during the period we considered in this chapter: the infant's and child's *perception* of speech. This had to wait for Eimas *et al.* (1971).

George Miller, still a self-declared although not "fanatic" behaviorist in 1951, did report on one behavioristic experiment on child language acquisition. This was Strayer's (1930) training experiment. One of a pair of 1;8-old twins got an elaborate five-week training in object naming, while the other one was kept as a control in a completely speechless environment. Then the roles were switched. The main observation was that the second child learned much faster than the first. Miller concluded: "A child cannot learn verbal responses until he is old enough and mature enough to learn. Maturation sets the pace." (p. 141). Here, Miller meets mentalist Delacroix, who wrote in 1934: "In this way, the initial capital of the child intervenes, its innate mental equipment . . . : it matures in the course of life and certain structures pop up during privileged periods. It is the fault of empiricists to have attributed too often to environment and experience what is the fruit of slow maturation. It is the fault of the apriorists to have forgotten the 'emergence' and the autonomy of development."[92] In retrospect, these citations announce the commotion in acquisition research, which was soon to come over issues of innateness and maturation.

[92] "*Ainsi intervient le capital initial de l'enfant, son équipement mental inné: non point certes qu'il soit donné d'un coup dès la naissance: mais il mûrit au cours de la vie et certaines structures surgissent à des périodes privilégiées. C'est l'erreur des empiristes d'avoir attribué trop souvent au milieu et à l'experience ce qui est le fruit de la lente maturation. C'est l'erreur des apprioristes d'avoir oublié l' 'émergence' et l'autonomie du développement.*" (Delacroix 1934, p. 16).

Chapter 11

Language in the brain: The lures of holism

In 1908 the Paris Society of Neurology became involved in a heated dispute, which lasted over three sessions. It became known as the "Aphasia debate." The main protagonists were Joseph Jules Dejerine and Pierre Marie, both professors of medicine at the University of Paris. The occasion for the debate was a series of no less than 11 papers produced by Marie during 1906/1907. The papers were a direct attack on the standard localizationist theory of aphasia in general and on Broca's "discovery" in particular. Let us first consider these two eminent doctors, Joseph Jules Dejerine and Pierre Marie.

Joseph Jules Dejerine

At the time of the debate, Dejerine (1849–1917) was professor of the history of medicine. He would soon (in 1911) become professor of neurology at the University of Paris. Dejerine, born in Geneva, received his medical training in Paris. When he was *chef-de-clinique* in the *Charité*, he met his wife-to-be Augusta Klumpke (1859–1927), who was on her way to become the first female doctor (MD) in France. Together they published a two-volume treatise on the anatomy of the nervous system (Fig. 11.1). Dejerine's *magnum opus* is the *Symptomatology of the disorders of the nervous system* (1914). With over 1200 pages and 563 figures it became the classic treatise in French neurology. Dejerine discovered an impressive range of neurological syndromes. In the domain of aphasiology his major (1892) discovery was a case of pure word blindness (*cécité verbale*). In this syndrome, the patient cannot read words, but shows no other language-related symptoms. Writing, in particular, is normal. These patients are usually hemianoptic (no perception in the right half of the visual field). Dejerine determined the underlying lesion, a disruption of the connectivity of the angular gyrus. Dejerine was also involved with the analysis of "pure motor aphasia," at that time also called *aphémie*.[1] Here the only symptom the patient exhibits is the inability to produce spoken words, despite an intact articulatory system. The word images ("inner speech")[2] are also intact. The patient can, for instance, tap the number of syllables of a target word. The lesion could not be fully localized, but,

[1] Not to be confused with Broca's aphasia, which Broca called "aphémie."

[2] Notice that this new use of "inner speech," the auditory-motor image of speech, is drastically different from those of Humboldt, Steinthal, and Wundt, which denoted the meaning/thought aspect of speech/language.

Fig. 11.1 Joseph Jules Dejerine and Augusta Dejerine-Klumpke. © Rue des Archives/Mary Evans Picture Library.

according to Dejerine, it was in the close cortical or subcortical vicinity of Broca's region. Dejerine was one of the leaders of the localizational approach to brain functions and very successful in this field.

Pierre Marie

When the aphasia debate took place, Pierre Marie (1853–1940, Fig. 11.2) was professor of anatomical pathology. He would become Dejerine's successor as chair of neurology in 1917. Marie received his MD under the supervision of Jean-Marie Charcot at the Salpetière

Fig. 11.2 Pierre Marie.

in Paris and continued there as Charcot's *chef-de-clinique*. In 1897 he moved to the *Bicêtre*, where he stayed until 1917, when at the age of 64 he moved back to the Salpetière. Like Dejerine, Marie discovered a large number of internal and neurological disorders and syndromes, to which his name is now attached, but not a single aphasic disorder.

Marie's entrance into aphasia research was somewhat indirect. He recruited his many students at the Bicêtre to do clinical and post-mortem brain dissection studies. Initially, aphasic patients were categorized in terms of the standard model, which also guided the anatomical work. Over the course of the years, Marie reviewed some 100 aphasic patients and performed about 50 autopsies, becoming increasingly aware of the misfit between theory, clinical syndrome, and lesion anatomy. Eventually, he began to attack the established view, which he deemed to be "radically false." Central to his claim was that the third left frontal convolution had no special role in the function of language. Broca had been hopelessly wrong on this point. Moreover, Marie argued that Broca's aphasia was just Wernicke's aphasia with a dose of anarthria (an inability to articulate words). This "deconstruction" was executed in a series of papers. The first three of them were published in *La Semaine Médicale* during 1906. Supportive evidence was presented in seven further papers that appeared in the *Bulletin*[3] during 1906/1907. One more paper, a vicious reply to Mr. Grasset, appeared in the *Revue de Philosophie*, also in 1907.[4] We will now turn to the views expressed in these papers and Dejerine's initial responses.

Pierre Marie's "deconstruction"

The first *Semaine* paper was an all-out attack on the standard account of Broca's and Wernicke's aphasia. Marie reminds the reader of what he considers to be fact: aphasia always involves some degree of intellectual damage. Dejerine had also been quite explicit on this point. But the clinical practice in aphasiology was to declare "intelligence is intact." This view perseverated because clinical testing tended to be quite superficial. According to Marie, what is lost in aphasia is a particular type of intelligence: things that one has learned by instruction. These include not only verbal skills, but skills that are not strictly verbal, such as doing arithmetic, telling the (clock) time, executing a recipe, and using conventional mimics. In addition, there is a general loss of initiative.

Wernicke's great discovery was based on an erroneous theory, the paper continues. Following his teacher Meynert, Wernicke had placed the center for audition in the superior temporal gyrus, but this is "radically false."[5] Sensory word images, Wernicke surmised, were stored in that brain region. Then, by a singular coincidence, Wernicke discovered in two aphasic patient autopsies lesions in the parieto-temporal region, which gave birth to the idea of a sensory type of aphasia, as opposed to the motor type proposed by Broca. However, Marie continues to explain, Wernicke had in fact discovered the one

[3] The *Bulletin et Mémoires de la Société médicale des Hôpitaux.*

[4] Most of these papers were reprinted in Marie (1926), which we will use for reference.

[5] "cette idée par laquelle Wernicke, s'appuyant sur la description de Meynert, plaçait le centre de l'audition dans la première circonvolution temporale est radicalement fausse." (Marie 1926, p. 4).

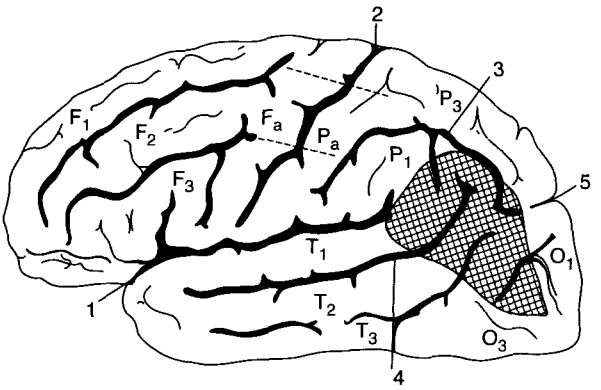


Fig. 11.3 Left hemisphere of a patient with severe Wernicke's aphasia. Reproduced from Marie, P., La troisième circonvolution frontale gauche ne joue aucun rôle spécial dans la fonction de langage, *La Semaine Médicale* (1906), *26*, 241–247, figure 3(c).

and only region of the brain whose damage causes aphasia, any aphasia. "Aphasia is *one*, its localization must be *one* as well, – and that is really the case. The only cerebral region whose damage produces aphasia, is the area called Wernicke's (supramarginal gyrus, angular gyrus and the 'feet' of the first two temporal gyri)."[6] Figure 11.3 depicts the left hemisphere of a patient who according to Marie had suffered from a "very intense" Wernicke's aphasia.

What about Broca's aphasia then? The standard theory was that the characteristic inability to produce words, while preserving comprehension and general intelligence, was due to a lesion of "Broca's area," in the third frontal gyrus (F_3 in Fig. 11.3). This, however, was a myth, according to Marie, a dogma to be dismissed. His argument was simple. There are, first, few documented cases of patients with a circumscribed destruction of Broca's area who do not have trouble with articulated speech. Second, there are well-documented cases of patients with an intact Broca's area who nevertheless suffered from full-fledged Broca's aphasia. Marie concludes: "*the third frontal convolution does not play any special role in language function.*"[7] The fact that Broca's area is often affected in aphasic patients is a trivial consequence of the Sylvian artery's distribution area. This includes both F_3 and Wernicke's area. When a stroke affects Wernicke's area, it may in many cases also affect Broca's area. What then is the real cause of the loss of articulate language in Broca's aphasia? Marie's claim was that this is due entirely to an *anarthria* (and/or *dysarthria*) in the patient. In pure anarthria, the patient cannot articulate speech, without there being any paralysis of the articulatory system or loss of "inner speech." Anarthria is

[6] "Mais, puisque l'Aphasie est *une*, sa localisation doit également être *une*, -- et il en est réellement ainsi. Le seul territoire cérébral dont la lésion produise l'aphasie, c'est le territoire dit de Wernique (gyrus supramarginalis, pli courbe et pieds des deux premières temporales)" (Marie 1926, p. 19).

[7] "*la troisième circonvolution frontale gauche ne joue aucun rôle spécial dans la fonction du langage.*" (ibid. p. 13).

caused by lesions in the left and/or right lentiform nucleus, according to Marie, i.e., in the basal ganglia of the brain. Anarthria is itself not a form of aphasia because language is intact in the patient. Marie then makes the following statement: "*Broca's aphasia is nothing other than an aphasia complicated with an anarthria,*" or the other way round, an "*anarthria complicated with an aphasia.*"[8]

All other variations of aphasic syndromes are variations in severity; they vary "*proportionally with the extension of the lesions in Wernicke's area or the fibers stemming from it.*"[9] A logical consequence of this proposition is that there are no "pure" aphasias, such as pure word deafness and pure motor aphasia. These syndromes were called "pure" because all other language functions were supposedly intact. "Pure aphasias" were the topic of Marie's second *Seminaire* paper.

Meanwhile, the French aphasia community was deeply shaken. Much later, in 1940, Marie remembered "Alas, what hornet's nest did I stir up!!! . . . During the next and following days, I noticed that I had called down a 'major excommunication' upon myself."[10]

Dejerine became the spokesman. He in particular could not accept Marie's claims, since he had published some of his best and most seminal papers on "pure" aphasias, in particular word blindness.[11] He immediately published two papers in defense of the traditional position on sensory and motor aphasia. Let us examine these responses before turning to Marie's second paper.

Dejerine (1906a) began his defense of the classical theory by presenting a short review of its history, from Bouillaud to Wernicke and Charcot's school in France. They developed an advanced theory of sensory aphasia, which resulted from lesions in Wernicke's area. Although one often observes intellectual defects in sensory aphasias, this was not an essential or defining trait. According to Dejerine, Marie's theory could not explain the specific characteristics of this type of aphasia, such as paraphasias and jargon. And it had no way of explaining the "pure" sensory aphasia word deafness and word blindness—"pure" because the other language functions were not affected. So the explanation of this type of aphasia must involve reduced accessibility of auditory or visual word images. In a second paper, Dejerine (1906b) focused on motor aphasia and the role of Broca's area. After a critical evaluation of Marie's positive and negative cases, Dejerine concluded that the available evidence was indeed insufficient to substantiate the claim that articulate language is uniquely and purely located in Broca's area. Adjacent cortical and subcortical regions may also be involved. But, Dejerine concluded, one can be certain that in that

[8] "*l'aphasie de Broca n'est autre chose qu'une aphasie compliqué d'anarthrie*" . . . "une *anarthrie compliquée d'aphasie.*" (Marie 1926, p. 18).

[9] "En un mot, *l'intensité de l'aphasie est proportionelle à l'étendu des lésions de la zone de Wernicke ou des fibres qui en proviennent.*" (p. 22).

[10] "Hélas, dans quel guêpier venais-je de me fourrer!!!" "Et, de fait, le lendemain et les jours suivants, je constatais que j'avais encouru "l'excommunication majeure."" (Fragments of Marie's 1940 letter cited by Alajouanine 1952, p. 756).

[11] Dejerine published his discovery of pure word blindness in Dejerine (1891, 1892).

zone there is "*a region whose lesion determines a motor aphasia* and that without any affection of the temporal lobe."[12] He then proceeded to attack Marie on the claim that pure motor aphasia is merely anarthria, resulting from a lesion in the lenticular zone. The two are completely and totally different, he stated. A pure motor aphasic can utter only two or three words, and these are always the same ones. The anarthric initiates the articulation of *all* words, but then fails to produce them. The first has lost the motor images, while the second has not. Dejerine presented two further cases of pure motor aphasia, without any other noticeable defects of language or intelligence. Both autopsies showed lesions in Broca's area and the adjacent region, with entirely preserved temporal lobes.

Marie's second *Semaine* article immediately responded to these papers, i.e., it addressed the question of the so-called "pure aphasias." The paper starts with expressions of irritation about the "general tone" and distortions of fact in Dejerine's papers. Dejerine "accuses me of setting the issue of aphasia '*back by more than eighty years.*' Alas! If I have dedicated the last ten years to the study of aphasia, it was certainly in the hope of arriving at a different result than to set back science by eighty years."[13] Marie then cites Dejerine on cases published by others, accusing Dejerine of distorting the original case descriptions. It is "my right to require of him to place the materials of the proceedings in their entirety under the eyes of the physicians who read us."[14]

Next Marie turned to the three kinds of "pure aphasia" reported in the literature (Lichtheim had called them "subcortical" aphasias): pure word deafness, pure word blindness, and pure motor aphasia. Marie's essential claim was that none of these exists. Beginning with word deafness, Marie stated: "*I have never observed a case of pure word deafness* . . . I deny that one can observe a case of pure word deafness with absolute preservation of intelligence, reading and writing, and with auditory acuity intact."[15] Marie went on to review the reported cases, including Dejerine's own. None of the patients were entirely free of other aphasic symptoms. The whole notion of a pure syndrome is a "myth," based on the "false assumption" of an auditory center in the posterior region of the first temporal convolution (T_1). (That assumption was almost correct, we now know. It was Henschen (1918) who localized it in Henschl's transverse gyrus in the lateral sulcus of T_1.) There are simply no auditory images stored in that region, according to Marie. If that were the case, then polyglots would have enormously developed posterior regions, which is not the case. One suggested way out is to propose that only auditory syllables

[12] "On peut être certain, dis je, qu'il y aura toujours dans cette zone *une région dont la lésion détermine l'aphasie motrice* et cela en dehors de toute altération du lobe temporal." (Dejerine 1906b, p. 453).

[13] "M. Dejerine . . . m'accuse de ramener la question de l'aphasie "*à plus de quatre-vingt ans en arrière.*" Hélas! si j'ai consacré ces dix dernières années à l'étude de l'aphasie, c'était certes avec l'espoir d'arriver à un autre résultat que de faire reculer la science de quatre-vingt ans." (Marie 1926, p. 32).

[14] "Mais ce que j'ai le droit d'exiger de lui, c'est que toutes les pièces du procès soient mises intégralement sous les yeux des médecins qui nous lisent." (ibid. p. 32).

[15] "Au point de vu clinique, *je n' ai jamais observé un cas de Surdité verbale pure* . . . je nie qu'on puisse observer un cas de Surdité verbale pure avec conservation absolue de l'intelligence, de la lecture et de l'écriture, et acuité auditive intacte." (ibid. p. 34).

are stored there, but then all words would have to be assembled from these syllables. This, however, is an intellectual operation, which can only take place in Wernicke's area, according to Marie.

The phenomenon of word blindness may exist, Marie writes, but then Dejerine's analysis of such alexia is wrong. First, reading ability is obviously not an innate human capacity and hence there can be no dedicated brain region for visual word images. Dejerine's localization of such a center in the angular gyrus is unfounded. The right hemianopia involved in these cases indicates a lesion in the left hemisphere's visual system. The linguistic aspect of the disorder is indicative of a lesion in Wernicke's area. This combination of defects, Marie surmises, may result from an obstruction in the posterior cerebral artery. That can cause simultaneous infarctions in the white matter beneath Wernicke's area and in the (visual) lingual and fusiform gyri in the inferior temporal lobe (T_3 in Fig. 11.3). The lesion Dejerine observed in the angular gyrus is not relevant; it is just an extension of the essential white matter damage.

Finally, Marie turned to the case of presumed pure motor aphasia, which Lichtheim attributed to a disconnection between Broca's area and the spinal nuclei. In pure motor aphasia all language and intellectual functions are preserved, except for the spoken expression of words. This was Dejerine's description of the syndrome, but Marie claimed it was just anarthria. It has nothing in particular to do with words, but with the execution of articulation. It is not a paralysis, but more akin to stuttering. Marie denied it was aphasia. Anarthrias are due to lesions in the lentiform nucleus. Marie also repeats his theorem: Broca's aphasia = anarthria (with lesion in the lenticular zone) + aphasia (with lesion in Wernicke's area or the fibers proceeding from it). In his 1940 letter, Marie remembers that right after his attack on Broca's law, he was challenged to produce an alternative theory of Broca's aphasia. "I had only wanted to disentangle a dogma. One required of me to establish a complete theory of aphasia, something I had never thought about!"[16] He then quickly came up with the anarthria story, inviting another round of attacks.

The third *Semaine* paper discussed how the standard view had arisen. Marie re-examined the well-preserved brains of Leborgne and Lelong, Broca's first two cases. The first showed extensive damage extending into Wernicke's area. The second was ill-diagnosed by Broca, according to Marie. The brain of 84-year-old Lelong shows all the signs of senile dementia, with atrophies but no lesions. F_3, in particular, was intact. Marie concluded that these cases were the basis of the doctrine that the third convolution was the seat of Broca's aphasia. We know better. In 1861 Broca attached no value whatsoever to F_3. His only concern was to establish a case for or against the existence of a mental faculty localized in the anterior brain. Marie fabricated the story that Broca's real purpose was to confirm Gall's localization of the language faculty. He reproduced some of Gall's plates in which the memory for words was localized (one of them is the left diagram in our Fig. 3.2a, with the language faculty behind the right eye case). The localization by and

[16] "Je m'étais proposé uniquement de démêler un dogme. On exigeait de moi d'édifier une théorie complète de l'Aphasie, chose à laquelle je n'avais jamais pensée!" (Alajouanine 1952, p. 756).

large conforms to F_3. "Gall is the direct ancestry from which proceeds the localization of language in the third fronal gyrus!"[17] We know that Broca was not pursuing such an agenda. Broca had started out as a Flourens disciple, not believing in the localization of faculties in the brain's convolutions. Instead he thought there were a few "grand regions" in the brain, which corresponded to the "grand regions" of the mind. He departed from that view in his 1861 paper, recognizing the possibility of localization of faculties. But even in 1863, when he reported on six more autopsies of aphasics with loss of articulate speech, all showing damage to the third convolution, he still did not claim the discovery of a center for articulate speech: "I don't dare to draw a conclusion from that and I wait for new facts."

Marie portrayed in his paper the emergence of a fad, "Broca's law." The grand physicians such as Charcot, who had first attacked it, ultimately all embraced it. The myth of centers with sensory, motor and visual word images became a generally accepted dogma. It was Bouillaud's ultimate victory, according to Marie, and the scientific crowd followed: And what about the "army of followers? It was the *crowd*, the crowd with its instinct for guessing and its deep ignorance, the crowd both incredulous and believing, especially if the object of its belief is something extraordinary and miraculous."[18] Had not Marie complained of the "general tone" of Dejerine's papers!

It was in this atmosphere that the Society of Neurology of Paris decided to settle the debate amongst its quarreling members, a wise decision indeed.

The aphasia debate

The Society held three meetings, a few weeks apart, during the months of June and July 1908. There were some 25 participants, among them of course the main protagonists Pierre Marie and Joseph Dejerine. The meetings were carefully organized and chaired. There was an abundance of case reports and once an anarthria patient was brought in to demonstrate that he could sing. The proceedings were exemplary in their precision and detail,[19] but they did not bring resolution. Both parties stuck to their position without budging an inch. For example, Dejerine presented a sharp rebuff to Marie's analysis of the brain and clinical status of Broca's second case, Lelong. This brain was the best example in the literature of a local and deep distortion of F_3; Broca had done a perfect job. The clinical picture was not subject to any doubt: this was not a case of senility whatsoever, but was a classical case of Broca's aphasia. This was responded to by Marie's intern François Moutier, who rejoined that there was no cortical lesion in the entire brain:

[17] "Et cependant Gall est l'ascendant direct dont procède la localisation du langage dans la troisième frontale!" (Marie 1926, p. 79).

[18] "Et que dire de l'armée qui le suivait? C'était la *foule*, la foule avec son instinct divinatoire et son ignorance profonde, la foule à la fois incrédule et croyante, surtout si dans l'objet de sa croyance il y a quelque chose d'extraordinaire et de merveilleux." (ibid. pp. 89–90).

[19] The debate was published in three issues of *Revue Neurologique* (1908). See Bibliography, Aphasia debate (1908).

"*the foot of the 3rd frontal is entirely intact.*"[20] Dejerine repeatedly asked to appoint a committee to perform one critical section in Lelong's brain, which was under the trust of the chair of pathology, Marie. Marie did not even answer the request. More important were the extensive discussions on Marie's "lenticular zone." Lesions there were the cause of anarthria in Marie's opinion. Madame Dejerine presented a full, sophisticated survey of what Marie's "lenticular zone" involved in the brain. Marie had never indicated that the lenticular *nucleus* was the critical site, but instead said it was something called the "lenticular zone." From Marie's and Moutier's publications it appears that the "zone" includes no less than one third of the left hemisphere, and even includes the two extremities of F_3. This "*is not localization,*"[21] Joseph Dejerine concluded.

When Marie was asked whether a lesion in the lenticular nucleus per se would cause anarthria, he answered that he believed so. Could he supply an actual case? No, he could not. Still, as far as the relevance of Broca's area was concerned, Marie was not at a loss for relevant facts. He reported three surgical cases, whose gruesome character was not an issue in the discussion. Surgeon Burckhardt decided to treat two "maniacs" with "verbiage" by ablation of their Broca's areas, in that way "silencing" them and "providing them with a favorable calmness."[22] However, as soon as they recovered from their chloroform anesthesia, they talked with the clarity they had previously, without a trace of aphasia. Both of them were right-handed, Marie added for completeness sake. The same surgeon had, on another patient, removed the posterior parts of the first and second left temporal lobes, which did not cause "a trace of word blindness." This, however, was not the whole story of surgeon Burckhardt, as we will see.

The final part of the third session was dedicated to the clinical picture, the psychology of aphasia. Marie attacked the notion of "verbal images" and centers, and that they could be localized. A discussion arose on the definition of "verbal image." Is it the memory trace of a sensation? Is it the mental representation of a word? Neither of those, according to Marie. "It is just words," "psychological speculations in which the words occupy more place than the facts."[23] But Marie failed to respond to the repeated question regarding the precise character of the specialized intellectual function of Wernicke's area. It is too general to speak of "things learned by didactic procedures," Dejerine remarked. An aphasic does not lose *all* those things. And then, more specifically, how do such patients become paraphasic? The classical theory provides a (self-monitoring) explanation; what was to be Marie's story? The answer was: "I don't know the mechanism of paraphasia and I consider as unfounded hypotheses whatever the classical doctrines have pretended to teach

[20] "*le pied de la IIIe frontale is absolument intact.*" (Aphasia debate 1908, p. 1011).

[21] "une localisation si étendue, qui comprenne le sector moyenne de l'hémisphère, *n'est pas une localisation*" (ibid. p. 976).

[22] "il réduirait ses malades au silence et leur procurerait ainsi un calme favorable" (ibid. 1908, p. 1046).

[23] "Ce ne sont là que des mots," "spéculations psychologiques où les mots tiennent plus de place que les faits" (ibid. pp. 1039, 1041.)

us on this subject."[24] Dejerine then addressed Marie's evolutionary argument against the existence of an innate center for visual word images, somewhere in the angular gyrus. There is no innate, preformed center for *any* intellectual function, Dejerine claimed. They all emerge from experience.

Marie's concluding observation was that none of the distinguished colleagues had provided any clinical evidence for the existence of the classical language centers. Given the great talent assembled, he suggested, this in itself was the most obvious demonstration of the non-existence of these centers. Dejerine, in contrast, concluded that the traditional doctrine had emerged as victor.

The aftermath

Progress in science is always a balance between evolution and revolution. Marie chose revolution. Henry Head later called him the iconoclast. Marie's attack on the currently dominant paradigm was ferocious. There is only one kind of aphasia, an intellectual disorder, he claimed. The critical location is Wernicke's area. Broca's area plays no role in language function. Word blindness does not exist. Word mutism (Broca's aphemia) does not exist. There are no regions that store sensory or motor word images. Associationist accounts of aphasic syndromes make no sense. Broca's aphasia is just aphasia complicated with anarthria. Anarthria is caused in the lenticular zone. Marie made himself an easy target. Still, his revolt did not remain without effect. It engendered a renewed interest in Broca's area, its delimitation, and its various cytoarchitectonic subregions and their functions—an active field of research till the present day.[25] More generally, his assault on strict one-region-one-function localism would reverberate throughout the century. Time and again, aphasiologists would appeal to Marie when defending holism as opposed to associationism. And some of Marie's claims became canonical in aphasiology: there is always some comprehension deficit in aphasia and there is always some adverse effect on intelligence.

In France, the scene was somewhat split. There was, on the one hand, the opposing camp of Dejerine and former students, such as Joseph Grasset in Montpellier and Belgian Albert Mahaim.[26] But many others were, to a greater or lesser degree, influenced by Marie's ideas. The aphasia debate was essentially Marie's final contribution to aphasiology. His "intern" (as Marie always called him) François Moutier (1881–1961) systematically wrote up Marie's ideas for his dissertation, which became the two-volume 1908 book *l'Aphasie de Broca*. It is a faithful, but also highly readable and systematic, account of Marie's views, supported in Volume I by a rich supply of patient data (jointly obtained observations and data from other authors) and in Volume II by Moutier's own observations.

[24] "Je ne sais pas quel est le mécanisme de la paraphasie, et je considère comme hypothèses sans aucun fondament tout ce que les doctrines classiques ont la prétention de nous apprendre à ce sujet." (Aphasia debate 1908, p. 1041).

[25] See, for instance, Hagoort and Levelt (2009).

[26] After Paris, Mahaim went to Monakow and Forel in Zürich and later became professor in Lausanne.

Moutier's historical review in Volume I is especially attractive. It is less biased that Marie's own conjectures and it contains the most comprehensive account I have seen of the many, many diagram makers that were active by the end of the nineteenth century. Lecours and Joanette (1984), in a beautiful account of Moutier's life and work, relate his unhappy relationship with Marie. Moutier could have expected to make a brilliant career after the monumental dissertation he had published, but Marie prevented it. He told Moutier, soon after the aphasia debate, that he should leave neurology and return to gastroenterology, an area he had worked in before. This in fact ruined his academic career. He did move to gastroenterology and produced an impressive number of monographs and papers on the subject. But even in this area, Marie, who was after 1917 in a position of substantial power, did not support Moutier's further advancement. Was Marie jealous because Moutier had written the book he should have written himself? It might indeed be so. Once he said about Moutier: "He has made my ideas disappear under a paving stone."[27] What is the sad moral? Never be faithful to your supervisor.

Among others influenced by Marie was psychologist Louis Barat, who took a holist position on the intellectual functioning of the brain. Language, music, and arithmetic all use the same general mechanisms, he claimed, which are to a large extent based on habit formation.[28] Dejerine's former student Théophile Alajouanine was also influential. In 1939 he published a monograph on phonetic disorders, co-authored with André Ombredane and Marguerite Durand. It builds on the old controversies between Dejerine and Marie. Dejerine (1891) had distinguished a subcortical form of motor aphasia. Such patients cannot pronounce any word, but have not lost inner speech; they can, for instance, tap the number of a word's syllables and often can write the word. There is, on the other hand, no paralysis of the articulatory muscles. Marie, however, had lumped all this together as anarthria, a non-intellectual motor disorder with its source in the lenticular formation. The monograph describes four cases of "phonetic disintegration." In the early stage these patients have a paralytic type of articulation, but then they recover, or rather adapt by developing an excessive, brusque way of articulating. They evade nasalization. Especially in longer words, they tend to assimilate sounds and to simplify consonant clusters. The purpose of the monograph was to define a precise syndrome that can, at least in part, replace the vague and controversial notion of dysarthria.

André Ombredane, who worked with Alajouanine in Paris, produced in 1933 a chapter on language in Dumas' six-volume handbook of psychology. It contains a diagram of regions in the left hemisphere whose lesions can cause aphasias. Broca's area is not one of them. Ombredane joins Marie, Hughlings Jackson, and Head in considering aphasia as an intellectual deficit, but unlike Marie, Ombredane was deeply interested in the underlying psychology. We will return to both Alajouanine and Ombredane in Chapter 15.

The reception of Marie's claims was at best tepid outside of France. The revolutionary appeal was noticed, though, and people felt an urge to respond. The New York and the

[27] "Il a englouti mes idées sous un pave." (Lecours and Joanette 1984, p. 225).

[28] Barat (1917).

Philadelphia Neurological Societies held a joint meeting in Philadelphia on 24 November 1906, i.e., right after the appearance of Marie's second 1906 paper. Charles Mills (1845–1931) of the University of Pennsylvania, "the dean of American neurologists," presented Marie's case and rejected it completely. Mills took sides with Dejerine and criticized the tendency "in some quarters" to "rebel against the extreme differentiation of the brain into areas and subareas and centers." The facts, rather, "would seem to show that this differentiation is even much greater than has been supposed by the most ardent believer in localization." (1907, p. 461). In the following discussion, according to the proceedings, Dr. Starr sided with Mills. Dr. Lloyd expressed the opinion that although Marie's papers were "not deserving of all the attention," he had a point in protesting against the "fine-spun schemes" of the "diagrammatic school" (p. 469). Dr. Dercum took sides with Marie by contending "that the intellectual deficit maintained by Marie must be unhesitatingly admitted" (p. 472) and Dr. Fraenkel argued that "pure" cases are exceedingly rare. One relevant person, it seems, did not participate in the meeting. This was Adolph Meyer (1866–1950), who was at the time director of the pathological institute of New York's State Hospital. The year before, in 1905, he had published an extensive paper on aphasia in *Psychological Bulletin*. It was a faithful review of the Wernicke–Lichtheim–Dejerine "standard theory." There was nothing like a revolt among United States aphasiologists.

In England, surgeon and ophthalmologist Nottidge Charles MacNamara (1832–1918) published a book entitled *Human speech* (1908), which was not really about speech but rather about the evolution of the brain and its intellectual functions. MacNamara took issue with the claims of Marie and Moutier with regard to the irrelevance for speech of Broca's area, which he rejected all-out. But Henry Head would soon turn the British scene.

In Germany, followers of Marie were rare. One of them was Erwin Niessl von Mayendorf of Leipzig University. In 1930 he published a monograph describing a case of Broca's aphasia in a patient with fully intact Broca's areas in both hemispheres. Despite this monograph, the Paris meeting did not affect mainstream aphasiology in Germany; the evolution of the existing paradigm remained the dominant approach. Within a year, the Berlin neurologist Liepmann (1909) published a detailed and devastating review of Marie's and Moutier's claims, which we will discuss.

Marie published one more paper on aphasia in 1922, which repeats at length the discussions we have seen unfold. But there is an interesting final note. It concerns the old notion of verbal centers, in particular ones that had been quite recently suggested: centers for nouns, for verbs, for adjectives, etc. This is not the way to think about the brain, Marie wrote. There are too many distinct psychological processes to be handled by such dedicated groups of cells. Instead, psychological processes correspond to *vibrations* in different regions of the brain. Any brain cell can take part in numerous different combinations of vibration, the total of which constitutes the psychological process. Thus, the same nerve cell can be "solicited by an infinitude of different vibrations and in this way participate in a large number of different psychological processes."[29] There is a modern

[29] "De telle sorte qu'une même cellule peut être sollicitée par une infinité de vibrations differentes et participer ainsi à un grand nombre de processus psychiques différentes." (Marie 1926, p. 139).

touch to this argument, formulated just a few years before Hans Berger was to measure the first electroencephalogram in man.[30] It also reflects a widespread trend in aphasiology before the cognitive revolution towards holism. Marie's revolt had been intended to undermine the localizationist paradigm. Aphasia is one, in Marie's own terms, and it is not a linguistic, but an intellectual defect, involving just Wernicke's area. In this final paper, Marie claimed that the brain's functioning is equipotential, involving overlapping vibrations emanating from different zones in the brain. In the United States equipotentialism was propagated by Karl Lashley (1929) and his followers. According to this view, the brain functions by "mass action," one part can take over the damaged functioning of another part (i.e., it is "equipotential"). In Europe *Gestalt* psychology invited holism in aphasiology. Kurt Goldstein became one of its main exponents, as we will see.

One final aspect of Marie's influence should be mentioned. We saw that Marie dismissed and clearly hated psychological, mentalist theories. In that respect he was a behaviorist *avant la lettre*. Only "facts" are relevant. He did not have the slightest interest in the phenomenology of aphasic syndromes or in psychological processing accounts. We saw him dismiss any effort to explain paraphasias. "Loss of didactically acquired processes" was all he was willing to grant. Both Marie's holism and occasionally his anti-psychologism did spread among French aphasiologists. But it also provoked opposition, especially in German aphasiology. It was Arnold Pick, to whom we will turn soon, who developed a new psychological approach to aphasiology. He had already rejected Marie's "intelligence trouble" theory in his paper at the 1908 Experimental Psychology Congress in Frankfurt,[31] in a symposium on language understanding organized by Karl Bühler. Let us first, however, consider an immediate German response to the aphasia debate.

A German response: Hugo Liepmann

The German tradition provided receptive grounds neither for Marie's rejection of the Wernicke–Lichtheim model nor for his anti-psychologism. Several former students or assistants of Wernicke had by then established leading positions in German aphasiology and most of them were continuing Wernicke's approach, although without the dogmatism which had been Marie's quixotic target. Indeed, being dogmatic was not a characteristic of Wernicke's students, such as Bonhoeffer, Heilbronner, Kleist, Goldstein, Liepmann, Sachs, and Storch. Hugo Liepmann (1863–1925), in his (1909) response to Marie and Moutier, wonders: "Where is the rigid dogma, which embraces the majority of the leading minds?"[32] He went on to review the varied opinions among the mentioned colleagues. There had been continuous testing and revision of Wernicke's ideas, driven by advances in brain anatomy and physiology, cellular histology, and psychology. The general

[30] His first measurements were from 1924. It was his 1929 publication which, still slowly, grabbed the general imagination.

[31] Pick (1909), pp. 30–31.

[32] "Wo ist das starre Dogma, in dem die Mehrheit der maßgebenden Köpfe befangen wäre, denen der dröhnende Weckruf entgegentönen müßte: "Auf zur Revision!"?" (Liepmann 1909, p. 450).

approach had been to "patiently expand the theory." It is "cheap" of Marie to require "absolutely compelling force of proof" (p. 452).

In his paper, Liepmann reviewed some of the classical clinical cases of word deafness, showing that the clinical case is real, and that it results from lesions in the temporal lobe. What had changed was the clinical interpretation. The psychological defect was the *synthesis* of the word sound from the acoustic stimulation. Speech understanding is a staged process, going from word sound perception to word meaning comprehension to higher integration into sentence meaning. Each of these could be affected in aphasia, but this "is unknown to Mr. Marie" (p 453). For him it was all just a weakening of intelligence. Recently, however, Marie had retracted, Liepmann claimed. He and Moutier now talk of "an intellectual disorder for language,"[33] which is precisely the classical doctrine.

Liepmann then turned to Broca's aphasia and Marie's two-pronged attack. First, are there cases of damage to Broca's area, without Broca's aphasia? Liepmann discussed the various anatomical delineations of Broca's area in the literature and the (ab)use made of them in the argumentation for or against the involvement of Broca's area. It is not only the convolutions, but also the cytoarchitecture that should be considered, he argued, and one should always be explicit on the fine details of a lesion. Still, this first argument of Marie, the existence of damage to Broca's area without the patient showing Broca's aphasia, is of little relevance. A patient who slowly recovers from Broca's aphasia still shows damage in Broca's area. And a patient who happens to be right-brained for language will not develop aphasia if Broca's area in the left hemisphere is damaged. But that was precisely Marie's "knock-down" argument: Burckhardt's surgical cases, in which extirpation of F_3 apparently did not cause motor aphasia. Here Liepmann went to the source. He retrieved Burckhardt's (1891) report and, first of all, castigated the surgeon's good judgment: "There is no need to waste a word on this method of therapy."[34] Burckhardt had asked himself: "Couldn't one take away the impulsive motor element from her brain mechanism and in that way turn the patient from an excited demented person into a quiet one?"[35] Liepmann then reported some of the gruesome details. The 51-year-old woman had gone through four successive operations. In the first one Burckhardt removed a strip from her right parietal lobe. The second operation extended the strip into the temporal lobe. The third operation removed a strip from the left supramarginal and triangular lobes. The patient was now "more quiet" and "word deaf." During the fourth operation a piece was extirpated from Broca's area. And, yes, the patient became a "quiet demented person," without loss of speech. This book is not a medical history, suffice it to mention that there were five more such Burckhardt victims. Moutier (1908, pp. 323–325) provides an extensive, unmoved report on all of them. Liepmann rejected Burkhardt's case as of being without any scientific relevance. The clinical description is "ridiculously bad"; there

[33] "ein trouble intellectuel *pour le langage*" (Liepmann 1909, p. 455).

[34] "Es ist wohl nicht nötig, über diese Heilmethode ein Wort zu verlieren." (p. 467).

[35] "Könnte man nicht dieses motorisch impulsive Element aus ihrem Gehirnmechanismus wegnehmen und dadurch die Patientin aus einer aufgeregten zu einer ruhigen Dementen machen?" (p. 467).

are neither photographs nor drawings; and there is no report of any psychological testing. And, finally, extirpated from F_3 was an amount of 1.5 g, whereas the critical area weighs about 20–30 g. The other Burckhardt F_3 extirpation reported by Marie involved 2.5 g. Hence, most of Broca's area had been spared in these two surgical cases and Marie's use of them was misguided.

More important, according to Liepmann, was Marie's second claim: There can be Broca's aphasia without damage to F_3; what it must involve is damage to the so-called "lenticular zone." Liepmann then reported in detail on 11 Broca cases of his own, real "word mutes." In nine of them there was major damage to F_3. One patient, who quickly recovered, showed damage extending into F_3. The other case was a total aphasic. Here F_3 is spared, but completely isolated from its periphery in terms of both its input (in particular from the arcuate fascicule) and its output (the bulbar nuclei). Two of these 11 patients were practically "pure" Broca's with lesions largely restricted to F_3 and not involving the lenticular zone, contrary to Marie's predictions. Liepmann finally reanalyzed all of Marie/Moutier's 12 cases with major damage to the lenticular zone. One should expect them to be word mute, "anarthric" in Marie's sense. But surprisingly, none of them were. They indeed have little spontaneous speech, but all of them can *repeat* words.

Liepmann concluded that "aphasiology and, more generally, brain pathology must build on the basic principles provided by Broca and Wernicke."[36] Indeed, that had been the approach in mainstream German aphasiology all along. It did not provide a climate for Marie-type "revolutions." Let us consider this early twentiethth-century German-language scene in some more detail.

The continuing German tradition

The Wernicke–Liepmann tradition was indeed still alive and kicking in German aphasiology. Let us consider its main representatives.

Carl Wernicke

Wernicke himself published his last major paper in 1903, under exactly the same title as his original monograph, *The aphasic symptom complex.* It opened with the presentation of a "pure" case of agraphia: a 46-year-old right hemiplegic woman, who had no noticeable language disorders, except for a slight degree of anarthria. She could read normally, but could not write. On first view, this patient provided the ideal argument for the existence of a "graphic" center in the brain, as Exner and Bastian had proposed. Wernicke used the occasion to deny the existence of such a center and to sketch the state of the art of localizing language functions in the brain. Such a "Broca" center for writing movements (supposed to be in the foot of F_2) would receive its instructions from the word sound images, which determine the letter ordering and grouping. Hence, the "word notions"

[36] "Die Aphasielehre hat weiter auf den Grundgedanken zu bauen, die ihr Broca und Wernicke gegeben haben." (p. 484).

(*Wortbegriffe*) should be intact. That was Wernicke's term for the dual sound-motor image of a word. It is not the sense of a word, but the form of a word in sensory-motor memory (French aphasiologists also called it the patient's "inner speech," as we saw, a term also adopted by Monakow in Switzerland). One test for its intactness is the patient's ability to indicate the number of syllables in a word (by tapping or counting). Wernicke's patient failed that test and other similar ones. Her agraphia apparently did not stem from a distorted center for writing movements, with preservation of input from the word notions.

To handle the case, Wernicke reviewed the latest developments in the Wernicke–Lichtheim approach, sketching a general convergence among colleagues on both the clinical symptomatology and the neuroanatomy of language disorders. We will not follow this in detail. For Wernicke himself it was important to reconsider two components of the original model. The first one was Wernicke's claim of the existence of a transcortical motor aphasia. Authors such as Kussmaul and Bastian had treated as almost self-evident that speaking impulses from the "conceptual center" can only reach Broca's area via the center of auditory word images (see Fig. 3.8). This was not the case, Wernicke argued, because articulated speech can be preserved upon destruction of the sensory speech area. The speech will only be paraphasic because of the lack of control from the auditory center. In other words, the speech motor area can, at least in adults, directly receive "transcortical" impulses from the "conceptual center" (which is not strictly localized according to Wernicke). The other component was a reconsideration of conduction aphasia. Wernicke admitted that convincing evidence for a "pure" case of conduction aphasia had not yet been forthcoming. Such a case should show disruption of the direct connection between the sensory and motor speech centers, in particular the insula and arcuate fascicule, with clinical symptoms including the inability to repeat words, diminished self-monitoring in speech, and preservation of speech perception. However, Wernicke argued, if transcortical connections exist from the auditory word center to the conceptual center, and from the conceptual center to Broca's center, then repeating a word should also be possible via this conceptual round-about. This requires a refinement of the repetition test. The test should exclude this round-about. Transcortical repetition will often be paraphasic (i.e., replacing the heard word by another, related one). The "pure" conduction aphasic, however, will not show immediate "echolalia" of new, nonsense words, whether or not there is a round-about route. The echoing of senseless words requires the *direct* connection between the two centers and that is what should be tested.

Not discussed anywhere in the paper is amnesic aphasia, the syndrome that did not "fit" in Lichtheim's house. Wernicke had probably definitively accepted Grashey's interpretation (as discussed in Chapter 3).

Wernicke extensively cited Dejerine[37] in Paris, who was a marked exponent of the Broca–Wernicke tradition, and Monakow in Zürich. Both of them had worked in the field of reading and writing disorders. Like Wernicke, they both rejected a "graphical center" and considered agraphia a "transcortical" phenomenon. Writing movements are

[37] Always written as "Déjérine" by Wernicke.

just hand movements, but they receive, transcortically, their instructions from a region of the brain where the memory images of spelled words are stored, i.e., an optical word center. The innervation of the optical center never comes straight from the "conceptual center" in Lichtheim's house (Fig. 3.8), but only from the "word notions," in particular the center of auditory word images located in Wernicke's area. If this connection or the word notions themselves are disrupted, agraphia results. But Wernicke disagreed with Dejerine on the unilateral angular gyrus localization of such an optical word center. With Monakow, he believed that optical memories for spelled words are bilaterally located. We will not follow Wernicke's detailed neuroanatomical treatment of these written language issues. In his paper, Wernicke reproduced the brain maps by both Monakow and Dejerine indicating the language areas in the Sylvian region. He happily concluded that the views of these men had converged with his own. Wernicke, who did not entertain personal relations with either gentleman, certainly considered them as like-minded leaders in the field.

Was Wernicke a "localist"? The answer to this question can best be illustrated in terms of Lichtheim's house. Below the "roof," Wernicke was an outspoken localist. Broca's and Wernicke's regions are dedicated speech regions, whose destruction causes specific motor and sensory aphasias, respectively. But Wernicke always resisted the temptation to localize the "top of the roof," the conceptual center, or the "transcortical" connections between the two speech regions and the conceptual center. When it comes to intellectual functions, it is not wrong to call Wernicke a globalist. This is not the same as being an equipotentialist. Wernicke never suggested that "intellectual" brain regions are exchangeable in function. Instead, the complexity of intellectual function must involve a complex neurological network.

Wernicke's assistants

The German tradition was especially carried on by Wernicke's students and coworkers previously mentioned. Apart from Hugo Liepmann and Heinrich Sachs (1863–1928), they were Karl Bonhoeffer (1869–1948), Wernicke's successor in Breslau, Karl Heilbronner (1869–1914), Karl Kleist (1878–1960), and Kurt Goldstein (1878–1965). We will first consider Sachs, Bonhoeffer, and Heilbronner, and later return to Kleist and Goldstein, who would have major roles to play.

Wernicke's views were slightly elaborated in a monograph by his assistant Heinrich Sachs (1905). Sachs stressed that the whole precedes the parts. Listening to speech is not going, step by step, from elementary sensory impressions to word images in memory. Rather it starts with the global activation of a complex associative network, usually by listening to a whole sentence. Only after that do the elements become conscious. Sachs acknowledged Wernicke's care in stressing the "transcortical" nature of concepts. Wernicke had always understood the conceptual center "as encompassing the total cortex."[38] Sachs essentially adopted Wernicke's updated views on the localization of

[38] "dass Wernicke das Begriffszentrum niemals anders verstanden hat, als den Inbegriff der gesamten Grosshirnrinde." (Sachs 1905, p. 27).

centers and their connections, but stressed the associative (i.e., "spreading") activation of the network as a whole in both the perception and production of speech.

Karl Bonhoeffer was Wernicke's assistant and later successor at Breslau. In 1912 he moved to the Charité in Berlin, where he stayed as a professor of neurology till the end of his career. That end was during World War II, during which Bonhoeffer was often outspoken about the misuse of psychiatry (see Chapter 14). In 1902 he published two cases of transient motor aphasia, resulting from brain surgery that had slightly affected the left inferio-frontal region. One of them, epileptic patient Mr. Rieger, could not speak after the operation, but then went through a phase of agrammatism during recovery. During that phase his speech was telegraphic, with phonetic paraphasias (such as *Tetschendach* for *Taschentuch* – "handkerchief"). Bonhoeffer concluded that the speech motor center is the site of grammatical notions. That agreed with Monakow's view (to be discussed), but conflicted with the views of Pick and Heilbronner. Pick, in his 1898 chapter on agrammatism, had claimed that the syndrome results from lesions in Wernicke's area. He kept to this view in his 1913 monograph on agrammatism, to which we will turn presently.

Karl Heilbronner, who had spent five years with Wernicke at Breslau as his assistant, originally shared Pick's view, but gradually changed his mind after Bonhoeffer's paper. Heilbronner moved to the chair of psychiatry in Utrecht in 1903, where he stayed till his sudden death in 1914. Liepmann's obituary on that occasion provides a rare glimpse into Wernicke's students' response to the master: "As all well-known students of Wernicke, he [Heilbronner] later liberated himself from the steepness and the grandiose one-sidedness of his teacher and modified his doctrine by adopting non-Wernickian elements."[39]

In 1906 Heilbronner published a paper on agrammatism, discussing a patient by the name of Paul V., who had suffered a stab in the left temple area. His speech was slow, with infinitive-type telegraphic style (*Kaffee trinken Morgen, Pille, Frühstück esse, . . .* – "coffee drink morning, pill, breakfast eat"). He made errors in the gender of articles and there were word phonetic paraphasias (as in Bonhoeffer's case). The latter is a sign of disrupted "inner speech." The patient's speech understanding, however, was fine and he had no trouble repeating single words. Clearly there was no sensory aphasia. Heilbronner argued that the observed telegraphic speech was not voluntary on the part of the patient, a way of economizing in speech by reducing expression to the essentials. The patient practiced the same style in his writing, where there was no time pressure. Also, the patient was not able to build sentences from small sets of printed word tokens and had difficulty repeating even short sentences. This was a real syntactic *inability*. The most likely explanation, according to Heilbronner, was that the patient, like Bonhoeffer's, suffered from a lesion in or near the speech motor area, because there is no sensory aphasia. The observed, lasting agrammatism in the patient must have been a primary effect of a lesion in the speech motor area. Pick's (1898) paper renewed interest in agrammatism. We discuss

[39] "Wie alle namhaften Schüler Wernickes, hat er sich später von den Schroffheiten und der grandiosen Einseitigkeit seines Lehrers freigemacht und dessen Lehre durch Annahme nichtwernickescher Elemente modifiziert," (Liepmann 1914, p. 1941).

Pick's 1913 monograph on agrammatism, which further elaborated his theory. But we must first consider Monakow's (1897, 1905) text, which had already considered some of these issues.

Constantin von Monakow

Monakow was somewhat less of a localizer than Wernicke, Sachs, or Dejerine. Constantin von Monakow (1853–1930), Russian-born but educated in Switzerland, spent most of his professional life in Zurich, from 1894 as a professor of brain anatomy. He paid a study visit to the Dejerines in Paris, whom he admired. His main text on brain and language appeared in the second edition of his *Brain pathology* (1905), a 1300 page monument of clinical neuroscience. Monakow's psycholinguistics is based on Steinthal, with his Herbartian notion of apperception. The text's first edition of 1897 had just preceded Wundt's *Die Sprache.* Monakow's text on aphasia is comprehensive, balanced, and highly readable. It presents in great detail the major clinical symptoms of motor and sensory aphasias, of pure "subcortical" motor and sensory aphasias, and of amnesia. In particular, Monakow describes the relatively preserved syntax of sensory aphasics, even though their speech is often paraphasic (i.e., replacing target words by sound or meaning-related other words). Loss of syntax, agrammatism, is much more characteristic of motor aphasias. On this point Monakow was in agreement with Bonhoeffer, but not with Pick, as we will see. As mentioned, this was becoming a controversial issue at the time.

Monakow also considers the continuing uncertainty concerning the notion of conduction aphasia. What about Wernicke's conjecture that it involves insular lesions? Monakow reversed the perspective and sketched the clinical symptoms resulting from distortions of the insula. His conclusion was that such lesions can cause just about *any* type of aphasia, depending on which subregions of the insula are affected. The text also provides the most complete, state-of-the-art review of written language disorders, agraphias, and alexias.

Monakow's most remarkable contribution, however, was his critical evaluation of strict localism, the view that there exist circumscribed localizable language centers linked by a clear network of fiber connections. Monakow prefers to speak of a "speech region" in the left hemisphere, which is the "area from where speech is particularly easily affected in the sense of aphasia, by continuity disruptions, flow infarcts, pressure etc."[40] This speech region involves the whole perisylvian area. One must assume that "the cortical area involved in the formation of spoken words and word sounds (. . .) extends, on the one hand, far into the frontal lobes and, on the on the hand, far into the basal parts of the temporal lobes, the angular gyrus, etc."[41] Also, speech can return when there are lesions in Broca's area, in the insula, or in the first temporal gyrus, i.e., the so-called negative cases.

[40] "sie ist zunächst nur das Gebiet, von welchem aus durch Kontinuitätsunterbrechung, Zirkulationsabsperrung, Druck, etc. die Sprache im Sinne der Aphasie besonders leicht gestört werden kann." (Monakow 1905, p. 827).

[41] "so kann man sich die Annahme nicht erwehren , daß das an der Bildung der Wortlaute und Wortklänge (. . .) beteiligte Rindengebiet einerseits weit in den Stirnlappen und anderseits weit in die basalen Teile des Temporallappens, des gyrus angularis etc. sich erstreckt." (ibid. p. 918).

The brain is flexible. Other, supplementary areas can take over. Monakow explicitly mentions that F_3 in the right hemisphere is probably *always* involved in the production of articulate speech. Whereas (left) Broca's area constructs "inner speech," the corresponding area in the right hemisphere is involved with the "outer mechanics" of speech formation (p. 917). The functioning of all areas involved depends on the integrity of their connectivity to other neighboring and remote areas. This interdependency of the speech regions accounts for the fact that really "pure" cases of aphasia (motor, sensory, etc.) do not exist; there are always concomitant symptoms in aphasic patients.

Monokow introduced the notion of "diaschisis" into neurology. It is the loss of function, caused by a fresh lesion, in other, more distant parts of the brain, with temporary but complicated symptoms as a result. The notion is related to Hughling Jackson's "dissolution of control," which is also a global "systems" effect following brain injury. A major aspect of diaschisis is the disruption of temporal coordination among brain regions, resulting in a loss of "kinetic melody." Monakow stressed that brain damage can affect the temporal organization of speech, rather than the memory for words or sounds that are supposedly localized in "centers." Patients often show a general slow-down of speech production and an increase in attentional effort during speech. In sensory aphasia, the time window of excitation may have become too short for sentence understanding. The patient can understand individual words, "but forgets, when reaching the end of a sentence period, its beginning"[42] There are global effects of excitability and conduction speed in the affected brain region.

Monakow also expressed anatomical concerns about the definition of "centers." Of major importance is the depth of a cortical lesion. Only deep local lesions can produce "pure" aphasias. If just the surface of a speech region is affected, the clinical picture is less clear. Although Wernicke had happily reproduced Monakow's diagram of the left hemisphere as agreeing with his own views, Monakow is definitely more of a holist, stressing the larger "control" issues in the brain, as Hughlings Jackson had done.

A psychological approach to agrammatism: Arnold Pick

Arnold Pick (1851–1924, Fig. 11.4) had been close to Wernicke. He obtained his MD in Vienna with Meynart, who, as we saw in Chapter 3, was greatly admired by Wernicke. Pick also spent a year (1875) with Wernicke in Berlin, where both worked in Otto Westphal's unit. Pick happily adopted Wernicke's ideal, namely to use aphasia as a window on the secrets of mental life. This was entirely opposite to Marie's "deconstruction" approach. Marie, Pick declared, had adopted "the position of blunt rejection against anything even approaching a pursuit of psychology."[43] Pick, who after Berlin spent his professional life in Prague (ultimately, from 1886, as professor of psychiatry at the German University), was cast from a different mould. He was a man of wide reading with an

[42] "er vergißt aber, am Schlusse einer Satzperiode angelangt, den Anfang." (p. 837).

[43] "Wenn . . . P. Marie . . . den Standpunkt schroffer Ablehnung gegen alles auch nur annähernd an Psychologie Streifende einnahm." (Pick 1913, p. 28).

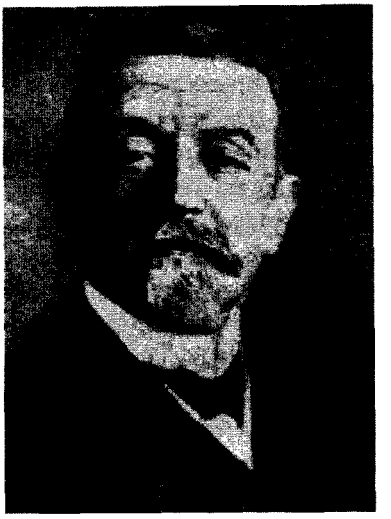


Fig. 11.4 Arnold Pick.

enormous library of his own. He was at home in German, French, and Anglo-Saxon psychology, philosophy, and linguistics. This wide reading is evident in his major contribution to aphasiology, *Agrammatical speech disorders* (Volume I) of 1913. The subtitle sets the aim, to provide a *psychological foundation for aphasiology.* The subtitle of Wernicke's monograph had done the same, announcing *A psychological study.* But how different the books are! Whereas Wernicke went straight to his point, needing only 68 pages, Pick unpacked his whole library before the final formulation of his theory. The introduction alone covers over 100 pages, without any subheadings. It is not a readable book, but the determined reader is rewarded with an innovative theory and many interesting observations.

Pick proposed a new approach to the hotly debated localization issue. It involved, first, the replacement of "stored elements" by "localized functions." Würzburg psychology had made the consideration of mental action as a mechanics of images, sensory, motor, or memory images obsolete. It was outmoded to speak in terms of images stored in nervous centers, such as Broca's and Wernicke's. The introspective evidence was that much happened unconsciously in thought and that conscious content is often imageless, such as being aware of a goal of thought, a "determining tendency," or of a sentence schema. In order to understand agrammatism, one needs a theory of the formulation of thought in speaking and of the corresponding grammatical formulation. These involve functional operations of various sorts. The approach further involves "psychological localizationism." The functional psychology will distinguish a number of processing *stages*, each performing a particular function or operation. Expressing thought in speech involves much more than just mapping an object image onto a word motor image. Knowing the "psychological localization" of the functions entailed providing a new guide to neural localization. Pick noted the tendency, not only in Marie's attack, but also in the work of others, such as Monakow, to localize higher psychological functions in broad zones of the

cortex, without further partitioning: the lure of holism. What was needed, however, was just the opposite: more detailed localization. Recent brain anatomy had revealed a fine-grained map of distinct brain regions. Clearly, the left temporal lobe was involved with thought and speech. But now the search would be on for the subregions that perform particular functions in the complex process of thought formulation and speech understanding.

Pick's book sketched in more detail these stages involved in the formulation of thought in speech. It did not attempt a more refined localization of partial functions in the language areas of the brain. It had, clearly, been Pick's intention to work this out in a second volume of his book, which never appeared.

When discussing the speaker's formulation of thought, Pick adopts the Sterns' definition of a sentence as expressing the speaker's attitude (*Stellungnahme*) with respect to some thought or state of affairs. The route from thought to speech proceeds through four stages. The first two stages concern the formulation of thought, the last two the formulation of language. The first stage is no more than what Messer has called a "state of consciousness." There are no words and the thought, if at all conscious, is global, without any partitioning. It is no more than a "flash" of thought. Exceptionally, this stage can be prolonged. In normal speakers this happens when the ultimate utterance is a one-word sentence, but the word does not immediately appear in consciousness. It is also akin to the "dreamy state" preceding an epileptic fit. In all cases the state has the character of a "feel" that reflects the speaker's attitude to the thought.

During the second stage the thought is further partitioned according to some "thought schema." The schema provides something like Ach's "determining tendency," a more or less conscious objective (*Bewußtheit*) to make the thought explicit. It guides the "logical partitioning" (p. 199) of the thought, for instance into subject and predicate. The elements of the thought and their relations now become conscious. This may involve images, but their relations are usually imageless. In other words, the thought is made "apt for expression" (*Ausdrucksfähig*, p. 229). At this point, Pick adopts from the linguist Gomperz (1908) an entirely modern idea. The still wordless formulation of thought is to some extent co-determined by properties of the target language. The target language has created a mindset (*Einstellung*) in the speaker to formulate thought in such a way that it is easily expressible in that language. The "different languages make different demands on the logical partitioning of thought."[44] The language set establishes a "determining tendency" during the formulation of thought. Pick also cites Franz Boas here (p. 287), who had formulated the same idea in 1911: "In each language only a part of the complete concept that we have in mind is expressed and . . . each language has a peculiar tendency to select this or that aspect of the mental image which is conveyed by the expression of the thought." This "language set" sometimes reveals itself in bilinguals (Pick had many Czech-German bilinguals in his clinic). A Czech patient, for instance, was asked a question in German,

[44] "daß die verschiedenen Sprachen an die logische Gliederung der Gedanken verschiedene Anforderungen stellen" (Pick 1913, p. 199). This is what Slobin (1987) called "thinking for speaking."

then continued answering all Czech questions in German. But the Czech "set" is still active. Asked to name a watch, the patient says "They are . . ." (*Es sind . . .*). In Czech "watch" is a plural noun (*hodinky*) (p. 237). The partitioning of a thought also involves the "musical" elaboration of its "feel." The prosodic character of an utterance emerges from this stage.

The third stage achieves the "grammaticalization of thought." At this stage "sentence schemas" or parts thereof become active in consciousness, but there are no words as yet. A sentence schema does not simply reflect the thought schema from the previous stage. There are smaller or larger mismatches to be handled, depending on the grammar of the target language. Here Pick adopts the Würzburg notion of sentence schema, but like Ach, Messer, and Bühler, the notion and its operation is not worked out in any detail. That had to wait, as we saw in Chapter 7, for Selz (1922), in which an algorithm is proposed for "phase-wise formulation." Pick only reviews the various "means of expression" languages provide, such as word order, inflexion, and prosodic grouping.

During the fourth stage, words are finally inserted. Again, Pick provides very little detail, but he makes two interesting observations. One is that words apparently emerge from what we now call "semantic fields." An amnesic patient often comes up with some word from the target's "environment." The other observation concerns the primacy of content words over form words. Pick acknowledges that form words are often directly expressive of grammatical relations; one would therefore expect them to arise directly following the third, grammaticalization stage. But that is not so, Pick argues, because in many languages these elements take the form of inflections and inflections require the presence of a content word. This may go awry in speech errors, where an inappropriate inflection is attached to a word (p. 245). But even there, the content word is primary (p. 248). Here, Pick missed an opportunity. Apparently his library did not contain Meringer's books on speech errors; they are not cited anywhere.[45] In these books he could have found examples in evidence of the primacy of syntactically positioned form words over content words.

Aphasic speech may arise from damage to any of these four functional stages. Amnesic patients are clearly handicapped in the final stage of processing, the retrieval of words. Their confused choice of words demonstrates that, given some thought, various (related) words may present themselves to consciousness. Pick distinguishes two major forms of agrammatism based on his theory. In the first type, the formulation of thought (stage 2) is not functioning well and as a consequence stage 3 is also handicapped. The aphasic speaker may elaborate a thought without consideration of the interactive speech situation. This is often the case in sensory aphasics (p. 86). In addition, the language set does not function well in the formulation of thought. The upcoming concepts are immediately expressed in words. They follow the "logic" of thought, expressing the natural order of thought, or at least the "skeleton of thought." This agrammatic style was known in Pick's time as *style petit nègre*, also, more considerately, called "telegraph style."

[45] Pick's only source of speech errors is Wells (1906).

Pick equates it with an early stage of child language and with the "natural" signing of deaf people.

In the second type, "real" agrammatism (p. 255), the elaboration of thought is largely intact, but its grammaticalization (stage 3) is handicapped. In this case conventional turns of phrase are activated, but not fully elaborated and combined. Syntax is not absent, but disordered. One would expect Pick to elaborate this type of agrammatism, as opposed to the previous one, but he does not. He fails to provide any aphasic speech data to distinguish the two types. Pick cites liberally from linguistics, but does not practice it in the sense of constructing relevant examples himself. There is, in particular, no effort to discuss telegraphic speech in terms of the new theory. In addition, the stage theory was not used to direct a refined search for the localization of functions. However, it is worth mentioning that Pick, in his very last, posthumous (1931) paper, accepted the view that there are two types of (real) agrammatism: telegraphic agrammatism related to lesions in the frontal region and jargon agrammatism related to damage in the temporal area.

Responses to Pick

Erich Salomon, Bonhoeffer's assistant in Berlin, published an extensive case study on an agrammatic patient. The double paper appeared in 1914, and the author managed to insert comments on Pick's new book. Salomon defended Bonhoeffer's original position that agrammatism results from defects in the motor speech area. It is the picture observed in both Bonhoeffer and Heilbronner's clinical cases. This is real agrammatism, not "pseudo-agrammatism," as Pick dismissed it. Psychologically speaking, the patient has lost the ability to generate the internal sound of longer sentences. This requires processing speed that is unattainable for the patient. The construction falls victim to evanescence. This also makes it impossible for the patient to monitor the motor speech image from the speech sound center. The coordination of both speech centers is hampered. The patient's inability to repeat any longer sentence is caused by the same syntactic inability. When a normal person repeats a sentence, he *reconstructs* it syntactically from the memorized words. That syntactic ability is lost in the agrammatic patient.

Karl Kleist

Wernicke moved to Halle in 1903. There Karl Kleist became his very last assistant till Wernicke's sudden death in 1905. Kleist (1879–1960) became a military doctor during World War I. His neurological observations during that period would form the basis for the grand work he published in 1934 on brain pathology. After his military duties, Kleist moved to the University of Rostock and then, in 1920, to Frankfurt University where he stayed till the end of his career in 1950. In Chapter 14 we will return to Kleist and his vulnerable situation during the Nazi regime.

Kleist's (1934) textbook marks him as the most outspoken localizationist among Wernicke's disciples. Here we are first concerned with the two papers he published in 1914 and 1916, in which he made a distinction between agrammatism and paragrammatism; we will then turn to his 1934 further elaboration of these notions. Agrammatism is the

syndrome described by Bonhoeffer and Heilbronner as the loss of complex syntax such as subordination, telegraphic style, the minimal use of function words such as pronouns and particles, and the loss of inflection and declination. In "paragrammatism," a term coined by Kleist (1914), syntax is not lost, but rather confused. Phrases are ill-chosen and they often contaminate. Syntactic constructions are left incomplete, but there is no simplification of syntax, as in agrammatism. Rather, there is syntactic and lexical overproduction. However, Kleist does not draw the expected conclusion of attributing agrammatism to frontal lesions and paragrammatism to temporal ones. At that time his problem was the lack of "pure" cases. There was always a touch of paragrammatism to agrammatism and vice versa. It could not be ruled out that Pick was right in at least globally localizing the site of both disorders in the temporal region.

In 1934, however, Kleist became more definite. Agrammatism is indeed caused by disorders of the frontal speech region. Observed concomitant paraphasias often resulted from additional affections of the temporal speech zone. More importantly, in lighter forms of frontal agrammatism syntax is not completely lost, but syntactic construction is still vulnerable. On the other hand, specific agrammatical phenomena may emerge in paragrammatisms due to damage in Wernicke's area. The amnesia for words and sounds caused by such damage may create temporal loss of excitability of syntactic constructions. "In short: In frontal sentence aphasia agrammatism and additional slight paragrammatism may arise and in temporal sentence aphasia not only paragrammatism but also amnestic-grammatical defects can become noticeable."[46]

Kleist's 1934 *magnum opus* on brain pathology was based largely on 276 often focal brain lesions he had studied in war victims.[47] That may have reinforced the localizationist perspective that Kleist had adopted as Wernicke's assistant. The book contains the most detailed functional brain map since Franz Jozeph Gall (see Fig. 11.5). Kleist saw his main task as a brain pathologist to relate the evidence from clinical disorders to modern insights on brain anatomy. On brain anatomy he was influenced by the leading work of Brodmann, Vogt, and their followers. Kleist's functional map in Figure 11.5 is based on Brodmann's architectonics. Kleist completed his impressive, detailed chapter on motor aphasias and agrammatisms with a section on the brains of highly talented speakers. Reviewing a dozen reported cases, he claimed the existence of noticeable elevations in their speech motor areas. This was Gall revisited.

Kleist expressed his localizationist alliance with Samuel Henschen (1847–1930), who had produced a gigantic three-volume meta-analysis of reported clinical cases of sensory and motor aphasia (1920, 1922)[48] and in particular with Max Isserlin "who essentially

[46] "Kurz gesagt: bei der frontalen Satzaphasie dürften Agrammatismus und daneben leichtere Paragrammatismen auftreten und bei der temporalen Satzaphasie können nicht nur Paragrammatismen, sondern auch amnestisch-grammatische Mangel bemerkbar werden." (Kleist 1934, pp. 738–739).

[47] Kleist used an additional database of 106 hospital patients, mostly in his study of the basal ganglia.

[48] They appeared as Volumes V, VI, and VII (1920, 1922) of Henschen's eight-volume *Klinische und anatomische Beiträge zur Pathologie des Gehirns*, which appeared from 1890 to 1930. Henschen taught internal medicine in Uppsala and moved in 1900 to the Karolinska Institute in Stockholm.

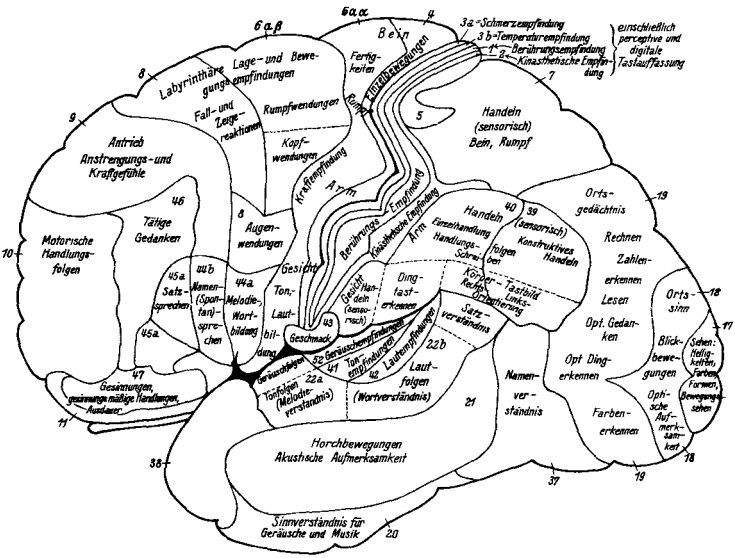


Fig. 11.5 Kleist's functional brain map. Reproduced from Kleist, K., *Gehirnpathologie*, p. 1365, © 1934, Johann Ambrosius Barth.

agrees with me on the partitioning of specific forms of aphasia and who like me rejects the exaggerated holistic teachings of Head and Goldstein."[49] The natural order, therefore, is to first consider Isserlin's theory before turning to the two "exaggerating holists."

Max Isserlin's adaptation theory

We met Max Isserlin (1879–1941) in Chapter 9, as a colleague of Karl Bühler in Munich. Isserlin had studied in Koenigsberg and with Wundt's former student Kraepelin in Heidelberg. He moved with the latter to Munich in 1904. There he became Kraepelin's assistant for experimental psychology. During World War I Isserlin headed a field hospital for soldiers with brain damage. After the war he managed to establish a permanent hospital for war victims with brain injuries, which he later extended with a clinic for child psychiatry. The Nazi regime dismissed him in 1933 and Isserlin ultimately emigrated to England in 1938. That story will be told in Chapter 14. He died in Sheffield in 1941.

Isserlin's main contribution to the study of aphasia was his 1922 paper on agrammatism. It was based on three detailed war victim case studies. They were well chosen to demonstrate Isserlin's new classification of agrammatisms. Building on Kleist's distinction between agrammatism and paragrammatism, Isserlin added a further distinction, based on the patient's language comprehension. All paragrammatic patients, Isserlin

[49] "der . . . mit mir in der Trennung einzelner Aphasieformen grundsätzlich übereinstimmt und wie ich die von Head und Goldstein geübte Überspannung ganzheitlicher Lehren verwirft." (Kleist 1934, p. 1363).

claimed, suffer from grammatical incomprehension. They are, to a large extent, unable to tell grammatical from ungrammatical phrases or sentences. Isserlin's example patient was often unable to detect case errors in German. The patient, for instance, accepted *Ich schneide mich* [should be *mir*] *die Haare ab* ("I cut myself the hair off") as correct, but rejected the correct version. Agrammatic patients, however, came in two categories, or at least ranged between two polarities. There were "pure" agrammatic patients, without any grammatical comprehension difficulties and those who do show some degree of grammatical incomprehension.

What is the psychology of the types of grammatical disorder, in particular paragrammatism versus agrammatism? Isserlin uses Pick's stage theory of production to consider paragrammatism. These patients do not have a primary disorder of thought and their sentence schemas are still available. In other words, their conceptual-linguistic design has not been lost. However, in the process of formulation there is a loss of "sense unity," Bühler's term for the speaker's intended meaning. It is caused by a loss of coherence between meaning details and further grammatical elaboration within sentence schemas, which is in turn due to the loss of sensory control. This was essentially Wernicke's monitoring explanation. The loss of coherence or coordination results in the characteristic contaminations, shifts, and substitutions of paragrammatic speech. In short, paragrammatism is a primary processing disorder, not an adaptation syndrome.[50]

The situation is quite different for both forms of agrammatism. Isserlin rejected Bonhoeffer's and Salomon's view that telegraphic style is a primary disorder. It is, rather, a set effect. Isserlin adopted the Würzburgers' notion of *Einstellung* ("set"). The agrammatic patient suffers from a permanent state of "speech need," the inability to fluently express thoughts into words. Grammatical schemas are difficult to excite, which blocks the grammaticalization of thought schemas. The speech need induces the set in the patient to use telegraphic style. It is a conventional, lawful style, which requires far less effort on the part of the speaker. One of his patients put it this way: "Speaking no time—telegram style."[51] According to Isserlin "telegraphic style is the sentence form of sign language, it is the form of expression of primitives, deaf mutes, children at certain developmental stages, and normal people in particular states of need."[52] The set is to use the "Gestalt" of telegrams. The primary force is the speech need, not the telegraphic set. Patients do not have to use it and sometimes do not, Isserlin argues. They can abolish the style, when they have to or when they are in a more formal speech situation with doctors or teachers. In those situations their (slow) speech will seemingly show paragrammatic syntactic errors. They often resort to analogical formations if the correct form cannot be excited, for instance using weak forms for strong ones (such as saying *gooses* for *geese*). The same may occur in

[50] "zentrale Störung des Gestaltens." (Isserlin 1922, p. 408).

[51] "Sprechen keine Zeit—Telegrammstil." (ibid. p. 397).

[52] "Der Depeschenstil ist die Satzform der Gebärdensprache, er ist die Ausdrucksform der Primitiven, der Taubstummen, der Kinder auf bestimmten Entwicklungsstufen, und der Normalen in bestimmten Situationen der Not" (ibid. p. 395).

writing without time pressure. Isserlin's set theory of telegraphic style was the precursor of Kolk and Heeschen's (1990) "adaptation theory." The occurrence of defects in grammatical comprehension in some agrammatics is due to a "loss of grammatical ordering,"[53] according to Isserlin, which is hardly a convincing explanation.

Isserlin did not hesitate to view agrammatism as resulting from motor aphasia and paragrammatism as being linked to sensory aphasia. This is in accordance with both Pick's and Kleist's ultimate positions.

Isserlin's last major publication on aphasia was his 1936 contribution to Bumke's *Handbuch der Neurologie.* The chapter was also separately, but informally[54] published as a monograph. It provides a lucid review of the state of the art in aphasiology. It discusses in particular the ongoing debate on classical localizationism versus holism. Isserlin concludes: "However, a monistic holism is, also in brain pathology, unjustified. And it cannot be concluded that theoretical-holistic 'iconoclasts' (Head) have succeeded in overthrowing localizationist doctrine. Rather, the principles of classical localizationist doctrine appear untouched in their essential features."[55] The chapter also contains the final formulation of his adaptation theory. "It should be noticed that correct, pure telegram style is not a faulty or erroneous language. It is in human evolution a frequent and rule-based form of discourse.... The telegram style is a *set phenomenon which arises from the speech need of the aphasic.* ... The notion of telegraphic agrammatism as a need phenomenon is supported by the fact, that the same patient can choose other forms of utterance in situations of less speech need – in writing – and produce relatively correct grammatical expressions."[56]

As mentioned, Isserlin had been Kraepelin's student. In his paper, Isserlin makes reference to an unusual but highly interesting monograph Kraepelin had published in 1906, *On speech disorders in dreams.* Emil Kraepelin (1856–1926) systematically collected stretches of speech he remembered from his dreams. Together with a few cases reported to him, the database contained 286 examples. In the monograph Kraepelin divided them into three categories: word-finding disorders (often new formations, paraphasias), disorders of discourse (*Rede*), and disorders of thought. He then compared these to the disorders of speech and language observed in the clinic, such as in the speech of alcoholics,

[53] "Verlust der grammatischen Reihenbildung" (Isserlin 1922, p. 408).

[54] "Nicht im Handel" on cover page.

[55] "Ein Ganzheitsmonismus ist aber auch in der Hirnpathologie unberechtigt. Und es kann nicht geurteilt werden, daß es dem ganzheitstheoretischen 'Bilderstürmer' (Head) gelungen sei, die Lokalisationslehre umzustürzen. Die Prinzipien der klassischen Lokalisationslehre erscheinen vielmehr in ihren wirklichen Zügen unberührt." (Isserlin 1936, p. 641).

[56] "so muß beachtet werden , daß der korrekte reine Depeschenstil keine falsche oder fehlerhafte Sprache ist. Es ist eine in der Entwicklung der Menschheit ausgebreitet und gesetzmäßig vorkommende Form der Rede ... der Depeschenstil ist ein *aus Sprachnot des Aphasischen entsprungenes Einstellungsphänomen.* ... Die Auffassung des Telegrammagrammatismus als einer Noterscheinung wird gestützt durch die Tatsache, daß der gleiche Kranke in einer Situation geringerer Not – beim schriftlichem Ausdruck – andere Darstellungsformen wählen und relativ korrekte grammatische Äußerungen liefern kann." (ibid. p. 749).

schizophrenics, and agrammatic patients. It is the latter comparison that Isserlin referred to. He cited the example *Die Tiere ersticken früher in werden dem Unmut* (something like "The animals suffocate earlier in becoming to displeasure") and suggested that such dream speech is often paragrammatic. Kraepelin had himself indicated this relation to sensory aphasia, noticing similar slight displacements between thought and its grammatical expression. Kraepelin kept recording his dream speech, scribbling it down on a note pad at his bedsite. Up untill 1926 he collected another 369 items. They have been published in modern editions.[57] Here is one more "paragrammatic" example from the latter corpus: *Ich hab' mich bei Meyers stets bei Tagewerk* ("I have myself at Meyers always at day's work"—Kraepelin's note: "I have always done my duty as a worker at the Meyers firm."[58]). It is not far-fetched to follow Isserlin and attribute such paragrammatisms to a lack of control or self-monitoring. Still, Emil Fröschels (1955) did not agree, as we will discuss in Chapter 14. Let us now turn to the "exaggerating holists."

Henry Head: a holist's view on theory in aphasiology

One of Henry Head's earliest papers (1915) is on Hughlings Jackson's aphasiology. The paper starts like this: "It is generally conceded that the views on aphasia and analogous disturbances of speech found in the text-books of to-day are of little help in understanding an actual case of disease. . . . Each patient with a speech defect of cerebral origin is stretched on the procrustian bed of some theoretical scheme: something is lopped away at one part, something added at another, until the phenomena are said to correspond to some diagrammatic conception, which never has and never could have existed." This was the beginning of a two-decade long witch hunt. The "diagram makers" formed his target with their "confused classifications." "The air was thick with schematic representations of centres of all kinds" (p. 23). Diagrams are despicable, and so are tabulations: "Tables are almost as fallacious as diagrams." (p. 139). The diagram makers "failed to contribute anything of permanent value to the solution of the problem of aphasia, because they were dominated by a philosophical fallacy of their day, which can still count its victims amongst writers on the subject" (p. 65), etc, etc. Chapter 3 already cited and criticized some of Head's diatribes against Bastian and Broadbent in his own country. But the Germans, in particular Wernicke, received a similar treatment. "Germany had just emerged victorious from her war with France and the awakened national consciousness influenced even so remote a subject as cerebral physiology. In 1874, Wernicke published his pamphlet entitled *Der aphasische Symptomencomplex* based entirely on the work of his

[57] Heynick (1993) contains an English translation of Kraepelin's 1906 monograph and presents the second corpus of 369 items with English transliteration, plus English translations of Kraepelin's contextual notes to the items. Engels (2006) contains a CD of the second corpus (items and notes) in their German original. I found one more, a linguistic study of dream speech, by Louise Pound (1934) of the University of Nebraska. She makes neither reference to Kraepelin, nor to the aphasia literature. Sadly, she then concludes her interesting paper with the remark that "such material . . . may be found to have . . . a scientific value that is not now apparent." (p. 180).

[58] Ich habe als Arbeiter der Meyerschen Betriebe stets meine Pflicht getan. Engels (2006), CD.

Fig. 11.6. Henry Head.
© Wellcome Library, London.

fellow-countrymen. The names of Ogle and Jackson are cited, but in such a way that it is evident he had not read their papers; Bastian he consistently ignores." (1926, p. 61). Did Head himself read Wernicke (1903), a paper he cited? There Wernicke writes: "Even more I recognize the obligation to state expressly, that the major symptom of sensory aphasia, loss of speech comprehension with preserved hearing, had already before [i.e. before 1874] correctly been diagnosed by two authors, . . . Schmidt . . . , Bastian."[59]

Henry Head (1861–1940, Fig. 11.6) had, among others, been a student of Ewald Hering at the German University in Prague, leaving the year Arnold Pick arrived there in1886. He then moved to University College London, where he made his further career. His main scientific contributions were to sensory physiology. Aphasiology was somewhat of a sidetrack, but Head was as ambitious here as he had been on any of his many other scientific projects. The reason for his interest had been his treatment of brain injured patients during World War I. During that period he familiarized himself with the classical literature on the topic and became inspired by his reading of Hughlings Jackson's work, which as we saw was quite deviant from the dominant perspective. Head had known Jackson intimately, but they hardly ever discussed aphasiology because Jackson died in 1911, before Head turned to aphasia. Head republished Jackson's main papers on aphasia in *Brain* (1915). He also dedicated his *Aphasia and kindred disorders* (1926) to Jackson, Hering, and Gaskell (of Trinity Hall).

[59] "Umsomehr erkenne ich die Verpflichtung an, ausdrücklich zu erwähnen, dass das Hauptsymptom der sensorische Aphasie, Aufhebung des Sprachverständnisses bei erhaltenem Gehör, schon vorher von zwei Autoren richtig gedeutet worden ist, und zwar auf Grund eigener Beobachtung von *Schmidt*, bei Erwähnung fremder Fälle von *Bastian*." (Wernicke 1903, p. 493). Wernicke's given citations are Bastian (1869) and Schmidt (1871).

Head's generalized contempt for the diagram makers really concerned a few basic notions that were characteristic of aphasiological psychology half a century earlier, or thought to be so. They concerned in particular the view that language consists of words and that "centers" with "images" form the neuropsychological infrastructure of language use. We will discuss these in order and then turn to Head's views on adaptation, types of aphasia, localization, and general methodology.

Words as units of speech

Head chided the diagram makers' analysis of speech in terms of words. "Speech is not simply the utterance of words, but consists of the formulation and expression of propositions." (Head 1926, p. 400). This had been Jackson's position, which was indeed advanced in nineteenth-century aphasiology. But by 1926 this view, or its variants, had become generally accepted in psychology (for instance by Wundt, the Würzburgers, and the Sterns), not to speak of linguistics. Head referred to Gardiner's (1922) paper on sentence and word, agreeing with him that a "phrase is not solely the conjunction of distinct words; it is thought expressed with a view to appreciation by an auditor" (p. 501). It is his only reference in the book to any linguist; linguistics was not his cup of tea. Head considered aphasia as a loss of "the power of symbolic formulation and expression" (p. 419 and *passim*). "By symbolic formulation and expression I understand a mode of behavior, in which some verbal or other symbol plays a part between the initiation and execution of the act." (p. 211). With Hughlings Jackson, he stressed the role of internal speech in thinking, without conflating the two, but there was no reference to the recent British dispute with Watson on the issue.

Centers and their lesions

Head rejected the static notion of "centers" containing images, sensory, motor, conceptual, their psychological interactions being association. He applauded Marie, the "iconoclast," for his head-on attack of the classical doctrine but otherwise entirely disagreed with him. Head argued for a more dynamic, functional view. "An act of speech is a march of events, where one changing condition passes insensibly into another." (p. 474). The psychology should be one of functions performed, not of images activated. This was hardly news in 1926. Monakow, as we saw, had already argued in 1905 that brain damage can affect the temporal organization of speech, rather than the memory for words or sounds that are supposedly localized in "centers." The Würzburgers had argued for imageless functioning of thought. Pick (1913), as we saw, took the first steps in designing a functional psychology which distinguishes a number of processing *stages*, each performing a particular function or operation. Piéron (1920), not cited by Head, argued that "centers" are not stores of images, but of coordinated mechanisms. They perform sensory or motor functions.

Head's further irritation was that these centers and their connecting fibers were considered to be strictly localized in the brain, or as he formulated it "It was assumed that for every mental element there was a neural element." (p. 139). This "dead-house anatomy" was untenable. However, even 50 years earlier, this generalization was rarely claimed

by practitioners. It was certainly dismissed by Wernicke, who rejected any attempt (such as Bastian's) to localize a "conceptual center." Head never understood that the detested diagrams were, first and foremost, *psychological models.* Localizing the nodes and arcs in the brain was quite another issue. Bastian, for instance, whom Head considered to be an extreme "localist," asked himself "Have we, in fact, to do with *topographically separate areas of brain-tissue* or merely with distinct cell and fiber mechanisms existing in a more or less diffuse and mutually interblended manner?" (Bastian 1880, p. 522).[60]

When Head speaks of "centers" it is always in the sense of anatomically localized structures that perform specific mental functions. This whole notion is ill-conceived, according to him: "no function is 'localized' strictly in any part of the cortex" (p. 499). But then, centers without quotes appear regularly in his text. He speaks, for instance, of the "centres involved in these automatic processes, which form an essential part of the conscious act" (p. 487) and of "lesions of the higher anatomical centers" (p. 496). This ambivalence is ubiquitous. We had therefore best consider Head's positive statements of the neuropsychological events going on in the brain, which unfortunately are hardly more precise. Here are a few characteristic quotes:

From the psychological perspective: "An act of speech comes into being and dies away again as an alteration in the balance of psycho-physical processes: a state, never strictly definable, merges into another inseparable from it in time." (p. 509). "When speech is defective, this easy motion or transition is impeded; one state cannot flow into another because of some mechanical imperfection in the process." (p. 474).

From the neurological perspective: "The processes which underlie an act of speech run through the nervous system like a prairie fire from bush to bush; remove all inflammable material at any one point and the fire stops."(p. 474). "There are no 'centers' for the use of language in any form, but solely certain places where an organic lesion of the brain can disturb speech in some specific manner." (p. 140). Or: "Most so-called 'centres' in the nervous system are strictly speaking nodal foci, where central neural activities undergo integration and other changes in relation to one another." (p. 474). Specific lesions can disrupt the function of speech in various ways, but "There is no point to point correspondence between physiological processes and the constituent elements of an act of speech." (p. 512).

At this point, Head closes the door on a research paradigm, which in modern aphasiology has turned out to be highly productive: "Every disorder of speech is manifested in psychic terms; but in no instance can the nature and extent of the disturbance be deduced from a priori consideration of the normal use of language." (p. 511). Pick's program, and others, are doomed to fail, according to Head (p. 530).

Adaptation

Aphasias are disorders of function, according to Head. "These morbid states express the manner in which the organism meets a novel situation with all the means at its disposal.

[60] See John Marshall's discussion in Eling (Ed.) (1994, pp. 105–116).

Each abnormal act of speech is the response of an individual to some interference with the mechanism necessary for perfect use of language, and is governed by the conditions of the moment." (p. 424). This is, certainly, a pithy holistic statement that is not negotiable. It nevertheless invites a research program along two lines. First, what is interfered with in the mechanism? Second, how does the patient meet the novel situation, adapt to it? Head disappoints on both scores. Nowhere is there an attempt to specify the underlying mechanism in any detail. The existing psychology and linguistics on this point, Paul, Wundt, Sechehaye, Bühler, Selz, and Piéron, is entirely ignored and any move in that direction is blocked, because there are no stages in the mechanism according to Head; every process flows over any other. Also, there is no formulation of a systematic approach to how patients adapt to their new situation. I found only two examples of patients' adaptation strategies. Aphasics tend to "follow a sequence" in order to arrive at a name or answer (p. 510), such as running over the days of the week, months of the year, letters of the alphabet. The other strategy (p. 539) is patients using their fingers to count or do arithmetic. Head was, apparently, in no way inspired by Isserlin's approach to telegraphic speech.

Aphasic syndromes

Head made a major point of arguing against "pure" aphasic syndromes. Every aphasic is complexly affected and all patients are different. Attempts to tabulate behavioral patterns are doomed to fail. But then, sprinkled with qualifications, Head does distinguish four classes, "possessing certain common characteristics" (p. 428). They are (1) "verbal aphasia," a difficulty of forming words for external and internal use but with a preservation of word comprehension, (2) "syntactic aphasia," "essentially a disorder of balance and rhythm; syntax suffers greatly" but understanding is preserved, (3) "nominal aphasia," a "loss of power to use names and want of comprehension of the significant value of words and other symbols," and (4) "semantic aphasia," "a want of recognition of the full significance of words and phrases apart from their direct verbal meaning" (pp. 428–429). Head's tabulation did not catch on in aphasiology.

Localization

Again with many qualifications, Head mentioned loci of injury that are typically associated with these four classes of aphasia. Verbal aphasia associates with lesions of "the foot of the precentral and neighboring gyri and the parts beneath them." Syntactic aphasia is associated with "lesions in and around the upper temporal gyri and the parts beneath them." Nominal aphasia relates to "lesions of the angular gyrus" and semantic aphasia is probably associated with "lesions in the vicinity of the supra-marginal gyrus" (pp. 500–502).

Methodology

Head's concessions on classification and localization did not diminish his methodological convictions. The aphasiologist's task is not to erect preconceived categories and to

tabulate patients' behavior accordingly. "Since no two examples can ever be identical, we choose out certain features as significant, collating them with our previous experience of similar or diverse signs in other cases, and we conclude that the patient is suffering from a certain disease. But this entity, which we have erected by conceptual abstraction . . . has no existence apart from our own intellectual activities. . . . Directly we behave as if they possessed any validity other than descriptive, we are liable to fall into the fallacious mode of reasoning which has been so prevalent in the study of aphasia and kindred disorders of speech." (p. 507). "Moreover, it is well to remember that if any two hypotheses equally explain a set of observed facts, there is no inherent reason why the simpler one should lie nearer to the truth." (p. 511). There is, in short, hardly any place for theory in Head's conception.

Head applauded the holistic approach in Goldstein's early work on aphasia and "mind-blindness." Goldstein occasionally acknowledged a similarity in perspective with Head, his rejection of the traditional doctrine and his holistic view that aphasic disorders are never "pure."

Kurt Goldstein and the single case study

The appearance of Head's book almost coincided with a major lecture Goldstein presented in Bern to the Swiss Society of Psychiatry in 1926. It was published in 1927. It is a good starting point for discussing Goldstein's major contributions to aphasiology. Goldstein, halfway through his career, presented the method and theory he had chosen to develop since his training with Carl Wernicke, which was a holistic paradigm he would stick to in all of his later work. As far as aphasiology is concerned, that work culminated in his 1948 book *Language and language disturbances.*

Kurt Goldstein (1878–1965, Fig. 11.7) received his MD with Carl Wernicke in Breslau in 1903. There he must also have become acquainted with the Sterns, to whose work he occasionally refers in his later publications. After Breslau he moved to Frankfurt,

Fig. 11.7 Kurt Goldstein. Original Source: *In Context* (Fall, 1999, p. 14). © 1999 by The Nature Institute.

Koenigsberg, and (in 1914) back to Frankfurt. As so many of his German colleagues had, he became involved with brain-damaged war victims, their diagnosis and their treatment. Like Isserlin in Munich, Goldstein established in Frankfurt a clinic for brain-injured war veterans, which he directed till 1930. These were the years of his productive collaboration with psychologist Adhémar Gelb (1887–1936). In 1930 Goldstein moved to Berlin as a professor of neurology. Being a Jew, he was dismissed in 1933 and moved to Holland, where he wrote and published his book *Der Aufbau des Organismus* (in 1995 republished as *The organism: A holistic approach to biology derived from pathological data in man*), supported by Dutch and American funds. He emigrated to the United States in 1935. His friend Gelb suffered a worse fate. He was dismissed from Halle University in 1933, also left for Holland, and then moved to Sweden in 1935. There he died of tuberculosis the next year, in miserable circumstances. Goldstein taught at Columbia and Tufts University. In 1938–1939 he delivered the William James Lectures at Harvard; they were published in 1947. Goldstein's work spanned the era from Wernicke till the "cognitive revolution."

In his 1927 paper, Goldstein developed his new, holistic perspective from a case of alexia or "mind blindness" he and Gelb had jointly studied over several years. The patient was, on first view, normal in most respects, except that he could not read words. Like Dejerine's pure alexia case, he was able to write. Goldstein used this so-called "pure" case to demonstrate that underlying it was a much broader, functional disorder. In the optical domain, the patient could not "grasp" figures. To find the center of a circle, the patient moved his head spiral-wise, and stopped in the midpoint. He could draw figures, but not straightforwardly copy them. But more was disordered. He could not subitize (i.e., estimate small simultaneous quantities), he had great difficulty recognizing objects from touch, he could not recognize musical intervals, etc. The functional disorder, Gelb and Goldstein (1920) conjectured, was one in Gestalt formation. The sensory elements were all there, but they could not be simultaneously grasped as a whole. The total impression remained amorphous, without "pregnancy." This case nicely demonstrated that the most conspicuous symptom of a patient, in this case alexia, need not be the patient's essential disorder.

For Goldstein only deep case studies were satisfactory. The aim was to discover the patient's underlying functional disorder. This perspective relativized the traditional approach, where the most striking symptom defined the aphasic type of the patient. A patient cannot write, so she is agraphic. A patient cannot name objects, so the patient is amnesic, etc. Indeed, amnesia has been another, recurrent demonstration theme in Goldstein's work, since his very first 1905 paper; we will return to it. The 1927 paper touches on all core notions in Goldstein's work. They concern holism and the organismic approach, the general effects of brain lesions, functions and types of language use, aphasic phenomenology, issues of localization, and contributions from neighboring disciplines. Goldstein's notions of therapy, however, we will not touch upon. Let us review these issues, without restricting ourselves to the 1927 paper.

Holism and the organismic approach

The traditional approach in aphasiology had been atomistic and based on a reflex theory of brain and psychological functioning. But even Wernicke, one of the godfathers of this

theory, recognized that more was at stake in explaining the phenomena. Wernicke, we saw, was a holist with respect to the superordinate "conceptual center."

Goldstein acknowledged that Jackson, Monakow, Marie, Pick, and Head had undermined the atomistic approach. He himself, still in Breslau, also began changing his mind under the influence of Ernst Storch, Wernicke's loyal assistant, and his *Habilschrift* of 1901. A more holistic approach was needed. By 1927 Goldstein's perspective had turned 180 degrees, "the single can only be understood at all from the whole."[61] What is the whole? It is the organism, the whole person. "The basic motive of organismic life is the trend of the organism to actualize itself, its nature, its capacities, as well as possible." (1948, p. 19). "From this point of view, it follows that every *individual speech-performance is understandable only from the aspect of its relation to the function of the total organism in its endeavor to realize itself as much as possible in the given situation.*" (p. 21).

Methodologically, this requires detailed case studies. "More important than everything else is first to describe the phenomena, as much as possible without prejudice."[62] This is not easy, but there are some rules of thumb. First, always observe the phenomena in relation to the behaving person as a psychophysical whole. Second, attend to any phenomena, without prejudice. Third, be clear about what the phenomenon is. Try to find out how the phenomenon came about in the patient's behavior. In many cases, patients respond "by taking a detour." The observed phenomenon, then, is a patient's adaptation to dysfunction. Any effort at localization should take this into account (it makes no sense, for instance, to try and localize telegraphic speech in Isserlin's sense). Try and find out which symptoms are discernable. Generally speaking, symptoms can be of four types. They can be *direct* symptoms, immediate effects of the disturbance. They can be *indirect* symptoms, secondary effects caused by responses of other, undamaged parts of the system. Monakow's diaschisis is a case in point. They can be symptoms of evading *catastrophe*. This is adaptation by avoiding risky situations in which the disturbance can seriously upset the organism. The therapeutic issue here is what type of milieu would minimize such risks for the patient. Finally, there are symptoms of fatigue and perseveration in situations of stress, often caused by a threat of catastrophe.

Unprejudiced observation is the principle, but in his later work Goldstein admits: "Even a simple examination presupposes some sort of theoretical concept and still more so does retraining." (1948, p. xi).

This whole-person perspective is, of course, highly respectable, but it easily becomes a *deus ex machina*. In the 1927 paper, for instance, Goldstein explains a severe aphasic's remaining ability to sing a song as a "total reaction" of the patient who is "strongly involved with his whole personality."[63]

[61] "dass das Einzelne überhaupt nur vom Ganzen aus verstanden werden kann." (Goldstein 1927, p. 68).

[62] "Wichtiger als alles andere ist zunächst einmal die möglichst unvoreingenommene Beschreibung der Phänomene." (ibid. p. 10).

[63] "weil es sich bei einem solchen ausdrucksmässigen Hervorbringen der Sprache, bei dem der Kranke so stark mit seiner ganzen Persönlichkeit beteiligt ist, um eine Gesamtreaktion handelt, die eine rel. einfache Leistung darstellt." (ibid. p. 47).

General effects of brain damage

Sensory input not only activates a specific region of the brain, but also the brain as a whole. This has the character of figure-ground organization. Figure and ground are two sides of the same coin, neither can exist without the other. Brain damage always affects this organization. More generally, any brain damage will cause "dedifferentiation" of function (1948, pp. 4, 50). This can have several consequences. The patient may no longer be able to discriminate elements in the whole. The impression is more stimulus-bound and hence less "constant" and less abstract. The patient becomes slower and less able to switch from one perspective (figure-ground) to another; this is what we would now call diminished selective attention.

The loss of "abstract attitude" is a returning theme in Goldstein's work. His account of amnesia is entirely based on it. Amnesia was traditionally seen as a loss of the ability to name things. Word images or concept-word connections are weakened or gone. However, intensive work on single patients (together with Gelb) showed a quite different underlying defect, a loss of abstract attitude. Normal naming involves *categorizing*. To denote a table by *table* is recognizing its kind. That capability is affected in amnesics. They cannot sort objects in terms of functional similarity, shape, or color. They sort on the basis of belonging together in concrete situations. There is a general inability to access superordinate concepts (1948, p. 266). Amnesia is not a disorder of language, according to Goldstein, but of abstract thought.

Instrumentalities and abstract language

As far as amnesia is concerned, Goldstein could agree with Head; it is a symbolic disorder. But Goldstein rejected Head's claim that all aphasic disorders are disorders of meaning. Many, if not most, aphasic symptoms are disturbances of the "instrumentalities" of speech (1948, p. 25). They are speech automatisms, which have to be up and running in normal language use. They are the mechanisms of word formation and articulation, of speech sound perception and word recognition, of activating phrases and their prosody. Goldstein calls this all "concrete language." Disorders of these mechanisms lead to anarthria, pure motor aphasia, word blindness, paraphasia etc. These are not defects of "symbolic formulation and expression," but of the *means* of expression. They must be distinguished from disturbances in the *use* of these instrumentalities.[64] These are disturbances of meaning, like amnesia. Goldstein speaks of "abstract" language use, which requires the previously mentioned "abstract attitude." It is volitional speech, with its dominant representative function, Jackson's "intellectual speech" (1927, p. 43). Abstract language use can be preserved, even if instrumentalities are affected. A patient recovering from motor aphasia may resort to telegraphic style, just to circumvent hampered instrumentalities. Inversely, Gelb and Goldstein had also seen cases where abstract, representative language was gone, but without the loss of instrumentalities. Speech flows without thought, without relation to context.[65]

[64] See Goldstein's (1933) paper and Gelb's introduction to it, delivered at Delacroix's symposium.

[65] Goldstein in Delacroix (1933, p. 494).

Inner speech

Wernicke had proposed the notion "word concept" (*Wortbegriff*) to indicate the pair of a word's sound and motor images. In the French tradition this was called inner speech. Some notion of this kind was needed to explain the troubles of written language use in motor and sensory aphasia. Reading requires intact word images, because in the traditional view reading required letter-by-letter composition of the word's sound image, which formed access to the thing-image or concept (this was Grashey's theory, discussed in Chapter 3). Tests for the availability of such inner speech had become standard in clinical practice (for example asking the patient to tap the number of a word's syllables). At the beginning of the century these notions were in flux among Wernicke and his students, Storch, Kleist, and Goldstein. Ultimately Goldstein came up with a wider circumscription of "inner speech": "*Inner speech is the totality of processes and experiences which occur when we are going to express our thoughts, etc., in external speech and when we perceive heard sounds as language.*" (1948, p. 94). It is, in production, what the instrumentalities do on the basis of the thought input. And it is, in addition, what drives external, overt speech. This notion is clearly related to stages 3 and 4 of Pick's production schema. Inner speech is the outcome of grammaticalization and the insertion of words.

Goldstein relates this notion to the Piaget–Vygotsky debate on small children's so-called "egocentric speech." Here he accepts Vygotsky's position. The children's monological speech is the precursor of inner speech. It is, moreover, entirely social, i.e., non-egocentric.

Language functions

Goldstein entirely missed the literature on language functions. Language, he writes in (1927, p. 43), is first used for representation (*Darstellung*). Second, it is used as expressive means (*Ausdrucksmittel*), as in Jackson's "emotional speech." This sounds somewhat like Bühler. But then, there is no mention of an "appeal" function. Rather, Goldstein adds an ill-defined third category, "colloquial speech," which emerges automatically from the total situation. Goldstein must have met Karl Bühler in Delacroix's 1933 Paris symposium on the psychology of language. What Goldstein picked up from his paper was the basics of structuralistic phonology.[66] *Sprachtheorie* appeared just after Goldstein had left Germany; he probably never saw it.

Forms of language disturbance

Goldstein continued to revise his typology of aphasias. He worked from two perspectives. One perspective we have just discussed. Defective language can result either from the disturbance of instrumentalities or from disturbance of the *use* of instrumentalities. Disturbed instrumentalities can affect internal speech and external speech; this is aphasia proper. Disturbed *use* of instrumentalities involves distortions of meaning. Goldstein never gave in to the holist lure of considering all of aphasia as disorders of intelligence or meaning, as did Marie and Head.

[66] Referred to in Goldstein (1948, pp. 33, 350).

The other perspective is a further elaboration of the traditional classification of aphasic phenomena. In his ultimate 1948 text, Goldstein lists nine aphasic syndromes, ranging from "impairments of abstract attitude" and "emotional language" to "disturbances of reading and writing" (see note[67] for the full listing). Goldstein's categories are not mutually exclusive and most categories are not at all uniform in their etiologies; they are phenomenological in the first place. We will not follow Goldstein in his discussion of still other symptoms, such as disturbances of calculation, of musical cognition, of gesturing, but turn to a final major topic in Goldstein's impressive bibliography, localization.

Localization

Goldstein's view on localization is best expressed in his own words: "to each performance corresponds *an excitation of definite structure in the cortex,* indeed, not in a circumscribed area but *widespread over the whole cortex,* differently in each performance. *This is what we should term localization.*" Still, Goldstein was not an equipotentialist: "Now it will further become evident that we are able to localize definite symptom complexes in definite areas, but with that reservation which consideration of the mentioned anatomic and symptomatologic complexity demands." In other words, there are dedicated areas in the brain subserving specific performances and their disruption gives rise to definite symptom complexes. But there is also always the "figure-ground" relation between the region's excitation (the "figure") and the brain's response as a whole (the "ground"). More practically, this "leaves unaltered the usual procedure of localization of symptoms or symptom complexes for practical purposes."[68]

Although Goldstein had fully dropped his teacher's reflex arc view of the brain, he nevertheless stayed close to Wernicke's main schema of localization. The pure sensory and motor aphasias emerge, for Goldstein too, from lesions in the temporal and frontal areas, as described in the classical literature. Localization of the more central, or "transcortical," aphasic syndromes is hardly possible for Goldstein, as it was for Wernicke. Here extensive regions of the cortex must be involved. Goldstein's important, detailed case studies largely concerned the latter, transcortical syndromes, whereas Wernicke built his career on the primary sensory and motor disorders. In other words, Goldstein's holism and Wernicke's localizationism only appear contrastive. They focused on different symptom complexes, Wernicke on ones that are well-localizable, Goldstein on others that are not.

Epilogue

Goldstein began his career in the twilight of classical aphasiology. He had absorbed Wernicke's analytical approach but faced attacks on the paradigm as a whole. He had to

[67] Goldstein's (1948) nine categories of language disturbance: disturbances of language by impairments of abstract attitude, emotional language, disturbances in finding words, disturbances of repetition of heard speech, disturbances of the expressive side of language, disturbances of the receptive side of language, disturbances of inner speech, disturbances of intelligence, and disturbances of reading and writing.

[68] All citations from Goldstein (1948, p. 50).

find his own way and he did. In an eclectic way he picked up some conspicuous notions of modern psychology. Thought and mental processes in general can be imageless, even unconscious. Mental experiences come as "Gestalts." The whole, the "figure" has precedence over the details. Importantly, there are no figures without backgrounds. These became the ingredients of Goldstein's psychological holism. Probably his core contribution to aphasiology, and to neuropsychology generally, has been the in-depth case study as a methodology. It is not a patient's eye-catching symptom that is to be described and localized, but the underlying disorder that has to be discovered. The disorder can account for the full variety of the patient's symptoms. This methodology Goldstein worked out in detail, and it has survived till the present day. Goldstein always remained analytic, not giving in to the easy, trendy holism that was in the air.

Luria, in his 1966 *in memoriam* for Goldstein acknowledged the "tragic side" of Goldstein's long life, the cut connection to his native country and to his natural audience. He had been most welcome in the United States, but was little understood or followed by the younger generation.

Roman Jakobson

Goldstein's contacts with linguistics had been quite limited, but he was aware of Jakobson's 1941 monograph, which we discussed in Chapter 10, and largely agreed with his treatment of phonemic deterioration in aphasic speech. Jakobson complained that in aphasiology "the description and analysis of concrete linguistic and in particular speech sound data show, with few exceptions, an astonishing imperceptiveness and bafflement."[69] Even Arnold Pick, who was so receptive to linguistics, wrote that he was "naturally unable" to express what was spoken in writing. That would have required "more precise phonetic analysis."[70] What can be observed in aphasic speech, according to Jakobson, is that speech sounds loose their distinctive or phonological character. This was, for instance, the case in Liepmann's Czech patient who had lost the distinction between *l* and *r*, saying *hlad* instead of *hrad* ("bridge"). Hence, the question is "How is the patient's phonological system changed?" These changes are regular, Jakobson suggests. They follow the same hierarchy as proposed for the development of children's speech, but in inverse order. The same point, we saw in Chapter 10, had been made earlier by the Viennese speech pathologist Emil Fröschels (1918), to whose work Jakobson makes due reference. There is, however, no anticipation in that work of Jakobson's theory of phonological development.

The phonological perspective on the aphasic phenomena of word deafness and word muteness is a functional one. If the system of phonological distinctions deteriorates in receptive aphasia, the incidence of homonyms increases accordingly. When a German aphasic (like Liepmann's Czech patient) has lost the liquid opposition of *l* and *r*, the

[69] "aber bei der Beschreibung und Analyse konkreter sprachlicher und insbesondere lautlicher Tatsachen zeigen sie bis auf wenige Ausnahmen eine erstaunliche Unaufmerksamkeit und Ratlosigkeit." (Jakobson 1941, p. 35).

[70] (ibid. p. 36).

patient is functionally unable to tell *Rippe* ("rib") from *Lippe* ("lip"). Capturing a word's specific sense becomes impossible when words are multiply homonymous. This is exactly the situation that emerges as the system of phonological distinctions falls apart. Similarly, articulation becomes increasingly paraphasic as phonemic distinctions disappear. Jakobson blamed both Marie and Niessl von Mayendorf for reducing speech phenomena in motor aphasia to dysarthria, while the real issue remains the inability to make the relevant phonemic distinctions in the motor expression of speech. In other words, it is not articulatory difficulty that is at issue, but functional linguistic distinctiveness. Goldstein had been aware of Jakobson's work, but it was Luria who would really incorporate Jakobson's analysis in his own theory, as we will see shortly.

In 1956 Jakobson, together with Morris Halle, returned to issues of aphasia in their *Fundamentals of language.* This small book is in two parts, the first formulating their joint feature theory of phonology, the second devoted to aphasia. Part two repeats Jakobson's (1941) complaint about aphasiologists ignoring linguistics (and linguists ignoring phenomena of aphasia). After a short summary of the 1941 theory ("Aphasic regression has proved to be a mirror of the child's acquisition of speech sounds, it shows the child's development in reverse." p. 57), the authors turn their attention to higher levels of aphasic dysfunction. Again the approach is linguistic. Linguistic signs involve two modes of arrangement. The first mode is *combination.* Any sign is itself made up of constituent units and/or participates in a larger combination with other signs, a superior unit. The other signs form the sign's context. The second mode is *selection.* Any sign, whether simple or complex, is a selection from alternatives, with the possibility of substituting one for another. These two modes can be observed in free word associations. With *hut* as the stimulus word, the response can be something like *burnt.* This is a response in the combination mode (so-called "syntagmatic association"). Or the response could be something like *cabin,* which is in the selection mode ("paradigmatic association").

These modes can be independently affected in aphasia. When the combination mode is defective, the patient has a "contiguity disorder." The patient shows agrammatism, a loss of the ability to "propositionize" (Hughlings Jackson's expression). The combination mode can also fail at the morphological level. In this case there is an abolition of inflection; words appear in their *unmarked* version. Also, morphological derivation becomes irresolvable. "Patients who understood and uttered such compounds as *Thanksgiving* or *Battersea,* but were unable to grasp to say *thanks* and *giving* or *batter* and *sea,* have often been cited." (p. 73).

When the selection mode is defective, the patient has a "similarity disorder." The person is unable to select key words and replaces them with anaphors or abstract substitutes. Here some of Goldstein's examples of amnesic jargon are cited.[71] There is a defect of substitution. If an object is pointed at, it is in consciousness and cannot be substituted by the appropriate word. Or a word in consciousness cannot be substituted by its definition,

[71] Such as: "Ich bin doch hier unten, na wenn ich gewesen bin ich wees nicht, wie das, nu wenn ich, ob das nun doch, noch, ja." (Jakobson and Halle 1956, p. 65).

by a synonym or a circumlocution. More generally, there is a loss of metalinguistic ability.

The authors argue that metaphor is akin to the similarity disorder and metonymy to the contiguity disorder. Metaphor involves the ability to substitute, which is lost in patients with similarity disorder. Metonymy involves the ability to relate to context, which is diminished in contiguity disorders. There is an interesting relation of these disorders to two different literary styles, the authors suggest.

Jakobson remained an exception in aphasiology until the "cognitive revolution." The field suffered from a persistent lack of linguistic sophistication in both method and theory.

Theodore Weisenburg and Katherine McBride: aphasia is diverse

The Philadelphia neurologist Theodore Weisenburg (1876–1934) died just before his book *Aphasia. A clinical and psychological study*, co-authored with Katherine McBride (1904–1976), appeared in 1935. Weisenburg had been Charles Mills' student and had become his successor at the University of Pennsylvania. The research published in this book was occasioned by the unexpected finding that many tests from Head's aphasia test battery were performed no better by people not suffering from aphasia than by people with it. Weisenburg and McBride set out to do a large-scale comparative group study. They subjected 60 aphasic patients (without other mental disorders) to a large battery of tests and compared them to 22 right-brain damaged controls without aphasia and to 85 matched people with no brain damage.

The tests provided only limited support for Head's four types of aphasia and Head's tests did not discriminate well. They had been developed, the authors remark, too much from the theoretical preconception that aphasia is a symbolic disorder. In many cases it is not. On the basis of the extensive test results, patients could be partitioned into four groups. The first and largest group consisted of patients with a dominantly *expressive* disorder. This was, essentially, the traditional category of motor or Broca's aphasia. The second and next largest category was formed by patients with a dominantly *receptive* language disorder. Although this is the traditional category of sensory or Wernicke's aphasia, the data revealed systematic and often quite marked expressive disorders in these patients, such as paraphasias and paragrammatisms. The next largest category of patients were classified as *expressive-receptive*. They suffered from global aphasia with severe limitations in the use of speech and language. A final, quite distinct group of patients were *amnesic*, with the rather restricted inability to access words when speaking but without loss of fluent expression for set phrases and with preserved word recognition. This, again, confirmed a classical syndrome. On the other hand, there was no evidence in this patient population for the existence of "pure" cases, such as pure word deafness, pure alexia, pure agraphia, etc. This should not be a surprise, because such pure cases (if they existed at all) were known in the literature to be exceedingly rare. The probability of their showing up in an average sample of aphasic patients is negligible.

Weisenburg and McBride reviewed the major theories of aphasia from their down-to-earth empirically grounded perspective. Their main conclusion on the basis of their own

records was "that aphasia cannot be understood as a unitary disorder" and "the only single hypotheses which could be developed to explain aphasia as a whole are so general as to be of little value" (p. 430). The authors distance themselves, respectfully, from Jackson's too general claim that it is the highly organized, automatic responses that are best preserved in aphasia. They reject Marie's view that all aphasia is an intellectual disorder; there are too many cases where intelligence is well-preserved in people with aphasia. They also dismiss the all-encompassing deterioration-of-Gestalt explanation of aphasia, that Goldstein, Van Woerkom (1931), and others had proposed. It is often, but not necessarily, the case that higher-level, complex organization breaks down in aphasia. There is no "regular regression"; aphasia is highly diverse in the first place. Closely related to this point, they argued that it is wrong to characterize aphasia as a regression to more "primitive" language use, a more primitive level of thinking as exhibited by children (as argued by Van Woerkom 1921) or, supposedly, in "primitive" peoples. "The aphasic patient is not more primitive in his behavior as a whole nor in many of his specific responses." (pp. 459–460). There is also no evidence for Goldstein's claim that people with aphasia suffer from a changed personality. Many of them maintain their normal network of personal relations. Finally, and more specifically, Weisenburg and McBride reject Goldstein's theory of amnesia as a general loss of "abstract attitude." There is no evidence for this in their patients; amnesia is just a distinct word-finding disorder.

The records are of necessity somewhat inconclusive with respect to issues of localization. At the time Weisenburg and McBride wrote their book, there were post-mortems for only three of their 60 patients. For others, in particular brain tumor patients, there was evidence from surgical operations. Still, the authors align with Jackson's adage: "To locate the damage which destroys speech and to locate speech are two different things." They add: "It is evident that it is impossible to localize language." Language always permeates thought and is inseparable from it. "That it is the result of the activity of the entire brain, however, there is no doubt." (p. 467). They pit their "dynamic" view against that of the strict localists, such as Henschen and Kleist. Still, they were not holists or equipotentialists. They hold that expressive forms of aphasia dominantly involve anterior regions of the left hemisphere, the receptive ones the posterior, temporal regions. But they add a new argument against strict localizationism, somewhat like Marie's (1922): "Recent evidence shows that the nerve impulses of the brain . . . consist of waves of electrical energy" . . . "Such a process, speech, is a live dynamic electrical function and cannot be measured." (p. 468). The authors, apparently, missed Hans Berger's (1929) classical paper on electroencelography, although they did cite his earlier paper on dyscalculia.

Aphasia by Weisenburg and McBride was the first systematic group study of aphasia and they were the first to systematically use brain-damaged people and controls. It set new standards, which survive till the present day.

Other American contributions

Weisenburg and McBride's work was on a par with the approach we saw in American child language research; developing careful diagnostic tests, performing group studies

with appropriate controls, eschewing "deep" theories, application-oriented. In 1955 Hildred Schuell (1906–1970) published her *Minnesota test for differential diagnosis of aphasia.* Schuell had a PhD from Iowa State University and then moved to the Minneapolis VA Hospital in 1948. The "Schuell test" or "Minnesota test" became the standard for a long time. It reflects the careful psychometric tradition pioneered by the Child Welfare Institutes of Iowa and Minnesota, which we discussed in the previous chapter; Mildred Templin was one of Schuell's Minnesota colleagues. Schuell was also a pioneer of speech therapy. Her "stimulation approach" aimed to re-activate impaired functions by auditory stimulation.

Together with James Jenkins and John Carroll she developed her test further, adopting useful features of the "new psycholinguistics."

In aphasia research, application meant therapy. Goldstein's influence on American aphasiology was not so much through his encompassing theory, but by way of his approach to person-centered therapy. That is quite apparent from both Granich's (1947) guide to the retraining of war veterans and from Joseph Wepman's (1950) guide to therapy, *Recovery from aphasia.* An important impetus for this work had indeed come from the Veterans Administration, which was responsible for therapy and re-integration of brain-injured World War II veterans. A substantial literature on therapy procedures and their evaluation arose in the post-war years, on both sides of the Atlantic.[72] It is, however, beyond the scope of this book to review these developments in any detail.

More generally on the American scene, by the 1930s issues of precise localization had all but disappeared from the research agenda. As Johannes Nielsen (1890–1969) put it in the preface to his 1936 book: "the medical world has had time to realize the errors of its ways and to abandon its naïve idea that aphasia could be of diagnostic value in cerebral localization." Still, Nielsen went through the trouble of reconsidering the issue of localization in aphasia. His book, for which he could not even find a publisher and which he ultimately published on his own, was based on cases from the literature, in particular Henschen's compendium, as well as his own patients. Nielsen argued that the empirical basis for studying anatomical localization had substantially shifted in recent years. Traditional work had almost exclusively depended on post-mortem brain anatomy, but brain surgery had meanwhile become another major source of evidence, evidence from the live patient. Nielsen concluded that his data were in large part "confirmatory of the old doctrines" on localization. He considered his own main contribution to be the specification of a "language formulation area" consisting of Brodmann's area 37 (i.e., the inferior temporal gyrus), in combination with areas 21 and 22. It is area 37, according to Nielsen, in which the inner speech appropriate to a given concept is generated. Specific disturbance of area 37 leads to "formulation aphasia," characterized by paraphasias or jargon.

A final American contribution that should be mentioned is Osgood's processing model of aphasic disturbances, summarized in Levi-Strauss *et al.* (1953, pp. 50–51).

[72] In England, Butfield and Zangwill (1946) pioneered the assessment of therapeutic efficacy.

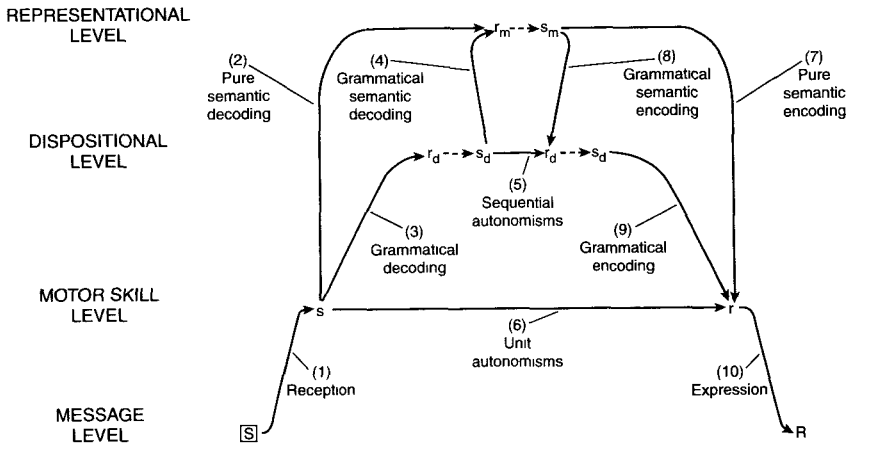


Fig. 11.8 Osgood's mediation account of the sign and the assign process. Reproduced from Levi-Strauss, C. *et al.* (1953) *Results of the conference of anthropologists and linguists.* Baltimore: Waverly Press, p. 51. © The Authors.

Like Kussmaul and Pick before him, Osgood took as his starting point the *normal* processes involved in the encoding and decoding of language. Aphasic syndromes result when one or more of these processes is disturbed by brain damage. Osgood in fact reversed the approach by arguing that aphasic symptomatology should be used as a test of the normal processing model, the theoretical model of linguistic behavior. Figure 11.8 shows Osgood's model. It is, clearly, a more detailed and sophisticated version of the models in Osgood and Sebeok's (1954) survey. It is mediation-theoretical, but actually the first mental processing model of the language user in modern psycholinguistics.

Osgood considered possible lesions of his model and related them to reported cases in the literature. He collected 39 fully reported cases from Head, Goldstein, and others, whose performance he could score on maximally 28 kinds of language performance. A contingency analysis then showed which language performances are correlated or independent. Correlated performances should involve overlapping or common pathways in the model. Osgood then tested the model by "damaging" it in 21 different locations and predicting the affected performances. This gave a satisfactory account of the actual performance correlations. Osgood was far ahead of his time with this aphasia approach to testing normal processing models of the language user.

Alexander Romanovich Luria

We met Luria in Chapter 10 when we discussed his and Vygotsky's criticism of Piaget's "egocentric speech" notion. At that time, the end of the 1920s, Luria was primarily a developmental psychologist, working under Vygotsky's mentorship. Luria the neuropsychologist was still to develop. In fact, Luria's contributions to aphasiology were hardly known in the West before the "cognitive revolution." It was only in 1970 that his major

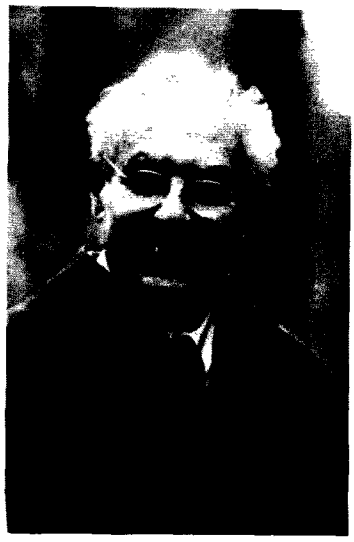


Fig. 11.9 Alexander Romanovich Luria. © University of California, San Diego. Alexander Luria Home page http://luria.ucsd.edu/index.html

work *Traumatic aphasia* appeared in English translation. The original Russian version had appeared in 1947, with a revised edition in 1959. The English edition involved a second revision.

Alexander Romanovich Luria (1902–1977, Fig. 11.9) had been one of the "troika," together with Vygotsky and Leontiev,[73] who had set out to renovate Soviet psychology during the late 1920s and early 1930s. They broke with Pavlov's reflex psychology and the "telephone switchboard" metaphor of the brain (see M. and S. Cole's *A portrait of Luria* 1979). Luria adopted a systems approach inspired by Karl Lashley. At the same time, the troika took a cultural-historical perspective on cognition, language being considered as one of the cultural tools for thought. Also, they considered Gestalt formation as culturally determined in the first place, not as a biological property of the brain. In the mid 1930s, however, shortly after Vygotsky's death, the political and scientific tide in the Soviet Union turned against the views of the "new" psychology and of Luria in particular. A ban on publications and the pressure of obligatory public self-criticisms made Luria decide to pursue his medical degree and follow an internship at the Neurosurgical Institute of the Moscow Medical School. The war years and the flood of victims with traumatic brain lesions turned Luria into the outstanding neuropsychologist we now know. Aphasia, its theory and therapy, became his core project. *Traumatic aphasia* is a book in three parts. Part I deals with the phenomenology of aphasias and the underlying brain anatomy and physiology. Part II presents the neuropsychological diagnostics of aphasia and Part III treats rehabilitation procedures. Here we will be concerned with Part I only and touch upon some of its core topics.

[73] Alexei Leontiev (1903–1979) later specialized in motivation and action theory. In 1969 he published a textbook of psycholinguistics, a German translation of which appeared in 1971.

The systems approach

In his autobiography (1979) Luria describes the great impression Head's holistic approach to aphasia and its brain anatomy made on him in the 1930s. But in the end Luria did not become a holist. Unlike both Head and Goldstein, Luria claimed that focal brain lesions do not lead "to general, diffuse disturbances but to *selective, systemic* disturbances." (p. 24). Focal lesions can undermine the functioning of specific, localized "analyzers" in the brain. This is the "primary" effect of such a lesion. But there will also be *systemic* effects. "If a given analyzer begins to function poorly, it is inevitable that *all complex functional systems dependent upon the kinds of analysis performed by that analyzer will also suffer.*" (p. 24). Luria correspondingly distinguished between symptoms and syndromes. A symptom is the sign of a failing analyzer. A syndrome is the characteristic of a malfunctioning complex system. Aphasic syndromes cannot be specifically localized in the brain (as Head and Goldstein had argued), but symptoms can. The art is to discover and localize the specific analyzers whose dysfunction upsets the complex functional system, be it speech comprehension, writing ability, or whatever. Luria's best example concerns the "phonemic analyzer" to which we will turn shortly. The cortical regions between the "primary analyzers" perform integrative functions within complex systems. Luria called them the "marginal zones." They are not the primary speech areas such as Broca's and Wernicke's, but bordering zones such as premotor, anterior temporal, and inferior parietal regions. Focal lesions in these areas often cause transitory speech disorders.

Data base

By early 1943 Luria's team had available "more than 800 cases of brain trauma inflicted by fire arms" (1970, p. 28). Of these, 394 had suffered left hemisphere damage with 240 showing signs of speech impairment. These left hemisphere patients constituted Luria's database. They were first examined hours or days after trauma, the "initial phase," and then again two to five months after trauma, the "residual phase." For all patients the site of skull injury was determined. It was used as a rough indication for the site of brain damage. Throughout, Luria provides statistics for types and severity of speech disorders as related to these sites of damage. The statistics show, for instance, that frontal and occipital injuries hardly ever caused speech disturbance. Damage of the superior parietal region tended to cause slight speech difficulties, whereas lesions in the perisylvian zone, i.e., the classical language area, did cause substantial degrees of aphasia. Figure 11.10 shows Luria's basic statistics.

The structure of speech activity

The systems approach requires analysis of the processes involved in language use. What are the "analyzers" involved in the comprehension and production of speech, in reading and writing? Luria makes reference to Pick's process analysis, but does not elaborate it. He rather takes the genetic point of view, theorizing about the early evolution of language and considering the early speech of children. In both cases the critical step has been the separation of the verbal code from context. Initially all signs, whether manual or verbal,

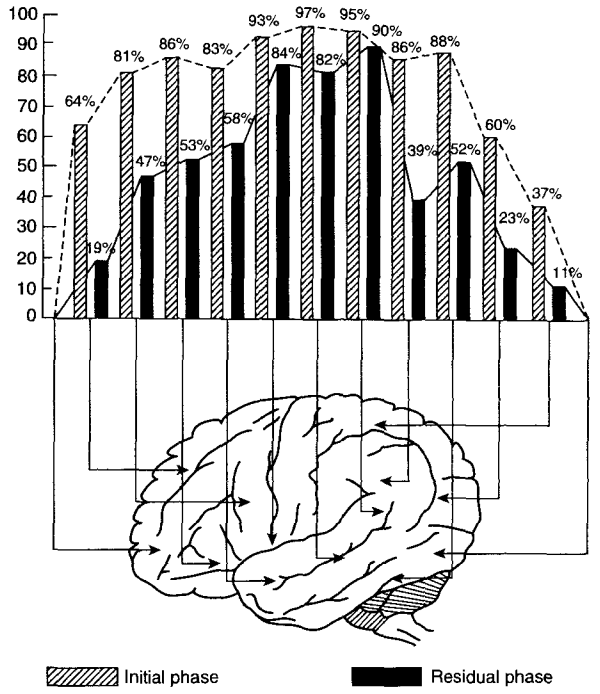


Fig. 11.10 Distribution of aphasias following injury to the left hemisphere. Reproduced from Luria, A.R., *Traumatic aphasia. Its syndromes, psychology and treatment.* (revised translation of the 1947–1959 Russian original) © Walter de Gruyter & Co, 1970 with permission.

functioned exclusively in a "sympractical context" (p. 81). But ultimately the phonetic, lexical, and semantic codes became a new "second signaling system of activity" (note the obligatory salute to Pavlov). Even in its sympractical phase, speech performs two major functions, a nominative and a predicative one. In the developed system nomination is expressive of *concepts*, not images. Predication is closely tied to the speaker's motives and intentions. The process of speaking, then, moves from intentions, to inner speech to overt speech. Inner speech mediates between thought and overt speech. It is a shortened, not yet sequentially patterned version of the overt speech that is to develop. It is the "internal semantic aspect of speech" (p. 84).[74] The sequential nature of overt speech requires the activity of dedicated analyzers, in both speech understanding and production. Luria has disappointingly little to say about these analyzers. As mentioned, his best case is the phonemic analyzer.

Phonemic analysis

Each language has its own specific phonemic system. Here Luria cites, among others, Baudouin de Courtenay, Trubetskoy, and especially Roman Jakobson. In fact, he entirely

[74] Notice, that here we are back to "inner speech" in the sense of Humboldt, Steinthal, and Wundt.

follows the latter in his presentation of phonemic hearing (Jakobson 1941). It is the system of phonological distinctions or oppositions, which guarantees the constancy of a word, in spite of variations in timbre, intensity or rate. That requires a specific analyzer, which Luria localizes in the "postero-superior parts of the left temporal lobe" (p. 119), i.e., in Wernicke's area. If this phonemic analyzer fails, various functional systems that rely on it will be affected. Words can no longer be uniquely recognized when relevant phonemic distinctions have been lost (like Jakobson's *Rippe* and *Lippe*). When words cannot be identified through auditory input, their meanings will not be captured by the patient and phrases in which they occur will not be understood. There will be both literal and verbal paraphasias (literal: the Russian word *kolxos* is repeated as *kolz* or *glos*; verbal: *glaza* –"eyes" for *očki* –"glasses"). At the same time, the patient will show a disorder of writing, in particular writing to dictation. For all but the most frequent words, writing requires phonemic analysis of the target word, which has become impossible. There will also be trouble in reading because letters no longer map to specific phonemes, and so on. The phonemic analyzer is part of the "temporal lobe systems." Luria further distinguishes two types of frontal systems relevant to motor aphasias and a "parieto-occipital" system involved in semantic aphasias. Let us consider them in turn.

Temporal lobe systems

Luria rejects Marie's view that (temporal) aphasia is a unitary phenomenon, a general intellectual disorder. There are at least three zones within the left temporal lobe that perform different and specific functions for speech. There is, first, Heschl's transverse gyrus (Brodmann's area 41). It is the primary projection area for auditory input. If Heschl's area is bilaterally damaged, central deafness occurs, with predictable consequences for the perception of speech. There is, second, Wernicke's zone (Brodmann's area 22).[75] Damage to this region causes a disturbance of "complex discriminative hearing" (p. 115). Here Luria presents extensive evidence for the aforementioned role of this zone in the phonemic analysis of the speech signal. And, third, there is Brodmann's area 37 in the inferior left temporal lobe, which together with the posterior part of area 21 performs specific functions in auditory memory. Patients with focal inferior temporal lobe damage can understand speech, but cannot remember words or phrases when asked to repeat them. They often "lose" a word or phrase before having understood its meaning. In more severe cases, they cannot access the meanings of words; there is a general "alienation" (p. 137) of word meanings. The patient becomes "acoustic-amnestic" (p. 148). Luria does not make the reference, but the syndrome is close to Lichtheim's "transcortical" sensory aphasia in which word images are preserved, but the connection to the corresponding

[75] Luria nowhere defines Wernicke's area. He speaks of the "acoustic-psychic zone," which includes Brodmann's areas 21 and 22 (p. 112). There is almost general agreement in the literature that Wernicke's area includes BA 22. For some the area, or its anterior part, *is* Wernicke's area. Others extend it as far as to include BA 22, 39, 40, 41, and 42, because they all play some role in the understanding of language. See also p. 367, note 6.

concepts is disrupted. Luria stresses time and again that disruption of the three zones does not cause diffuse intellectual disorders, but rather quite specific systemic syndromes.

Frontal systems

Luria distinguished three motor aphasic syndromes. *Afferent* (or *apraxic*) motor aphasia is essentially the "pure" or "peripheral motor aphasia" described by Dejerine, Liepmann, Goldstein, and others. Inner speech[76] is preserved, but there is a disorder of articulatory-phonetic organization. Luria speaks of an *afferent* movement disorder, following Liepmann (1900). The reason for this is that the disorder is believed to result from insensitivity to proprioceptive feedback from the articulatory musculature. The patient cannot "feel" whether the intended articulation of a speech sound is in fact correctly performed. "Schemata of articulation," in particular the schemata governing the *spatial* constellation of the articulators, can no longer execute their executive control. On the basis of his lesion data, Luria concludes that afferent motor aphasia is principally caused by lesions in the Rolandic operculum, which is the directly postcentral, inferior part of the left parietal lobe.

Efferent (or *kinetic*) motor aphasias result from damage in the left premotor area. If the lesion is in the upper part of that area, i.e., above Broca's area, speech is possible, but loses its smoothness of delivery. The delivery is word by word rather than phrase by phrase; speech is "halting." Here, the dynamic organization of speech, its "kinetic melody" (p. 171) is affected. If the lower part of the premotor area, which includes Broca's area, is affected, then classical Broca's aphasia is likely to result. There is a "loss of the inner structure of the speech act," a loss of its dynamic constellation. The schemata of word articulation have lost their *temporal* constellation. With the loss of these inner schemata, writing becomes similarly affected. Successive writing movements are governed by the same dynamic word schemata. When inner speech disintegrates, the formulation of thought, predication, becomes impeded. "It is these dynamic schemata of propositionalizing . . . which suffer most in efferent forms of motor aphasia." (p. 189).

Frontal dynamic aphasia is what Goldstein had called "central dynamic aphasia." Here the patient is deprived of Head's "symbolic formulation and expression." In pure form, this type of aphasia is rare. It results from "lesions involving the lower parts of the left frontal lobe just anterior to Broca's area" (p. 199). The mechanism is not well understood, according to Luria. Clinically it involves loss of verbal initiative and impairment of active thought.

Parieto-occipital systems

"Patients who have been wounded in the occipito-parietal area[77] have no difficulty in speaking smoothly or hearing and understanding individual words," Luria writes, but

[76] Here it is "inner speech" in the new sense of articulatory-auditory image.

[77] Luria refers to Brodmann's areas 37, 39, and 40 (p. 225).

they "have a tendency to forget names of objects" and are subject to "confusion if the examiner confronts them with a somewhat complex or unusually constructed question." Deeper examination of such patients shows "*a profound disruption of the epistructure of words.* . . . The primary image represented by a word, i.e., its specific object relationship remains intact. But the system of relationships centered about the word is profoundly disrupted." (p. 228). "The movement from concept to concept . . . or, to use Bühler's expression, according to a 'synsemantic' plan, becomes extremely difficult." (p. 229). The patients cannot tell *a circle under a cross* from *a cross under a circle*, or correctly interpret *Cate is darker than Mary, but lighter than Jenny.* They suffer from "receptive agrammatism" (pp. 230–231). There is no grasp of larger constructions, and this often extends to non-verbal spatial relations as well. The syndrome had been recognized before by Head, and Luria also makes reference to Nielsen's work on this syndrome. At an earlier stage Luria had considered this syndrome as *amnesic* aphasia (see Luria 1979, p. 135). But later he realized that there are at least three types of amnesic aphasia with quite different underlying causes. Apart from the amnesia shown by semantic aphasics, there is a temporal-acoustic variety caused by disruption of the phonemic analyzer and a frontal-dynamic variety caused by loss of the dynamic schemata for words and sentences. "*Amnesic aphasia* will cease to be classified as an independent clinical entity." (p. 244). Similarly, the inability to repeat words, the classical litmus test for "conduction aphasia," is not a unitary phenomenon either, according to Luria. It can result from both temporal and frontal syndromes.

Luria's project is a monument in the history of aphasiology. Under the pressure of a devastating war he managed to design and keep control over a huge, systematic study of brain-damaged war victims. There was considerable medical, psychological, linguistic, and neurological sophistication in his balanced approach, setting new standards for method and theory. We will return to other studies of brain-damaged war victims in Chapter 15, but Luria's will remain the prototype in the history of psycholinguistics.

Retrospect

In spite of its length, the present chapter could not be comprehensive in its review of aphasiology. We have not discussed issues of measurement and therapy, and we have ignored many, if not most, of the secondary players in the field, among them many a first-rate scientist. Rather we largely concentrated on the theoretical views of the dominant figures of the time. The main distinction between these views is localizationism versus holism. This division existed in both the psychological and anatomical domains, but they tended to go together. Psychologically, the localizationist stance is, what we now call, a "modular" one. The mind's processes and faculties function in relative independence; interaction or integration takes place at "higher" levels of processing. Most of nineteenth-century aphasiology was of this kind, with the noticeable exception of Hughlings Jackson. Anatomically, the localizationist stance was to localize images, memories, or processes in specific circumscribed regions of the brain. In nineteenth-century aphasiology the talk was of stored sensory and motor images, but the modular view of mind was usually not extended to specific localizations for concepts or thoughts.

The twentieth-century holists rejected these views on either anatomical or psychological evidence, or both. Monakow stressed the holistic response of the brain to focal trauma. Marie's "revolution" was formulated in both psychological and anatomical terms, although both lacked precision. Pick almost exclusively argued his moderate holism from a psychological process theory. Head's explicitly holistic arguments were psychological in the first place, where he characterized aphasia as being a disorder of symbolic representation. The widespread belief that larger brain zones had to be equipotential in their functioning was derived more from such holistic psychology than independently demonstrated. Goldstein's holistic theory had a more romantic psychological touch: Any aphasic syndrome was seen as the response of the whole person to some malfunction; it was a loss of general Gestalt-forming abilities. Goldstein, however, never gave up the search for anatomical localization, especially of primary sensory and motor functions.

Although the interest in anatomical localization clearly diminished among aphasiologists between the world wars, it never disappeared, with Henschen, Kleist and Nielsen as major proponents. It was Luria especially who, during World War II, proposed a new "systemic" approach to relating a modular psychology to a network of functionally specialized brain regions. Together with a substantially improved diagnostic tool kit, the scene was receptive for a renaissance of studying specifics of psycholinguistic functioning in their relation to the brain's anatomical and physiological detail. Penfield, Hécaen, Zangwill, Quadfasel, Goodglass, Geschwind, and Milner, among others, were in the starting blocks. We will return to some of these issues in Chapter 15.

Chapter 12

Empirical studies of speech and language usage

Around the turn of the century, there was great promise for the empirical study of speech and language processes. Wundt's laboratory had pioneered some of the now standard paradigms in speech perception and production, association, and reading. James McKeen Cattell, for instance, had run the first sophisticated experiments on word, picture, number, and color naming. In Berlin, Ebbinghaus had masterminded the experimental study of verbal memory. Erdmann and Dodge in Halle and Edmund Huey in Pittsburg had innovated reading research by their analysis of saccade and fixation patterns. Huey's 1908 book became the classical text on reading for the next half century. Last, not least, Meringer and Mayer had established the still modern speech-error-based analysis of the speaking process.

Was this promise redeemed during the early twentieth century? Not really. The great German tradition began to peter out after the disaster of World War I and entirely disappeared soon after Hitler's appearance in 1933. The Americans admittedly took over, but their marriage to behaviorism formed an impediment for a chapter of psychology, which is pre-eminently concerned with mental processes. What remained was largely the study of the periphery of these processes. Acoustic and articulatory phonetics, for instance, thrived and so did the study of eye movements in reading, but just eye movements without their relation to the underlying process of comprehension. Speech error research entirely disappeared, and so did Selz's procedural approach to sentence formulation. But the United States would not be its pragmatic self without applied research, in this case mostly serving the educational testing of verbal and reading abilities. Entirely by chance, it produced the occasional experimental breakthrough, as we will see.

One noticeable new direction was pioneered during the 1930s by a psychological outsider, George Zipf, linguist at Harvard University. He made great efforts to develop the statistical approach to text and text production. It turned out to be of lasting relevance to psycholinguistics. Another direction was the application of Shannon's information theory to verbal behavior, as pioneered by George Miller and his associates. It would, in fact, become the final "upbeat" for the cognitive revolution in psycholinguistics, a theme we will return to in Chapter 15.

What we have to face in this chapter is that there was little cumulative development in the core domain of adult psycholinguistics. How does the speaker manage to verbally formulate intentions, to act by means of speech? How does the listener manage to infer the speaker's intentions from an utterance? How do the two of them negotiate meaning?

How does our language interact with our perception and thought? These were not the questions studied in the laboratory or by systematic observation.[1] It was only piecemeal that these issues were touched upon. Our approach will have to be down to earth, considering themes judged relevant by the researchers themselves. They were in particular (1) the perception and production of speech sounds and spoken words, (2) the study of meaning through association, phonetic symbolism, metaphor, and scaling, (3) verbal learning and memory, (4) the statistical approach to text, (5) individual differences and speech as the expression of personality, and (6) reading. We will take them up in that order.

Perception and production of speech and language

When you hear a speech-like stimulus and are asked to reproduce it, the task is as much one of perception as production. As long as the stimulus is not perfectly clear, you will make a best guess and be subject to response bias. You will tend to produce a more frequent or familiar spoken response. In the following we will move from dominantly perceptual to dominantly production studies of speech. On the perceptual side, we will review studies in the perception of speech sounds, consonants, and vowels, Fletcher's more applied speech intelligibility studies, and studies of word perception. On the production side we will discuss quite limited work on speech errors, research in articulation and the innovative work by Lee and others on delayed speech. In between these two sets of studies is Skinner's work on the "verbal summator," which is largely a study of response bias.

Perceiving consonants and vowels

The perception of individual speech sounds had been a returning topic in nineteenth century physiology, especially in relation to the clinical testing of auditory acuity (see, for instance, Wolf 1873). We will not follow the clinical literature, and experimental studies on normal, adult, consonant and vowel perception were fairly scarce. There was a major, almost four decades gap between the early studies by Wells (1906), Gutzmann (1908), Köhler (1910), and Ruederer (1916) and the landmark Miller and Nicely study of 1955.

Frederic Lyman Wells (1884–1964), after studying German, wrote his doctoral dissertation under the supervision of Cattell, Thorndike, and Woodworth at Columbia University. The title *Linguistic lapses* is somewhat misleading. There is indeed a small section on speech errors, but the bulk of the dissertation reports on an experiment in speech sound perception, i.e., on lapses of the ear. Wells presented his subjects with meaningless trisyllables of the type CVCVCV, for example *monaku* and *ledibu*. A list of 96 such non-words contained each of 16 test consonants evenly occurring in all positions and each of five test vowels evenly in each position. The second syllable was always slightly stressed. The subject would listen to just one such list per day. On the second day subjects would get a new list of 96 non-words, and so on to a total of 12 test sessions. The experimenter,

[1] See, for instance, Adams and Powers (1929), a review of a scattered discipline.

facing the subject at a distance of 3 meters, would read each item, after which the subject wrote it down. Hence, there was no additional noise or distortion; hearing conditions were optimal. Still 23% of the consonants were misheard; *p*, for instance, was often misheard as *k* and *m* as *n*. Wells spared no pains to analyze the confusion tables. A first major surprise was that confusions were often not symmetrical. For instance, *h* was often heard as *p*, but *p* rarely as *h*. This should not be the case if confusion is solely the result of similarity. Similarity must be symmetric, *p* must be as similar to *h* as *h* is to *p*. Wells discovered what we now call "response bias": "Obviously there are certain sounds having a tendency to be heard rather than other sounds." (p. 64). Wells then shows that the response bias is towards sounds that are well-perceived. For instance, *k* is rarely misperceived but relatively often given as response, i.e., such speech sounds "tend to be heard." However, there are also great individual differences here. One further analysis of Wells is quite "modern." How do confusions relate to phonetic features? Errors tend to preserve "manner" at the expense of "place." Plosives are heard as plosives, spirants as spirants, etc. But place of articulation is easily lost, *k* being heard as *p*, *m* as *n*, *b* as *g*, etc. Wells calls this "the ground principle of auditory substitution. The articulation place is changed – the articulation method is preserved." (p. 51). The cause must be sensory, because changing the place of articulation is a major change from the motor point of view. This featural analysis foreshadows Miller and Nicely (1955).

Wells also included the vowels *a*, *e*, *i*, *o* and *u* in his perceptual analysis. Vowel discrimination was, from best to worst, *a–o–u–i–e*. This correlated slightly with the subject's response bias, as was the case for the consonants.

Vowel perception was also the topic of Wolfgang Köhler's (1910) dissertation. His supervisors were Carl Stumpf and Max Planck in Berlin. Köhler (1887–1967) used tuning forks, on a pitch scale from 125 to 4000 cps, and asked his ("trained") subjects to judge them on a vowel scale from *u* to *i*. A 550 cps tone was heard as *o*, a 1100 cps tone as *a*, a 2100 cps tone as *e* and a 4000 cps tone as *i*. Köhler claimed that the perception of formants in vowels is based on this elementary perceptual scale. In addition, the scale is octave-based: "pure" *a* is the octave of "pure" *o*, *e* is the octave of *a* and *i* is the octave of *e*. It was a heroic theory, too good to be true (cf. Stumpf 1914). Jaensch (1913), just appointed professor at Marburg University, used a clever siren contraption to find out what it is that makes a sound vowel-like. He demonstrated that a pure tone turns into a vowel-like sound if the periodicity is slightly disturbed.[2] He then managed to independently vary vowel quality (*a*, *o*, *u*, etc.) and vowel pitch in the following way. He had the siren produce a periodic sinewave fundamental tone on which he superimposed a slightly non-periodic higher tone. The two could be "programmed" independently. He found that the non-periodic higher tone determined the vowel quality and the fundamental periodic tone determined the perceived pitch of the vowel. In other words, the vowel's character was determined by the non-periodic higher "formant," a term used by Jaensch. This was Jaensch's "duplicity theory." We will return to both Köhler and Jaensch in Chapter 14; they were to play totally opposite roles during the Nazi period.

[2] By non-periodic pitch variations or by non-periodic phase shifts.

Two years after Wells (1906), Hermann Gutzmann (1865–1922) published a similar study of perceptual confusions, of German consonants. We met Gutzmann earlier as the editor of Kussmaul's textbook. He was the second of a three-generation dynasty of speech therapists. He established phoniatrics as an academic discipline in Berlin, where he had a professorial appointment in the ENT clinic of the Charité. He published influential books, including some on children's speech disorders and stuttering, and a standard text (1909) on voice physiology. He was also a dedicated experimenter. His 1908 paper reviews his earlier speech perception experiments and adds a new one. In all experiments, subjects would hear a nonsense word and write it down immediately. In the earlier experiments the words were spoken by Gutzmann himself, either in direct contact or over the telephone, but the ultimate experiment involved the phonograph and a streamlined procedure. The 20 stimuli recorded on the phonograph were all three-syllabic nonsense words with CVCVCV structure, exactly as in Wells's study, whose work is not cited. In contrast to Wells' stimuli, the vowel was always *a*. Seventeen different consonants were used, creating stimuli such as *gabada*, *parala*, *wasaja*, and *tapaka*. The subjects' written responses were tabulated and the types and ratios of misperceptions were analyzed. The results confirmed Gutzmann's earlier findings. There are usually five confusion groups, (*p*, *t*, *k*), (*b*, *d*, *g*), (*sch*, *f*, *ss*), (*m*, *n*, *ng*), and (*w*, *s*, *j*),[3] whereas no errors were made in the perception of *r* and *l*.

Like Wells, Gutzmann considered response bias. The hearer will tend to respond with "words that are closest to his whole thought world, his present constellation of images." Among his examples is one of an "18-year-old, fickle young man, much in love; everything female attracts him and the constellation of images is easily discernable from the frequent use of girl's names."

Hans Ruederer's 1916 dissertation with Bühler in Munich took a different approach to the perception of speech sounds. It was not a study of perceptual confusions but of thresholds. Ruederer tested his (trained) subjects in the open field, in fact following Wolf's (1873) paradigm, discussed in Chapter 5. In his main experiment Ruederer would begin at a (pre-tested) distance from his subject and produce his test sound. That could be a consonant in *a-a* context (such as *ascha*) or a vowel. There were 27 different speech sounds in the test. The test began at a distance at which the sound could just be perceived but not be identified. That ranged from about 30 to about 300 meters (for *h* and *a* respectively). Ruederer would then move closer by half a meter and repeat the presentation. This went on till the subject identified the speech sound correctly and with subjective certainty. Hardest to identify was *h* (at 21 meters), next was *d* (at 39 meters), and so on till *a* (at 264 meters). The recognition thresholds could now be expressed as a distance ratio with the critical distance for *h* (21 meters) as the nominator. So, *a*'s perceptibility value was a high 12.5. Ruederer's table shows that the values of all vowels were higher than the values of all consonants. He then repeated the same experiment in the mode of whispering the speech sounds. This time the vowels lost their total superiority. The sibilant

[3] This follows Gutzmann's own notation. It is self-evident, except for *ss* standing for /s/ and *s* standing for /z/ as is normal in German.

consonants, for instance, did better than all vowels. Ruederer added a characteristic Würzburgian experiment. Here his subjects listened to phonographically recorded speech sounds and had to report what they experienced. For example, with *t* as a stimulus, the subject would say "a sharp, momentary noise (etc.)." Ruederer then analyzed the written records (known as protocols) for common, recurrent features. Each consonant, Ruederer concluded, has a single dominant feature. There are just four of them: voicing (as in *l, m, n, w*), plosive (as in *d, b, g, t, p, k*), spirant (such as *f, s, sch, z*), and the articulation place of the sound, which was often mentioned as its most characteristic feature. Ruederer's trained subjects happily displayed their knowledge of phonetics.

Several decades later, in 1955, Miller and Nicely returned to the study of consonant perception. They tested perceptual confusions of English consonants. Their experimental paradigm was closely related to Wells' and Gutzmann's and so are the results. But Miller and Nicely did not refer to these much earlier studies. Miller, always liberal in acknowledging historical sources, must have missed them. The subjects listened to monosyllabic CV stimuli, where the vowel was always *a*, as in Gutzmann's experiment. The authors used 16 different consonants. Subjects responded orally, not in writing. In addition, Miller and Nicely systematically filtered their stimuli to different degrees. But so did Gutzmann, although by force majeure. His phonogram must have been a low-pass filter, comparable to Miller and Nicely's 200–2500 cps condition. In that condition, Miller and Nicely's confusion groups were (*p, t, k*), (*b, d, g*), (*f, s, θ, ʃ*), (*m, n*), and (*v, ð, z, ʒ*), quite similar to Gutzmann's—a fine result on a different language, and half a century later. Miller and Nicely applied Shannon's (1948) new information theory to compute the information transmitted from speaker (or rather loudspeaker) to hearer. That was certainly a major new feature, to which we will return in Chapter 15.

The Bell Laboratories had been leading the way in the acoustics of speech since the 1920s. After World War II they pioneered the modern acoustic study of vowel perception and production. Potter and Steinberg (1950) had speakers pronounce ten English vowels in the context of *h—d*, i.e., as *heed, hid, head, had, hod, hawed, hood, who'd, hud* and *heard*. They measured the distributions over speakers of the first and second formant of these vowels. Peterson and Barney, in their seminal 1952 study, did the same for 76 different speakers, men and women, but now added perception tests. Seventy adult subjects listened to the recorded syllables and classified each of them as one of the ten listed words. Hardly any of the test stimuli were unanimously classified by the listeners, but this varied substantially between vowels. The *i* and *u* sounds caused very little disagreement, but unanimity in the identification of the speakers' *l* vowels was minimal. Figure 12.1 shows the measured distributions of the vowels in the formant space (left) and the points of best agreement among listeners in the identification of the ten vowels (right).

Vowel perception was also the topic of Pickett (1957), who studied, in a Cambridge US defense laboratory, the identification of 12 vowels presented in noise. The noise could be in four different frequency bands, from low to high frequency. In this way, he could specifically knock out the vowels' first or second formant. This led predictably to quite specific vowel confusions; the confusions "preserve" the unmasked formant, confirming the formant theory of vowel perception.

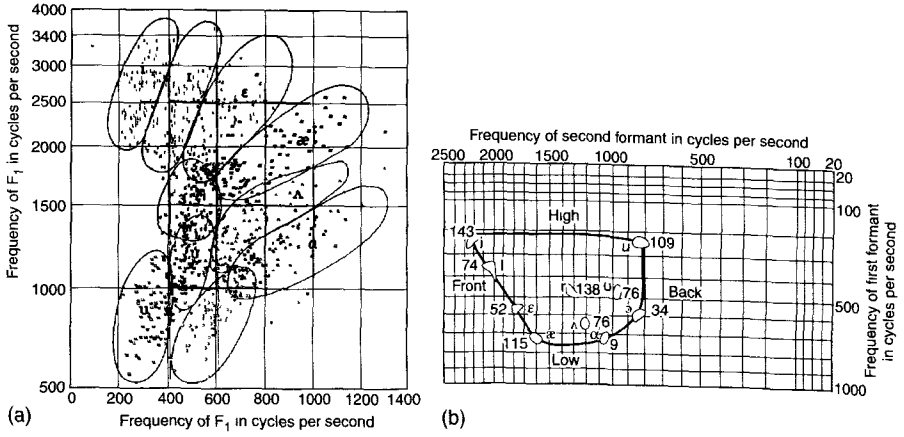


Fig. 12.1 (a) Distribution of 10 vowels in the speech of 76 speakers in the space of first and second formant. (b) The "vowel loop" in the formant space, with numbers of sounds unanimously classified by listeners; each sound was presented 152 times. Reprinted with permission from Peterson, G.E. & Barney, H.L., Control methods in a study of the vowels. *The Journal of the Acoustical Society of America, 24*, 175–184. © 1952, Acoustical Society of America.

The major breakthrough in the study of vowel perception, however, came from the Haskins Laboratories in New York. It was based on using the newly invented pattern playback device, which we will consider in Chapter 15 (Fig. 15.2). This device made it possible to "paint" formants in arbitrary frequency bands and then play them for subjects to judge their vowel quality. Delattre *et al.* (1952) used the device to determine the optimal formant combinations for all English vowels. Figure 12.2 summarizes their findings.

In a further test, the authors determined the best approximations to several vowels by using a single formant. Although these never sounded as good as the two-formant ones,

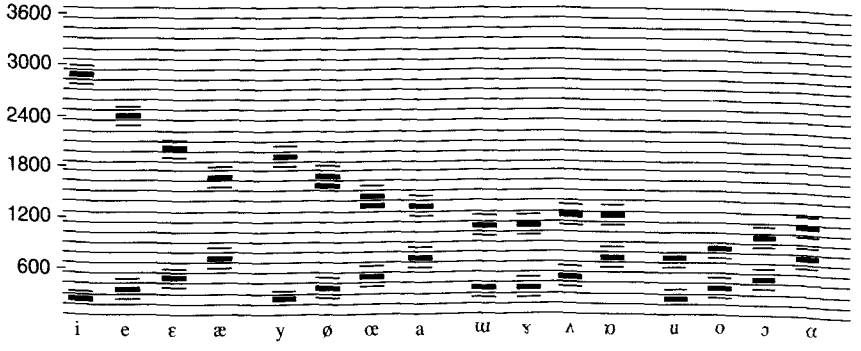


Fig. 12.2 Synthetic approximations to the cardinal vowels of the IPA, using two-vowel combinations. Reproduced from Delattre, P.C., Liberman, A.M., Cooper, F.S., & Gerstman, L.J., An experimental study of the acoustic determinants of vowel color; observations on one- and two-formant vowels synthesized from spectrographic patterns, *Word, 8*, pp. 195–210. © The International Linguistic Association, 1952, with permission.

optimal formant positions could be determined for the seven "cardinal" vowels: *u* (240 cps), *o* (480 cps), ɔ (720 cps), *a* (1200 cps), *ɛ* (2160 cps), *e* (2520 cps), and *i* (3000 cps). Notice that the values for *o*, *a*, and *e* are just about the same as Köhler had obtained in 1910 using tuning forks.

Harvey Fletcher's approach to intelligibility

We will not follow the developments in auditory phonetics, but one classic text must be mentioned, Harvey Fletcher's (1929) *Speech and hearing*. Fletcher (1884–1981) was a physicist. Together with Robert Millikan in Chicago, he measured the charge of the electron, for which the former received the Nobel Prize. Later in life, in 1916, he became director of all physical research at the Bell System, which became the Bell Telephone Laboratories in 1925. There he contributed significantly to the physics and phonetics of speech, starting with a paper in 1920. In 1929 Fletcher became the first president of the American Acoustical Society. In *Speech and hearing* Fletcher introduced the notion of "articulation" (later termed "articulation index"), which simply means the percentage of correct oral responses to test stimuli. For instance, Fletcher constructed lists of 100 test syllables. Such a list would contain 20 CV or VC syllables, such as *wi*, *ha*, *on*, or *ez*, and 80 CVC syllables, such as *jod*, *kev*, or *lan*. The syllables were spoken at a rate of one per second and recorded. The experimental subject would then listen to the list and immediately repeat each syllable. If 90 of the syllables were correctly repeated, the articulation index would be 90%. This was essentially the measure Gutzmann had introduced. Fletcher used the index to test the effect of signal distortions, such as cutting off higher frequencies. He found, for instance, that very little was lost when frequencies above 2500 cps were filtered out. But even if the recognition of syllables drops, running speech can remain intelligible. Fletcher tested this out by constructing "intelligibility lists." These were lists of 50 simple test sentences to be answered, such as *Name a state which has no sea coast* or *What candy is black and good for colds?* The "intelligibility index" was the percentage of sentences correctly answered by the subject. As Fletcher put it, a sentence contained, on average, some four "thought words." These words must at least be understood in order to provide a correct answer. How does this sentence intelligibility relate to the articulation index? Fletcher tested this by varying the distortion of the speech signal over a very wide range,[4] measuring both the articulation index and the intelligibility index for each distortion condition. Figure 12.3 displays Fletcher's results.

The intelligibility curve shows that if as much as 70% of the syllables are missing, 85% of the sentences are still correctly answered. In other words, there is enormous redundancy in the speech signal. This issue of redundancy would become a core topic in speech perception research shortly after World War II, when Claude Shannon, also at Bell Laboratories, published his seminal paper *A mathematical theory of communication* (1948), which is discussed in Chapter 15. With less redundancy, the listener will also be slower in capturing a sentence. This is, according to Fletcher, a loss of "efficiency." This is

[4] But he does not mention *how* he distorted the speech signal in this experiment.

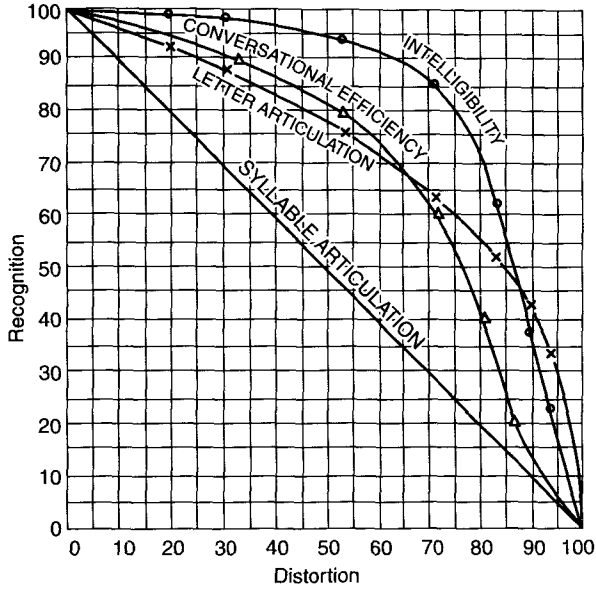


Fig. 12.3 Sentence intelligibility as a function of syllable recognition. The horizontal axis is "distortion," reverse of the syllable articulation index, ranging from 100 to 0%. The vertical axis, "recognition," stands for three quantities. The first one is the sentence intelligibility index, displayed in the curve "intelligibility." The other two are "conversational efficiency" and "letter articulation"—see text. Figure 124 from Fletcher (1929).

displayed as "conversational efficiency" in the figure.[5] The curve of letter articulation displays the percentage of speech sounds (in syllables) correctly recognized. Notice that Fletcher was not interested in a featural analysis of misperceptions, as introduced by Wells and much refined by Miller and Nicely. Both Fletcher and Miller and Nicely ignored issues of response bias.

Perceiving words: noise and number of alternatives

Karl Bühler reviewed the comprehension of words in his 1909a paper on language understanding. For him the issue is: what do we experience when we hear or read a word? The obvious way to find out is to ask a subject to introspect upon hearing a word. One drawback is that in most cases subjects experience nothing, or at least that is what they say. The first to try this out was Ribot (1891), who, like Charcot, was very much interested in individual differences, experiential "types." These might reveal themselves in the introspective reports, but mostly Ribot's subjects experienced "nothing." The way to handle this problem, according to Bühler, is to stretch the experience, by giving the subject some loading assignment, like a problem to be solved. That is what the Würzburgers Messer, Watt, and Bühler himself had set out to do. We have already discussed what they

[5] The measure of "efficiency" is casual and not really specified.

discovered: often a word would not provoke an image or a feeling, but rather an "imageless" experience. The last major paper in this tradition is by Johannes Schaefer (1928). A handwritten word, for instance *chair*, would appear in a window of a tachistoscope while the subject was pressing a button. That finger should be lifted as soon as the subject understood the word. At that moment a second word, for instance *back*, would appear in another window and the subject had to report on what had been experienced. Here is such a protocol: "Saw chair, particular chair in front of me. Rounded back, quite particular form. Second word . . . somewhat reluctant: Back was there already. Hence nothing new . . . It was like a useless reduplication."[6] We will not follow Schaefer in his phenomenology of hundreds of protocols. In conclusion he reports that in 15% of cases a full, concrete image appears in consciousness. In another 50% some concrete feature of the referent is imagined, whereas in 18% of cases consciousness of the word meaning is imageless. The most conspicuous feature of this paper is, however, that this line of research had reached its dead end.

Howells and Schoolland (1934) studied the perception of words as a potential diagnostic of auditory functioning. Their reasoning was that there are people with good auditory acuity who still have difficulty understanding words in noisy environments. Acuity cannot be the full story. The authors recorded 124 familiar words, one, two, or three syllables long, on a phonograph. Each word was mixed with "noise," which could be "shaking objects in wood boxes," "crumpling paper," etc. The list was played to 110 normal-hearing subjects, who wrote down what they had heard. The test turned out to be highly reliable. The same subjects were tested with two different auditory acuity tests. Surprisingly, the word recognition scores showed hardly any correlation with the acuity tests, nor with any other visual or auditory perceptual test. The authors concluded that there must exist "a non-sensory, auditory function which has much to do with ability to understand conversation." (p. 344). They do not provide any conjecture as to what that function might be.

The first to systematically vary the effect of noise on word perception was George Miller (1947).[7] Like Howells and Schoolland, Miller used a list of recorded test words. They were "difficult" words, Miller reports, but we are left in the dark about which words, or even how many.[8] For him, the only relevant variable was the type of noise added. Miller used pure tones at different pitch and intensity levels, narrow noise bands varying in mid frequency and intensity, continuous versus interrupted masking noises and so on. The most effective masker of speech turned out to be speech. Not one talking voice, but three or four simultaneous voices. A single talking voice has too many soft or silent moments to be a continuous masker. A careful meta-analysis of this rich set of experimental data demonstrated that the effectiveness of speech masking "depends primarily on the speech-to-noise

[6] "Stuhl-Lehne": "Sah Stuhl, bestimmten Stuhl vor mir. Runde Lehne, ganz bestimmte Form. Zweites Wort . . . . . etwas unlustbetont. Lehne war ja schon da. Also nichts Neues . . .." (Schaefer 1928, p. 135).

[7] This, Miller told me, was based on his defense research during World War II.

[8] There is a reference to Egan (1944), who developed such test lists. Miller *et al.* (1951) refer to Egan (1948).

ratio over the range of frequencies involved in speech." In other words, the closer the intensity and frequency distribution of the masking noise are to that of the speech to be masked, the more effective the masking will be. A solid, but not surprising finding.

Miller and Licklider (1950) used the same or similar word lists to test the effect of speech interruption. A word's speech signal was interrupted (by silence) at different rates, varying from less than once per second to about 5000 times per second. In the main condition the on and off times were 50/50. If an interruption came once per second, the whole word would disappear if it was "hit" by the interruption; without such a "hit," it would be normally perceived. On average, therefore, 50% of the words would be recognized. But at an interruption rate of 5000 cps nothing much would happen; all speech relevant frequencies are transmitted by the system. Indeed, the recognition curves slope from 50% to 100% recognition (with some local minima that we will not discuss). In addition to the 50/50 condition, the authors also present curves for speech-to-total-time fractions of 6.25%, 12.5%, 25%, and 75%. In all cases, recognition is almost perfect at the highest interruption rates. The authors discuss the possible application of simultaneously transmitting multiple speech messages over the same telephone channel. At high interruption frequencies, listeners simply will not notice it.

A final use of the word recognition procedure reflects Miller's adoptation of Shannon's information theory. It is a pioneering step towards Miller's "new" psycholinguistics, although its philosophy is still entirely behavioristic. Miller, Heise, and Lichten (1951) set out to apply information theory to the issue of word recognition: "It seems reasonable, therefore, to call a speech unit intelligible when *it is possible for an average listener with normal hearing to distinguish it from a set of alternative units.*" This would hold as much for "phonemes, syllables, words, phrases, sentences." (p. 331). This is, in fact, a new way of avoiding issues of meaning: "The critical part of this definition concerns the set of alternative speech units from which the particular unit is selected. This part of the definition reduces intelligibility to discriminability, and avoids the questions of semantic rules and meaning." Since discriminability is a function of the number of alternatives, the obvious experimental test is to vary the number of words that are alternatives to the target word. The subject was given a list of monosyllabic words to choose from when listening to the test item. The list could contain 2, 4, 8, 16, 32, or 256 words. The subject studied the list before the onset of testing. In a further condition the number of alternatives was not specified. In addition, different levels of noise were added to the tests words. Figure 12.4 presents the data from this landmark experiment. In a further experiment, the subjects had to recognize sentences that were five words long. This was compared to recognizing the sentences' "key words" in isolation. The sentence context reduces the number of alternatives for each of its words, as in "Apples grow on —," although it is unknown by how much. And indeed, word recognition was better in sentence context.

Sumby and Pollack (1954) essentially repeated that experiment, with by and large the same results. But they added an interesting condition: the listener could also *see* the speaking face. That added to the intelligibility, particularly at high noise levels. Bruner, Miller, and Zimmerman (1955) also repeated the experiment, with similar results, but now added another task: the subject was asked to recall the word list he heard. The recall

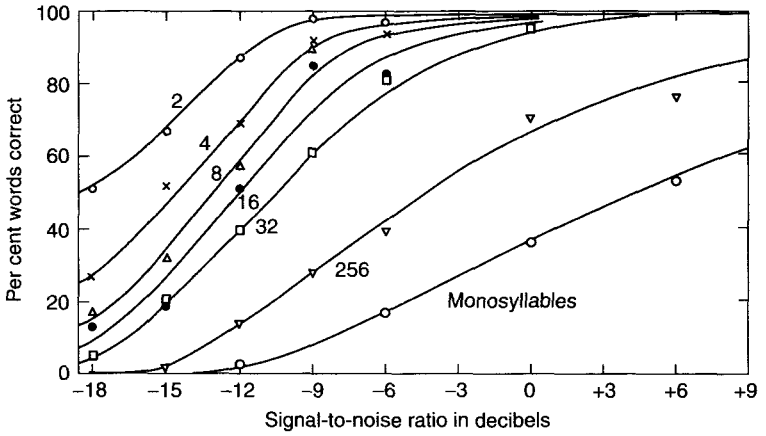


Fig. 12.4 Intelligibility of monosyllabic words as a function of the number of alternatives in the test vocabulary and noise level. Reproduced from Miller, G.A., Heise, G.A., & Lichten, W. The intelligibility of speech as a function of the context of the test materials. *Journal of Experimental Psychology, 41*, 167–173. © 1951, The American Psychological Association.

curves looked like the recognition curves. Both showed better performance on shorter lists than on longer lists. And both showed steady improvement on repeated tests. The authors argue that improved recognition is based on a stronger memory trace, i.e., on a better recall of the test items. Irwin Miller (1957), finally, replicated the Miller/Heise/Lichten findings for non-words, whose information value (i.e., number of alternatives) was varied.

All modern theories of spoken word recognition incorporate, in one way or another, the notion of a relevant set of alternatives. This has become an essential ingredient. What still had to be developed, though, was a conception of the *process* by which the set of alternatives is reduced to one or a very small number of alternatives. There was no inkling of that in the work by Miller or any other mid-century author. Exner's (1894) insight of an on-line step-by-step reduction of the alternatives set had been irrevocably lost. Also lost, and more surprisingly, was Bagley's (1900) pioneering experimental approach to "on-line" spoken word recognition. He had already demonstrated the decreasing contribution of word-initial, -middle and -final consonants to word recognition. He had also, more systematically than Miller *et al.*, studied the effect of sentence context on word recognition, demonstrating an increasing context effect from sentence-beginning to mid-sentence to end-of-sentence word position. The seminal paper had been published in the *American Journal of Psychology* and Miller had referred to it in his *Language and communication*, two years after Bagley's death. Still Bagley's concern for the process of word recognition in context was lost on the new-style psycholinguists.

Skinner's "verbal summator" and response bias

Response bias was the topic of the one psycholinguistic experiment Skinner ever performed (in 1935). He had subjects repeatedly listen to a very soft stimulus, consisting of

a sequence of stressed vowels and (unstressed) schwa's (ə). Examples of such stimuli were *ə-ə-a-ə*, *ə-ə-i-e-ə*, *a-ə-ə*, etcetera. The subject was instructed to listen to these "speech samples" on phonograph and report what was said as soon as it "pops into your head." There were 684 such strings, varying in length and vowel composition. The vague stimulus would elicit a set of latent verbal responses in the listener. These would summate in repetition. Skinner therefore called his procedure the "verbal summator." On average some 10 repetitions of the stimulus were needed for the subject to produce an answer. One aspect of the stimuli was reasonably reflected in the responses: the number of "syllables"—there was only a slight "underestimation." Would the response biases reflect frequency distributions in normal language use? Indeed they do, Skinner argued. For instance, the responses fitted Zipf's rank order distribution, to be discussed, and the word frequency distribution; high-frequency words were more likely responses than low-frequency words. There were also context effects. Subjects sometimes produced what Garrett (1980, p. 209) would call "environmental contaminants," names of objects in the perceptual environment. Also, a response, once used, had an increased probability of being used again; a perseveration tendency. Skinner even recommended the verbal summator as a personality test, just like the ink-blot test. This was indeed tried out, as we will discuss.

Speech errors

Moving now to the issue of sound and word *production*, the first thing to consider is: what was the sequel to the ground-breaking work on speech errors by Meringer and Mayer? The sad answer is: hardly any. There were the occasional remarks by descriptive linguists, not adding much at all, and there were the eternal citations of Freud's deep insights. We saw in previous chapters that the only source on speech errors referred to by Arnold Pick, by Henry Head, and by Fred Skinner was Wells' doctoral dissertation. However, Wells' section on production errors was largely based on data from Meringer and Mayer (1895) and from Bowden (1900). He only added some of his own observations on handwriting errors. His main theoretical point was that errors of anticipation (also called "regressive assimilation"), such as *Griebes* for target *Liebesgram* ("pains of love"), are almost always interrupted or partially corrected metatheses. In the example, the speaker interrupted *Griebeslam*, according to Wells. More interesting is Wells' discovery of feature errors, a rare category. One of his own observations here was somebody singing *Da schlug die Ühr halb vier* ("The clock struck half four") instead of . . . *Uhr* . . . Here, according to Wells, the *u* is preserved, except for its tongue position feature, which anticipated the tongue position of *i* in *vier*. Meringer (1908) reported such feature slips two years later. Wells also reported this for handwriting, such as his own slip of the pen *voicelles* for "voiceless." Here the abstract feature of doubling "landed" on the wrong letter. This phenomenon, now in typing, was rediscovered six decades later by MacNeillage (1964).

With these monographs, a highly promising line of research prematurely came to an end. It would be over four decades before it was picked up again, by namesake Rulon Wells (1951). The first half of his six-page paper was wasted on a eulogy of Sigmund Freud and his *Psychopathology of everyday life*. "He spectacularly showed how slips of the

tongue or pen . . . might reveal hidden intentions on the part of the person committing them." (p. 82). But after this uncritical start, Wells proceeded to formulate three phonological "laws" of blending. The first one says that blends are "phonetically possible." If *past* and *by* blend you can get *py*, or *bast*, but not *bst* or *bt*. The second law says that if two words are rhythmically similar, their blend resembles them both. For instance, *behavior* and *deportment* will blend into *behortment*. The third law says that if the original words contain the same sound in the same position, their blend will contain that sound in that position. A blend of *frown* and *scowl*, for instance, will become something like *frowl* or *scown*. The arbitrariness of these "laws" (why not present ten other such laws?) and the absence of any reference to Meringer, Bawden, or Frederic Wells once more demonstrated the fatal disappearance of a rich research tradition. Another 15 years were to pass before Anthony Cohen (1966) restarted serious, data-based research on errors of speech, soon followed by Boomer and Laver, Hockett, Fromkin, Fry, MacKay, Nooteboom, and others.

Articulation

The situation was quite different for the study of speech articulation. The tradition established by Kussmaul (1822–1902) in Strasbourg, Rousellot (1843–1924) in Paris, Henry Sweet (1845–1912) in Oxford, and Eduard Sievers (1850–1932) in Leipzig was continued uninterrupted by leaders such as Hermann Gutzmann (1865–1922) in Berlin, Edward Wheeler Scripture (1864–1945) in New York, Vienna, and London (in this order), Raymond Stetson (1872–1950) in Oberlin, Ohio, Daniel Jones (1881–1967) in London, and Eberhard Zwirner (1899–1984) in Berlin and Münster. Elise Richter's small (1912) monograph *How we speak* nicely sketches the process of speaking from this phonetic perspective, extending into issues of grammatical encoding.[9]

Sarah Barrows (1916) reports the existence of some 25 phonetic laboratories. Articulatory phonetics was their main business. Sophisticated machinery was developed to record speech and movements of the articulation musculature, including X-rays of speakers in various languages (Russell 1931).

Phonetic laboratories offered their services to speech therapists, language teachers,[10] acoustic designers, and engineers, and, yes, to the military. Experimental phonetics developed as an independent science and it is beyond the aims of this book to handle its history. Still, there is much common ground between psycholinguistics and experimental phonetics. We witnessed a happy convergence of these research traditions after World War II, to which we will return in our last chapter.

[9] Elise Richter (1865–1943) was the first woman university professor in Austria, leading the Viennese Phonetic Institute. She was murdered in Theresienstadt in 1943.

[10] And applied texts on speech for teacher training institutions and speech therapy continued to appear over the century, such as Villiger (1911) on children's speech disorders and Stinchfield (1928) on speech training.

Delayed speech

A fine case of convergence between phonetics and psycholinguistics was Bernard Lee's (1950a,b, 1951) discovery of the "delayed auditory feedback effect" (DAF). Lee was a staff member of the Signal Corps Engineering laboratories at Fort Monmouth, where he discovered by chance that when you hear your own speech somewhat delayed, speech fluency breaks down. The effect is maximal at a delay of about 150 milliseconds. Under such conditions, Lee reports, speakers start saying *alumininum-num* or count backwards, *ten-nine-nine-eight-seven.* Lee immediately saw the potential relevance of this finding for theories of self-monitoring in speech. He proposed the "speech model" depicted in Figure 12.5a.

The model contains four loops that operate in parallel. They exchange information at the common junction only. The smallest loops drive the articulation of the individual phonemes. Their output (at passing the junction) is monitored by kinesthetic feedback. If passed, they proceed into the next voice or syllable loop. Syllables reaching the junction are auditorily monitored, still at a reflex level. If passed, they proceed to the next larger loop, becoming constituents of words. The monitoring of words is volitional and so is the monitoring of the "thoughts" in the outer loop. Delayed auditory feedback interferes with this process, especially with the reflex-type auditory monitoring in the second "voice and syllable" loop.

It is remarkable how obvious it was for an acoustic engineer to propose such a mental processing model, unhampered by behavioristic dogma. Others began measuring the critical parameters of DAF. Figure 12.5b shows the basic measurements by Butler and Galloway (1957), showing the effects of amount of speech delay and feedback loudness.

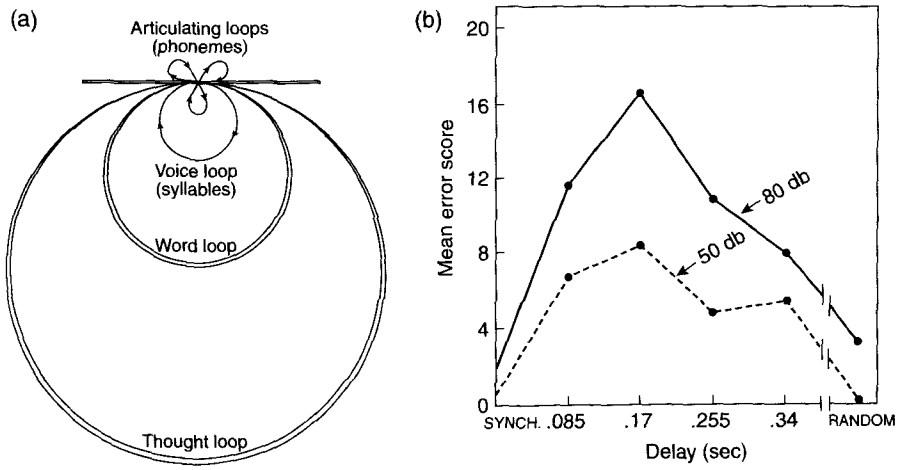


Fig. 12.5 (a) Feedback loops in the speaker's self-monitoring. Reprinted with permission from Lee, B.S., Effects of delayed speech feedback. *Journal of the Acoustical Society of America, 22,* 824–826. © 1951, Acoustical Society of America. (b) Error scores in reading numbers at different levels of delay and intensity in speech feedback. Reprinted with permission from Butler, R.A. & Galloway, F.T., Factor analysis of the delayed speech feedback phenomena. *Journal of the Acoustical Society of America, 29,* 632–635. © 1957, Acoustical Society of America.

They found 170 milliseconds to be the optimal delay, which is about the average duration of a syllable. Soon DAF was applied in the treatment of stuttering, dysphonia, the speech of schizophrenics, depressives, Parkinson patients, you name it.

Studies of speech perception and production showed little continuity from the promising beginnings of the late 1800s and early 1900s. Especially remarkable is the almost total disappearance of the good old chronometric approach, which had been so successfully introduced by Cattell and others, and the virtual disappearance of speech error research. This was certainly no accident. What got lost after World War I was a theoretical perspective on mental *processes*. On the American scene in particular, the tunnel vision of behaviorism did not allow for more than a bare minimum of factors mediating between stimulus and response. In addition, much of the work was applied and non-theoretic in nature: how to measure, improve, or destroy speech intelligibility. Only by the end of the 1950s did the tide begin to turn, as we will discuss in Chapter 15.

Meaning

The study of meaning was a frightening challenge for psycholinguists. Language use is all about meaning. Its study simply cannot be avoided. But how to grasp meaning empirically? The whole notion is elusive, ill-defined. In the behavioristic world, moreover, meaning is a mentalistic concept. It can only be studied by avoiding it, by redefining it in operational, objective terms. In this section we will review a variety of empirical approaches to meaning that were developed during the decades under review, from studies of word association to scaling studies, content analysis, and studies of phonetic symbolism and metaphor. We will conclude with a brief assessment of these studies.

Associations

Chapter 5 reported on the boom of association studies following Galton's 1879 publication in *Brain*. Remember Cattell's inventive restrictive association paradigms, Thumb and Marbe's study of analogical change and Marbe's law, and Aschaffenburg's applications to clinical settings. This stream was never interrupted, flowing on into the "cognitive revolution." Cattell and Bryant (1889), in particular, had published association norms for 10 concrete and 10 abstract words. Robert Sommer, whom we met in Chapter 5, developed a standardized list of 60 words for clinical testing as part of his habilitation thesis (submitted in Würzburg). This was picked up by Kent and Rosanoff (1910), who expanded Sommer's list to 100 and collected associations to these words from over 1000 participants without disabilities, among them 200 "highly practiced observers," 500 nurses and other hospital personnel, 150 high school students, and over 50 clerks and farmers. Their 100,000 responses, excluding the responses that were given just once to a stimulus word,[11] were listed in 100 tables. They are now known as the Kent/Rosanoff tables of free association or the Kent–Rosanoff word association test. The first word in the list is *table*, with "chair" as most common associate (27%). The last word is *afraid*, with "fear" as most frequent response (20%). Woodrow and Lowell (1916) exactly replicated

[11] The so-called *hapax legomena*.

Kent and Rosanoff's study on 1000 school children aged 9–12. O'Connor (1928) faithfully replicated Kent and Rosanoff's study on 1000 men from industry.

There was a boom in clinical applications of the association method. Like Sommer, Kent and Rosanoff and an army of American followers applied their standard list in clinical settings. In Europe Swiss psychiatrist Bleuler (1857–1939) had independently been using word association tests since 1901. He asked his assistants Jung and Riklin to perform a standardization on participants without disabilities. They did and published (1906) association norms for 400 stimulus words, based on 38 subjects. However, the tables did not display the subjects' responses, as in the Kent and Rosanoff tables. Rather, they summarized *types* of responses for different categories of subjects (educated/uneducated men/women). Among the types were "predicative," "word completion," "alliteration" etc. Carl Gustav Jung (1875–1961) then took the lead in developing further clinical and forensic applications of the word association method. Jung (1906a) introduced the word association test, combined with the measurement of reaction times, as a lie detector, getting into a priority conflict with Max Wertheimer, who had published (together with J. Klein) a similar test in 1904. Jung (1907, Peterson and Jung 1907) also combined the word association test with measurements of the patient's psychogalvanic skin response. Jung's later assistant Ludwig Binswanger (1910) standardized that response on participants without disabilities. But Jung's ultimate interest was in unraveling the affective "complexes" of compulsive and hysterical patients (Jung 1906b). He had them respond to lists of stimulus words, but these could vary from patient to patient. He then analyzed the response latencies and response "complexes" as a diagnostic. Hysterical patients, for instance, often responded very slowly, as it were overdoing the case: "the test person has a tendency constantly to give to others more feeling than is required and expected. According to Freud, "this is a sign of a reinforced object-libido, that is, it is a compensation for an inner unsatisfaction and voidness of feeling." (Jung 1910, p. 228). It should come as no surprise that Freud quickly adopted Jung after the latter had sent him his 1906 studies on word association.

Mildred West Loring (1918) reviewed over 100 word association studies, far too many to be discussed here. But a few examples will suffice to give a taste of this new wave in experimental psychology. Atherton and Washburn (1912) introduced a type of restricted association that even Cattell had not thought up. They cited an association from Aschaffenburg (1896), where the stimulus word was *Allmacht* ("omnipotence"), triggering the response "Mater." Here the "mediation" had apparently been *alma mater*. But had this been a *conscious* mediator? In order to maximize the probability of unconscious mediation, the "observer was given the instruction that on hearing the stimulus word pronounced she was to respond by giving a word that had no association with it." (p. 102). In good Würzburgian tradition, the subject then provided an introspective report. Subjects developed various strategies to cope with the task. For instance, some "took a walk" in the surroundings and mentioned a landmark or object they mentally encountered there. Still, in almost half the cases the subject could not explain her response. Some unconscious mediation must have taken place, or in the authors' words: the "link was physiological rather than psychological" (p. 109).

The Würzburgian influence was also apparent in an exchange of papers between the schools of Titchener and Külpe. The exchange was, as it were, a reenactment of the Wundt-Bühler exchange. Titchener had proposed his "contextual theory of meaning," which says that "meaning, psychologically, is always context" and "Context, in this sense, is simply the mental process which accrues to the given process through the situation in which the organism finds itself." The meaning of a word is not the sensation when hearing or reading the word, but whatever accrues to the sensation. The start is bodily: "originally, meaning is kinaesthesis; the organism faces the situation by some bodily attitude." The sensation arouses an image: "image has now supervened upon sensation, and meaning can be carried in imaginal terms" (Titchener 1910, p. 367). As far as conscious content is concerned, Titchener is still a Wundtian: it is either sensation, image, or feeling. Meaning is whatever arises in consciousness in the process of accrual following sensation and that depends on the subject's context of experience. But the Würzburgians knew better: much of a word's meaning resides in imageless content of consciousness. They were going to respond to work by Jacobson and Clarke of Titchener's laboratory at Cornell.

Edmund Jacobson (1911) and Helen Clarke (1911) had subjects introspectively respond not only to words, but also to letters and (in Jacobson's paper) to whole sentences. The conclusion, in Clarke's formulation (p. 249), was "that conscious attitudes can be analyzed into sensations and images and feelings, or traced genetically to such analyzable complexes, and therefore do not warrant the proposal of an additional conscious element." [meant was some imageless thought]. We will not follow the authors' extensive introspective reports in detail, but consider one prototypical introspective response in Jacobson's paper. It was one of many in which, initially "the meaning did not come to the subject at all in spite of a wealth of visual, organic, kinaesthetic and tactual sensations."[12] The written stimulus sentence was "Did you see him kill the man?" The (trained) observer's introspective response was a longish enumeration of vague visual, auditory, and kineaesthetic sensations, including articulatory ones. But "no meaning all the way through." It is probably possible indeed to entirely ignore the meaning of a sentence during reading and to concentrate exclusively on all the sensory impressions of the moment, delaying the comprehension process, but the Würzburgers' response was different: Of course the sentence's meaning had been there in consciousness right away, but it was imageless. That was the counter claim by Thomas Moore (1915), from Külpe's laboratory in Munich. It is not the case that a word or sentence first triggers a host of sensations, which, in turn, release a host of images that constitute its meaning. "Imaginal terms may accrue to incoming sensations and constitute by definition their context. Do they constitute their meaning? A determination of the temporal relation that imagery bears to meaning shows that this is impossible." Imageless thoughts, constituting the item's meaning, *precede* those images. "It is a definite mental process *sui generis*." (p. 225). This challenge was, finally, taken up by Weld (1917) of Titchener's laboratory. He used 29 drawings of unfamiliar technical or ethnological objects, with totally unfamiliar names (such as *brob*

[12] As Moore (1915, p. 223) put it.

and *mino*). His three subjects learned these object–word pairs and were then tested on the words. They were, for each word, instructed to react either to the image of the object or to the meaning of the word. Weld concludes from his transcript protocols that under the "meaning" instruction, his subjects are happy as soon as they can make the reference. For them the word's meaning is its referent. The response only requires consciousness of the "image-of-object." But under the "image" instruction, the subjects respond to the "image-as-image," its presence and its properties. No surprise, Weld concludes, that the meaning (i.e., consciousness of the referent) can be there before the further elaboration of the object image.

There are moments at which one can sympathize with the early behaviorists' ridicule of introspection. One of the most conspicuous features of the extensive introspectionist transcript protocols is that they faithfully reflect the local theory. The subjects were, after all, trained observers, "university professors," their assistants, and doctoral students. Reports on imageless consciousness were the trade mark of Würzburg protocols, on "kinaesthetic feel" for observers at Cornell, the "whole" or Gestalt in Budapest,[13] and so on.

Erwin Esper's approach, we saw in Chapter 8, was different. No introspections, only "objective" data. Esper (1918) replicated Thumb and Marbe's (1901) word association experiment with English materials, Leonard Bloomfield helping him on the translations. He used 126 subjects, many more than in Thumb and Marbe's study, students, children, and janitors, all "untrained." In almost all respects Esper's results confirmed Thumb and Marbe's. In particular, Marbe's "law" was confirmed: the more frequent the response word, the shorter the response latency. Also, the response word was predominantly from the same word class, nouns releasing noun responses, adjectives adjective responses, etc. A fine study, but without any additional hypothesis or insight. That is different for Esper (1925, 1933). These are among the first experiments in artificial language learning.[14] The study is again motivated by Esper's interest in language change. What determines the stability or instability of a linguistic designation? It must relate, behaviorist Esper argues, to the stability of the stimulus-response unit, the relation between environmental stimulus and verbal response. Thumb's claim is in the background: analogical language change follows word association.

To study this, Esper created an artificial stimulus response world.[15] The 16 stimuli were based on four tangram figures (Fig. 12.6).

The figures were selected from a tangram naming experiment; figures were chosen that induced the least number of common responses. Each figure came in four sizes, with different artificial names. There were 12 names in total, because one size of each figure was unnamed. In the experiment the subjects first learned the relevant 12 figures and their

[13] In Hermann-Cziner (1923, p. 92) one reads, for instance: "Nach dem Lesen war wieder ein Zustand des Verstehens vorhanden, welcher eigentlich die Einheit von drei Dingen in sich enthält: des Wortes, des Ich und der Bedeutung; es besteht gar keine Trennung."

[14] The first one was Fischer (1922), to whom Esper does not refer.

[15] We will follow Esper (1933) here.

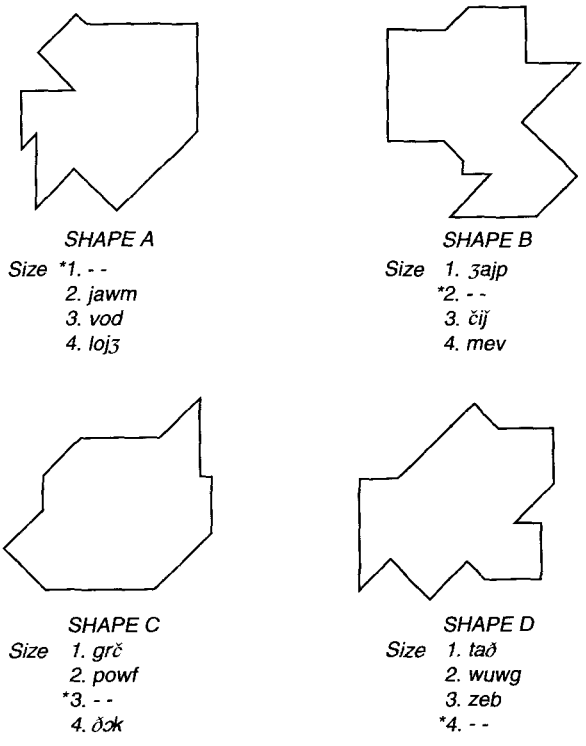


Fig. 12.6 Esper's tangram figures and their nonsense names for different sizes. One size of each figure was not given a name. Reproduced from Esper, E.A., Studies in linguistic behavior. I. Characteristics of unstable verbal reactions. *Journal of General Psychology, 8,* 346–381. © 1933, The American Psychological Association.

(only spoken) names. That was the new, artificial world. Thumb and Marbe argued that language change would follow the path of associations. Esper now asked: what are the determinants of association? The subjects were instructed to give an immediate association to each of the 12 "names." They were mixed with 12 real words. The majority of subjects' responses consisted of other learned "names," in a fairly regular fashion. The most frequent associate was the name of the next bigger or smaller *same* shape (like *vod* → *jawm*). This is classical "stimulus generalization." In other words, language change is not caused by following the path of word associations as Thumb and Marbe had argued, but by the similarity of the stimulus conditions that control the verbal responses. In other words, associations are not the *cause* of linguistic change, rather "certain stimulus situations have had a strong tendency to elicit simultaneously or successively the two responses." (p. 377). It is these variation-causing stimulus conditions which drive linguistic change.

The other question Esper's experiment was intended to answer was "How is a new object to be named"?[16] To test this, Esper presented his subjects with the four unnamed stimuli (see Fig. 12.6), together with all the others. Again, results were unequivocal: the preferred response was the name of the next larger or smaller stimulus of the same shape, i.e., by and large the same response as in the association task. "Such an introduction of a

[16] This would become Brown's (1958a) issue, to which we return in Chapter 15.

new object temporarily increases the instability of the entire system, but there is a tendency to a new equilibrium," Esper concludes (p. 379).

Wolfle's (1932) study is based on his 1928 master thesis with Weiss at Ohio State, who had introduced him to Esper at Washington University. Wolfle's experiment is very much a replication of Esper's, even using exactly the same stimulus figures (Fig. 12.6). But the names were different. In a first *regular* condition, the "stem" of the name denoted the shape and the "suffix" the size. So, for instance, *lud* and *pof* were names for shape A and shape D in Fig. 12.6, whereas the suffixes *nas* and *vus* denoted the largest and smallest sizes. Hence, *ludnas* referred to largest shape A and *pofvus* to smallest shape D. In the *irregular* condition morphological deviations were introduced. For instance, *pofvus* was replaced by *vuskof* or *ludnas* by *neslud*. Not surprisingly, learning the artificial language was much slower in this latter, irregular condition. During the learning period, about half the naming errors preserved the shape, whereas about one-third preserved the size. These are the same type of "stimulus generalizations" that Esper had observed.[17] For both conditions and after perfect learning had been obtained, Wolfle also performed association tests. These immediate associations to the newly learned names revealed that in neither experiment "was there any very close agreement between the errors and the associations to a given figure." (p. 43). Under Thumb's claim that should have been the case. Wolfle did not measure response latencies; there was no test of Marbe's law.

Marbe's law, however, was reconfirmed by Schlossberg and Heinemann (1950). They performed measurements of response latency on word associations to the Kent and Rosanoff list. They concluded that "the median log reaction time was found to be an inverse linear function of communality" (p. 246). But the paper made reference to neither Marbe's law, nor Esper's paper, nor even to the studies by Karwoski *et al.* (see 'Scaling'). The records of science can be very short-lived.[18]

The idea that a word's meaning is the context that provokes it was basic to behaviorism. Howes and Osgood (1954) begin their paper on conjoint word association with: "It is a commonplace that meanings of words depend upon the contexts in which they occur." The paper considers word associations to *multiple* context words. The subject is asked to provide an association to the last word in a string of four. The stimulus string is, for instance, *skin, hour, utter,* ***rough*** and the subject responds to test word ***rough***, for instance with "hand." How is the response probability of "hand" dependent on the test word and its preceding context? One finding was that the probability increased when the context word *skin* appeared in third position, just preceding test word ***rough***. The strong association between *skin* and *hand* became more effective with less intervening "context." More generally, however, all context words contributed (positively or negatively) to the

[17] This notion of stimulus generalization regularly returned in artificial naming experiments. Lipton and Blanton (1957), for instance, used nonsense syllables to name "circular" figures and "triangular" figures. When they then combined a learned "circular" figure name with an electrical shock, the evoked GPS response generalized to the other names for "circular" figures, but not to names for "triangular" ones.

[18] Heineman taught at the University of Ohio, where Esper had obtained his PhD.

appearance of "hand" as response and these contributions were additive. In other words, the context words did not interact as meaning components of "hand." It was also found that if a context word was high-frequency it exerted a larger effect than if it was a rare word. Association studies were quite popular during the 1950s (see Rubenstein and Aborn (1960) for more detail). They also gave rise to scaling approaches to meaning.

Scaling

Cattell's original notion of restricted word association, limiting the responses to certain categories, kept returning in the literature and eventually led to scaling methods in the study of meaning. Three papers by Karwoski *et al.* (1944, 1945, 1948) from Dartmouth College marked the transition. The first paper compared word associations to words, to objects they denote, and to drawings of these objects. The main conclusion here was that the subjects' response tendencies differed somewhat for pictorial and verbal stimuli. Visual stimuli released more "dynamic" responses, words more "static" ones. The physical spoon object, for instance, triggered *eat* as the most frequent response, whereas the word "spoon" got *fork* as the dominant response. The second paper compared free and restricted word association, as Terman and Miles (1936) had done before. Each stimulus word was combined with a four-way multiple choice. Care was taken that the four response alternatives were of different "categories," with "essential similarity" (*large*–"big") and "essential opposition" (*large*–"small") as extremes. Also, all four response alternatives were common responses (over 10%) in a free association task. In contrast to Terman and Miles, the authors found major discrepancies between their free and restricted association tests. The major preference in the multiple choice test was to go for "similarity" (*large*–"big"). Participants eschewed "opposite" alternatives (*large*–"small"), which is a normal, often dominant response in free association. In other words, participants conceive of the multiple choice task as a kind of similarity judgment task, a quite different "set" to the free association task. Is it easier for participants to make a similarity judgment than a dissimilarity judgment?

The third and last paper tested this explicitly by measuring subjects' response latencies. In a free word association test participants were *fastest* when they gave a contrastive response (*large*–"small"). As a spin-off the authors explicitly confirmed Marbe's law here: the most common responses were the fastest ones and these were frequently opposites. A contrastive *judgment*, however, might be hard on the participants. To test this, participants were asked to judge a pair of words (such as "hand–mouth") as more "similar" or "dissimilar." The pairs were, of course, taken from the free association responses. Indeed, "similar" judgments turned out to be systematically faster than "different" judgments, a finding that has been replicated.[19] Is this specific to similarity judgments? What about a "good–bad" judgment scale? The authors had their subjects judge 28 different adjectives on such a choice pair. There was no latency difference between "good" and "bad" judgments.

[19] See, for instance Perea *et al.* (2011).

Explorations of this kind abounded during the 1940s. Osgood (1941) had already introduced the scaling of terms of social relevance, such as "American" or "isolation" on adjective pairs such as "strong–weak" or "valuable–worthless" and shown that responses were faster for more extreme judgments and for judgments on which most participants agreed. Mosier (1941) had almost 300 adjectives, such as *neutral, normal, excellent,* and *disgusting,* judged on a 11-point scale of favorableness by 140 subjects. This was largely an exercise in mathematical scale construction and is not relevant here. But there was one psycholinguistic extra. Some of the same adjectives were also scaled when combined with adverbial modifiers, such as *quite, very, highly,* and so on. The main effect of these "intensives" was to shift the meaning away from the neutral point of the scale to the "closest" extreme. For *quite* the shift is small, for *extremely* it is large. But the amount of shift depends on the modified adjective: *highly desirable* is much more favorable than *very desirable,* but *highly unsatisfactory* is just as (un)favorable as *very unsatisfactory.*

The major innovation in the scaling approach to meaning was Osgood's (1952) "semantic differential" (SD), discussed in Chapter 8. In fact, this was the transition to *multidimensional* scaling. A stimulus word was not judged on a single scale, but on a set of scales. Osgood's scales co-varied in at least three underlying dimensions: "evaluation," "potency," and "activity." Hofstätter (1955) introduced Osgood's technique in Germany. Jenkins *et al.* (1958) published a widely used atlas of semantic profiles for 360 English words. In Chapter 15 we will mention Lambert's applications of the semantic differential to the study of bilingualism.

Meaningfulness

The ultimate meaning scale would, of course, be one of "meaningfulness." Bingham (1943) demonstrated that words judged as "meaningful" generated relatively strong galvanic skin responses, with meaning happily captured in an "objective" measure. This idea got further extension in the work of Noble (1952, 1957). He defined his measure *m* of a word's (or non-word's) meaningfulness as the mean number of continued written associations given to the word within a minute. Noble's *m* became a widely used experimental measure. High-*m* words were easier to memorize than low-*m* ones. The average of a word pair's two *m*s predicted how easy it is to associate the two words, etc. Staats and Staats (1959) compared Noble's *m* to meaning explicitness as measured by SD, namely extremeness of responses on the judgment scales. Noble's *m* and SD explicitness are correlated, but different, was their conclusion.

Content analysis

Another, much more applied, approach to meaning is "content analysis." During the 1930s and 1940s social scientists had been analyzing political, advertising, and interviewing texts to derive underlying attitudes, key concepts, and evaluations. The dominant practice was to count the frequencies of key words in such texts. Berelson (1952) reviews much of this work. It was more of a practice than a science, lacking theoretical and methodological depth. To improve on this, the Social Science Research Council organized a

winter seminar on the topic, which took place in Monticello, Illinois in 1955. Social scientists and linguists, among them de Sola Pool, Osgood, Saporta, and Sebeok, discussed how to draw inferences from texts to their "antecedents" (speakers, writers) and to their effects (on listeners, readers). Can written texts, such as newspapers, diaries, biographical texts, folk tale texts, or spoken texts, such as clinical interviews or political speeches, be analyzed by quantitative procedures to infer the source's attitudes, interests, intentions, valuations, etc.? Osgood's paper for the seminar was an all-out attempt to provide this practice with a theoretical basis. Rather than counting frequencies of critical words in the text (which was the common practice), one should go for contingencies of critical words. The *contingencies* in experience cause an individual's "association structure." Content analysis should be after these associations in the thinking of a person. The way to approach this is to measure contingencies and co-occurrences in the relevant texts. How often do terms in the text that carry some relevant coded feature go together with terms that carry some other relevant coded feature? Osgood exemplifies the precise procedure to be applied by some of his own content analyses. One of them is an analysis of the Goebbels diaries. Osgood found, for instance, that references to German generals in the diaries were significantly contingent on references to internal frictions in Hitler's inner circle. References to the German public were associated with references to bad morale, and so on. The carefully, but rather slowly, edited proceedings of the seminar (de Sola Pool 1959) became a standard reference in the field.

Phonetic symbolism

The study of phonetic symbolism has been with us since Humboldt; it never disappeared from the psycholinguistic agenda. Still, its potential relevance as a causal mechanism in the emergence of human language was no longer a topic of discussion after the turn of the century. Wundt had apparently successfully argued that no trace of that evolution has been preserved. But sound metaphor still is a somewhat productive phenomenon according to Wundt. It is not real sound imitation, but rather phonetic reference to some characteristic aspect of meaning, such as the repeated click sound of a clock (*tic-tac*). Hermann Hilmer (1914) critically explored this in some detail in his Stanford linguistics dissertation with George Hempl. He tabulated, for German and English, sound-imitating words of different types, such as the *knack*, *knick* words, the *tappen*, *tupfen* words, the *clump*, *clamp*, *clumper* words, etc. On the bases of such lists he concluded that sound symbolism kept being a productive force in a language's new formations. They are not just dead remnants from long-lost roots, hence confirming Wundt's thesis. But in contrast to Wundt he claimed that these were often real sound imitations. Jespersen (1922) has a whole chapter on sound symbolism. He essentially sides with Wundt and Hilmer. Sound symbolism is a fairly productive process. It is not an archaic remnant of primordial language. In fact, most sound-symbolic words are of relatively recent origin. Jespersen argues that words which did not originate as sound symbolic may "stick" better if they happen to be experienced as sound symbolic. Words such as *kid* or *pin* are felt to be appropriate for small entities, although that had nothing to do with their etymology. But now we "keep" them. Similarly, sound-symbolic loan words have a better chance of "sticking"

than arbitrary ones. Jespersen mentions *jungle* (from Hindi *jangal*) and *mugwump* (from Algonkin *mugquomp* –"chief").

Can phonetic symbolism be studied in the laboratory? Three small sets of papers form the main harvest of empirical studies of phonetic symbolism before World War II.

The first round was triggered by Kollarits (1914) from Budapest. He claimed, without any experimental demonstration, that we often attach extensive visual imagery to names of known or unknown people. Claparède (1914) responded that such impressions are sometimes provoked by the sound character of the name. Mister *Patapoufard* is probably "corpulent, stocky, puffy and a bit ridiculous" (p. 302). English (1916) turned this suggestion into an experiment. Subjects described what a named, but unknown, person "would look like." There was, however, no system in the responses. Alspach (1917) from Cornell tried to improve on this by way of an introspectionist experiment. A single, highly trained subject, Professor Forster, was presented with no less than 50 nonsense proper names, such as *Spren*, *Désnep*, *Thaskúwin*, and *Koldak*, with the instruction to describe these people. Mr. *Koldak*, for instance, was described as "The man is quick tempered, vicious; jumps when spoken to. The word is like the sharp, clean-cut ticks of a metronome." The professor mentioned in 35% of the cases that his description emerged from the sound of the name. Hence, that must be true. But often, this observer also reported kinesthetic sensations on hearing the name. We are, after all, in Titchener's laboratory. A final study in this set was by Hermann Müller (1935). It was triggered by his supervisor Wittmann, who published a preliminary paper on Müller's results in 1934. Müller presented his subjects with words from various tribal languages and asked them to guess each word's meaning. So, for instance, the spoken word *fiti* was interpreted as an arrow, something sharp, some small, fast predator, a high tone, etc. Müller then related the interpretations to the word's vowel and consonantal qualities. The double *i*-sound and the sharp *t*, for instance, in *fiti* would create a "whole" (*Ganzheits*) quality of suddenness, speed, etc.

The second, more serious round was triggered by Edward Sapir (1929a). Language, Sapir argued, has its prosodic tool kit of directly expressive devices (emphasis, interrogative intonation, etc.), but in addition there may be something like phonetic symbolism. Can this be experimentally demonstrated? The simple experiment Sapir designed was this. Subjects were presented with a pair of spoken nonsense words, such as *mil* and *mal*. Both words were supposed to mean "table," but one referred to the larger variety and the other to the smaller one. The subject marked on a form whether the first word (*mil*) indicated the larger or the smaller referent. Each new non-word pair combined with another real word (as in the case of *mil-mal* "table"). Sapir took care that each of the 100 pairs contrasted in their vowel or in a consonant. A limited set of vowel contrasts was used, such as *a-i* and *u-o*. Similarly for consonant contrasts: voiced versus voiceless (such as *b-p*), stops versus spirants (such as *p-f*). It turned out that the vowel *a* (as in *art*) induced "large" answers, whereas *i* induced "small." Sapir suggested three possible explanations. It could be the larger acoustic volume of vowels such as *a* that induces the symbolism. Or the symbolism is kinesthetic, relating to the "larger" felt tongue position or resonance chamber of the vowel. Or maybe it's both. Sapir's student Newman (1931), whom we met in Chapter 1 as a participant in Carroll's 1951 summer seminar, was given the consonant

data for further analysis, but then turned Sapir's paradigm into a sophisticated scaling experiment.[20] For the "small–large" judgments a linear "largeness" scale of vowels was derived: *i-e-ɛ-a-a-u-o-ɔ*. Similarly for consonants: *p-n-d-s-h-k-b-l-g-m-dj-r-gl-gr-br*. The experiment also involved another dimension of judgment: "dark–bright," which produced different scales.

An inventive extra in Newman's study was to check whether English words with a sense of "smallness" involved more "small" consonants or vowels than words with a sense of "largeness." He selected 183 "small" words from Roget's thesaurus (such as *decline, jot, short, puppet*) and another set of 173 "large" words (such as *chief, flood, monster, space*). He then scored all their consonants and vowels on the obtained scale values. The result was disappointing in one respect: the differences were in the right direction ("large" words having "larger" vowels and consonants) but the difference was almost negligible (no statistics presented). Not disappointing was, of course, that the latter finding excludes the often suggested explanation of phonetic symbolism, namely that it occurs by association to existing meaningful words, i.e., by analogy. In other words, it confirmed Esper's and Wolfle's doubts about Thumb's hypothesis. Curiously, however, Wolfle (1934) argued in a theoretical, behavioristic paper that sound symbolism is all a matter of generalization. There are always lots of exceptions to a particular symbolism. For instance, in view of their vowels, *big* should mean "small" and *small* should mean "big." One should expect such exceptions if there is no hard and fast rule. As soon as a language happens to have more *i* vowel words meaning small things than large things and more *a* vowel words meaning large things than small things, then a process of generalization will set in. As new words are added to the language, the habitual dominant vowel meaning will generalize to them, or "conforming" new words will have the greater survival value (as Jespersen had argued). This explanation makes it unnecessary to accept Sapir's nativistic explanation, which predicts generalization over languages, Wolfle says. Later studies would confirm Sapir's universalist claim, at least for the symbolism of the *i-a* distinction.

The final paper in this run was by Bentley and Varon (1933) of Cornell University. They performed Newman-type experiments not only on the dimension of largeness, but also on the adjectives *strong, angular, liquid* and, yes, *foolish*. Nonsense words containing the vowel *a* differed from those with *i* on the dimensions "large" and "angular." The authors concluded that the symbolism must proceed from the "feel" of sound production itself: "this tactual and kinaesthetic component, variable in many directions and through wide ranges, probably serves in quite essential ways to convey its own meaning" (p. 86). We are still in Titchener's laboratory (six years after his death).

The third set of papers deals with phonetically "appropriate" names for objects, figures, and drawings. What name should be assigned to which drawing in Figure 12.7? The example is from Wolfgang Köhler's *Gestalt psychology* (1929). The issue was in the air.

[20] Applying Thurstone's (1928) statistical procedure. Newman (1905–1984) became a linguistic anthropologist, among other work publishing a grammar of Californian Yokuts language.

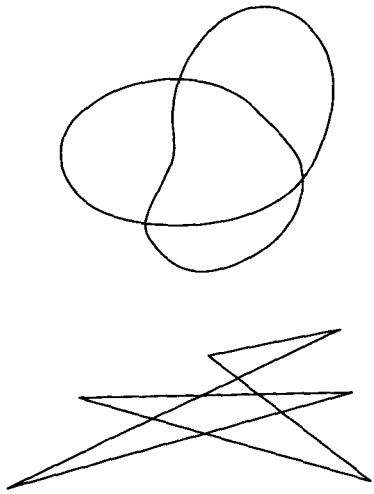


Fig. 12.7 Which of the two figures should be called "takete" and which "baluma"? Reproduced from Köhler, W., *Gestalt Psychology*, figures 18 and 19 © 1929, Horace Liveright.

Siegfried Fischer (1922) touched on the appropriateness issue in his extensive study of name assignment. Just three years before Esper, he had his subjects learn an artificial language, a set of nonsense names for a set of nonsense figures. Fischer had worked with Isserlin and Bühler in Munich, and had followed Bühler to Dresden, where he completed his monograph. Fisher taught his subjects a "secret code," consisting of nonsense drawings with nonsense names. The (spoken) names were one-, two, or three-syllabic, such as *Scheuk, Polof*, or *Haurisat*. In a further experiment the subjects saw photographic portraits of the "members of the secret society," and had to remember their (nonsense) names. In Würzburgian fashion, the subjects were asked how they had associated the name to the figure or portrait. In many cases, the subjects only remembered "associating" the two, without further consciousness. But in other cases some relation was created between name and diagram or portrait. For instance: the diagram looks like a palette, name is *Polof*; there is a *p-p* link. Or there is a mediating word: the diagram looks like curves, name is *Kursum*. But occasionally there was an "appearance" relation: the "heavy" name *Battich* fits the "self-important" face. Fischer also ran a free-naming experiment for the same figures. Here he found some preference for names with sounds such as *k, i, z* ("ts") for angular figures, as opposed to smooth ones. That agrees with Köhler's observation.

What is an "appropriate name"? Usnadze (1924) asked his subjects to assign "the most suitable name" to a set of nonsense figures. For each of the six figures there were seven nonsense names to choose from. Usnadze found that the choices were not arbitrary. Some names, for instance, were never selected.[21] Subjects often chose on the basis of an "affective" relationship or a "general impression," Usnadze reports. But his statistics are

[21] With "8–9 subjects" only 29 names out of the 42 (6×7) were ever selected, Usnadze reports. This is exactly what one would expect for a chance distribution.

flawed and there is no further analysis of these special relationships. Much better is the paper by Charles Fox (1935) from the University of Rochester, who ran a more sophisticated version of Usnadze's experiments, the selection of a most appropriate (nonsense) name for a particular nonsense figure. He used 14 such figures, each combined with six names to choose from. The choices were not entirely random, but this was largely due to a general preference for the first name on the list.

One can reverse the question and ask, what is an appropriate visual cartoon or scribble for a meaningful word? Bartlett (1932) had observed that there are preferred associations between words and visual signs, such associations are more easily remembered than non-preferred ones. Hall and Oldfield (1950) designed an experiment to test this in more detail. Their subjects got cards on which a somewhat abstract word was printed (RHYTHM, WRONG, JUSTICE, SERENE, STORM, or PHILOSOPHY). Each word was combined with two mostly meaningless line figures. For each word they had to choose the most appropriate figural sign. They found a high degree of consensus among their subjects, clearly preferring one sign over the other for each of the six stimulus words. In a further experiment Hall (1951) reconfirmed this finding on 50 other abstract words (such as FATE, HARMONY, and CRIME). What causes the consensus? Interviews with the subjects did not help much to answer this question. There was talk of "general impression" and of "accuracy." McMurray (1958) then took the step of replacing interviewing with scaling. Using Hall and Oldfield's words and figures, and finding the same degree of consensus in their subjects' pairing of words and signs, he had his subjects rate all words and all signs on the 15 scales of Osgood's semantic differential (such as *good–bad*, *rough–smooth*, and *fast–slow*). It turned out that preferred word/sign combinations went with similarity in the semantic profiles of word and sign. That means affective similarity, back to Usnadze. Here, however, it presumably was the affective value of word meanings, not of (non-) word sound patterns.

What then about acoustic stimuli? After World War II, in 1954, Heinz Wissemann (1912–2001), Slavist at the University of Tübingen, took up the Usnadze's procedure, applying it to sound events. His subjects heard 14 different live events taking place in an adjacent room (with the door ajar). The events are so carefully described that one could replicate, record, and acoustically analyze them. There were events such as washing hands, dropping (and breaking) a thermos bottle on the wooden floor, or breaking, with two hands, a wooden stick. For each event the subject was asked to select the most appropriate meaningless word from a list of six. Examples of dominant choices were *güschguschum* for the washing hands event, *zrisch* for the thermo bottle event and *knok*[22] for the stick breaking event. Although the choices were by no means arbitrary, too much depended on the composition of the choice list for an event. Wissemann therefore decided to repeat the experiment with the open instruction to invent an appropriate spoken sound imitation. In addition the subjects were asked to guess what had caused the sound, to describe it in detail and to explain their chosen onomatopoeia. This got Wissemann into

[22] In German, both *k's* are pronounced.

the Würzburg trap of having to report introspection after introspection, page after page. Still, he did pick up some general tendencies in the subjects' naming. The basis of their onomatopoeias was always German phonology. Deviations thereof, such as *zkrak*[23] for the stick breaking event or vowel-less *krrch* for a rasping event, were often explicitly motivated in the introspections as expressing an acoustic feature. The new formations were often variations on existing words. One subject, for instance, denoted the stick breaking by *bristern*, a blend of *brechen* ("breaking") and *knistern* ("crackle"). More generally, sound analogy to existing sound-denoting words was a frequent basis for new formations. Subjects produced multisyllabic onomatopoeias when the sound event consisted of subevents, as in hand washing. There was otherwise no relation between event duration and length of onomatopoeia. Voiceless plosives were much preferred for the expression of clicking sounds and the dark vowels *o* and *u* were preferred for the expression of low frequency sounds.

A short but influential paper by Tsuru and Fries (1933) introduced a quite different procedure. The authors provided their subjects with 25 pairs of contrasting words and their Japanese translations (in Romanized spelling), such as *white–black*, *shiro–kuro* and *fast–slow*, *hajai–osoi*. The subjects had to assign the correct member of each Japanese pair to its corresponding member in the English pair. They succeeded in "at least 75% of the pairs," where 50% is chance. Curiously, the authors do not try to explain these results, except by resorting to "organized processes." Gordon Allport, in his Harvard dissertation (1935), successfully repeated this experiment with Hungarian word pairs instead of Japanese and so did Rich (1953) with Polish word pairs. Brown, Black, and Horrowitz (1955) got similar results with Chinese, Czech, Hindi, and Croatian word pairs. They also had Chinese participants doing the matching with Hindi and Czech. Brown in his *Words and things* (1958a) provided a rather subdued account of their findings. For the English language subjects they had replicated the earlier above chance results. They interestingly also found that where their subjects were wrong, they also tended to agree. In other words, the native English shared both their conceptions and misconceptions of the foreign words' meanings. A further check showed that this had also been the case in Allport's data. This, then, raises the question of whether these shared conceptions are specific to speakers of English. It might be due to analogy or to sound resemblances with English meaningful words. But that could undermine the universality of sound symbolism as Sapir (and other linguists, such as Jespersen) had played with, such as "largeness" of *a* versus "smallness" of *i*. So, what came out of the Chinese control subjects? Very little. They were better than chance on a total of just five word pairs in the Hindi and Czech lists. But since there were 42 word-pairs to be judged, this result was negligible. The authors had carefully included Sapir's *mal–mil* pair as an extra item in the Hindi list and offered *small–large* (in Chinese) as the to be matched translation. The outcome here was naught, so even the "largeness" of vowels had not worked. Later work

[23] The initial phoneme cluster is illegal in German.

by Miron and others, beyond the temporal scope of this book, would reestablish largeness of vowels as a good candidate for universal sound symbolism.

Metaphor and physiognomy

William Stern's 1893 dissertation was on analogy in folk thought. It also contained a chapter on metaphor. Whereas analogy formation is usually an unconscious phenomenon, the use of metaphor requires consciousness, according to Stern. Here Stern adopted Wundt's (1880) analysis. In metaphor a relation is established between two conscious images, the subject and the figure of metaphor. In Stern's terms, the one item gets denoted with the name of the other item. However, this does psychologically *not* implicate an essential identity of the two items, as further elaborated in Stählin (1914). Heinz Werner (1890–1964), whom we met in Chapter 10, moved to the Sterns in Hamburg in 1917 and published his first monograph on the origins of metaphor in 1919. It contains, however, no reference to William Stern's youthful work. Werner's treatment of metaphor is largely ethno-psychological. There is a seeming abundance of metaphor in "primitive" societies. They blossom in the context of taboos. The Dakota Indians talk about "precious children" when denoting buffalos to be killed. But Werner argues that, although these foreshadow metaphor in the evolution of language, they are not yet full metaphors themselves. They do not consciously, voluntarily arise, but are rather the result of speech need, of a lack of abstractive ability. We will not follow Werner in these ruminations, but turn to his *Grundfragen der Sprachphysiognomik* (1932), a monograph on linguistic metaphor or "physiognomy of language/speech." In Werner's treatment these phenomena include much more than phonetic symbolism alone. There exists, for instance, a physiognomy of syntax. When you compare the "medial" syntax of *the sword breaks* to the "active" syntax of *the enemy breaks the sword*, you experience a "dynamic" difference. The former sentence suggests the appearance of a sword exploding into pieces. The latter rather marks a controlled transition in the state of the sword. These, Werner argues, are not properties of the verb, because it is *break* in both cases. It must, somehow, be due to the syntax (p. 220). *Grundfragen* was an experimental study not of the meanings, but of the *appearances* of verbal materials, sounds, words, and phrases. The method was Würzburgian. The (trained) subject would receive an item and report on the impressions it provoked in consciousness. Here are two examples:

Stimulus *Holz* ("wood"). Introspection: "The woody character is determined by the brittleness, it is something splintery. 'Ol' is the focus of concentration for the sound, 'z' the splintering noise, at the same time character of the rigid, inflexible."

Stimulus *Bitter*. Introspection: "The word conveys the impression as if it convulsively, spasmodically contracts the mouth and afterwards shivers—brr!"[24]

[24] "'Holz': Der Holzcharakter ist bestimmt durch die Sprödigkeit, es ist etwas Splinterendes. 'ol' ist der Verdichtungspunkt für den Klang, 'z' das splitternde Geräusch, zugleich Charakter des Starren, Unbiegsamen." " 'bitter': das Wort hat den Eindruck, als ob es gepreßt, kramphaft den Mund zusammenzieht und sich hinterher schüttelt -- br!" (Werner 1932, pp. 74, 204).

Werner's trained subjects spared no effort in producing elaborate, colorful renderings of how the verbal items impressed them. Or, in other words, they liberally provided him with what he had asked for. It is Würzburg reduced *ad absurdum*.

Empirical studies of meaning were quite diverse during the period we are considering. But it was somewhat like blind men studying an elephant. Whereas it had been a long and respectable tradition to take the sentence as the basic unit of language, sentence meaning was hardly ever addressed in the empirical studies reviewed in this section. In particular, none of the studies was truth-conditional. Is sentence S correctly representing a state of affairs A? Is sentence S a logical entailment of sentence S? And so on. By and large, all studies were lexical. What, then, is lexical meaning? The dominant view during this period was that a word's meaning is the totality of conditions under which it is used, so how can we get at a word's meaning? By studying the word's network of associations to those conditions. The network consists largely of *verbal*, word-to-word associations. The royal road to the study of meaning was by word association. That holds for the traditional word association studies, originated by Galton and then in Wundt's laboratory by Cattell and Aschaffenburg, and leading to the Kent and Rosanoff scales and their wide use. It also holds for the scaling studies, where the strengths of word–word associations are judged. It holds for content analysis, where co-occurrences of words are statistically analyzed. Here, in particular, it is surprising that the propositional content of sentences and texts was never analyzed. Studies of sound symbolism were also tests of a spoken word's associations. Only Esper's artificial language approach went one step further. He asked the theoretical question: what is it that determines the strength of a word–word association? His answer was: association strength is conditioned by the overlap in conditions of use, mostly referential conditions, of the two words. In other words, a word's meaning resides in its referential conditions of use. Clearly, there was still a long way to go in the study of meaning.

Verbal learning and memory: orders of approximation

The study of human learning and memory usually involved verbal test materials, mostly nonsense syllables, digits, words, or short phrases, but the issues were rarely psycholinguistic in nature. Instead, the questions were more of the following: What size is our memory span? Is it different for nonsense syllables, digits, concrete words, abstract words, etc.?[25] What is the status of a learned, but not remembered, word?[26] What is more efficient, learning everything in one swoop ("massed learning") or distributed in chunks over time ("spaced learning")? Is learning facilitated by articulating the materials?[27]

[25] Yes it is different, Brenner (1940) demonstrated. For a review of the memory span literature, see Blankenship (1938).

[26] Ohms (1910) from Göttingen reported on an advanced technological enterprise of measuring the state of activation of learned paired associates. In his experiment the associates were Russian words. He measured their state of activation (after learning and with appropriate controls) by presenting these words over an acoustically reducing telephone line and measuring the subject's reproduction latency.

[27] Yes, according to Witasek (1907) and Barlow (1928).

One psycholinguistic issue we discussed in Chapter 8 was whether in classical conditioning there is generalization from a word such as *urn* to a semantically related one, such as *vase*, or to a homophone, such as *earn*. Razran had originated this paradigm and Cofer developed, with co-workers, the first mediation models to explain such phenomena of generalization. But not all behaviorists were happy with this simplified world. Cason (1934), commenting on the Razran paradigm, sighed: "It does not seem reasonable, however, to neglect the thinking and inner speech of human subjects just because the symbolic activities of subhuman animals are difficult to observe. . . . If the thought processes of people should be neglected because they are not found in rats, should we not also neglect those organic processes in monkeys which are not found in rats? It seems reasonable that none of these organic processes should be neglected." (p. 564). But it was a voice crying in the American wilderness. Kantor's student Briones (1937) at least tested whether it was easier to learn a list of words, such as *friend, house, love, . . .*, or a list of short sentences, such as *How are you?, That is true, I am hungry now.* The sentences are easier and moreover contain many more words. This may be of use in learning a second language, as Libby (1910) and Smith and Powers (1930) had already suggested. But why should this difference exist? The question was not even raised. Hovland in his 1951 review of human learning introduces his tiny section on verbal learning with "much remains to be known about the process, particularly at the basic level of symbolization and the functioning of language" (p. 618). This was mere lip service.

Ebbinghaus had elegantly demonstrated that meaningful materials are far easier to learn than nonsense materials. That wheel was reinvented time and again. Lyon (1914) compared the learning of nonsense syllables, digits, stanzas, prose, and the time it all requires. Tsao (1948) varied *degrees* of meaningfulness. Nonsense syllables can be more or less meaningful (DAP, TUS, and VOL are more "meaningful" than JID, PEJ, or YUF). Fortunately, the less meaningful ones are harder to learn. But they profit more from spaced practice, Tsao demonstrated. Don't ask why this is so. What counts in these days are objective facts, not how they come about. Welborn and English (1937) reviewed the learning and memory of meaningful materials. Their core observation was that memory is best, easiest, and longest retained for *substance*, for the gist, if there is any in the materials. We are best at remembering "fundamental details." This had been known since Binet and Henri (1894) and Henderson (1903–1904), and was confirmed time and again, for example by Key (1925–1926) and in particular in Bartlett's *Remembering* (1932).

Bartlett introduced the notion of *schema*: "an active organization of past reactions, or of past experiences, which must always be supposed to be operating in any well-adapted organic response" (p. 201). This somewhat woolly definition[28] reflected the British version of *Gestalt*, at least as it is organized in memory. Köhler, in his *Gestalt psychology*,

[28] In fact, Bartlett borrowed the term from Head, in spite of the fact he disliked it: "I strongly dislike the term 'schema'. It is at once too definite and too sketchy." (Bartlett 1932, pp. 200–201). Oldfield and Zangwill in a four-part paper, published in the 1942 and 1943 volumes of *The British Journal of Psychology* put considerable effort into clearing up Head's and Bartlett's theoretical notions of "schema." But neither language nor speech is an issue in these articles.

writes: "Evidently, a process of organization occurs during learning and most of all during the first readings of the series" [of syllables] (p. 217). Like Bartlett, Köhler rejects any theory of learning based on "blank" A–B associations. Association is not a basic mechanism as traditional psychology had always assumed. It is rather a derived notion. It can only exist by virtue of organization. Association "is not more than a name for the fact that organized processes leave a trace picturing their organization and that in consequence of it reproductions are possible" (p. 225). But then the question becomes what principles of organization are at issue in learning and memory, for our present purposes: as far as verbal materials are concerned. Here neither Köhler nor Bartlett were very specific. Inspired, however, by Bartlett's book, Oldfield and Zangwill (1938) ran an experiment in which they presented their subjects with three types of text to be learned. The first type was "normal" text, some 60 words long. One such normal text began as follows: *Under a cloudless sky, he left Royston at half past seven in the morning* . . . The second type was "ungrammatical" text, for example: *Under a cloudless skies, he leave Royston at halves past seven in a morning* . . . The third type was semantically anomalous or "inconsistent," such as: *Under a cloudy sky, he left Royston at half past one in the evening, the lately set winter sun beating down upon his face* . . . The changes or "errors" in the reproductions at successive stages of learning were then categorized by the authors as omissions, transpositions, synonymous phrases, or redress of grammar. The latter type of "error" was, of course, quite frequent in the ungrammatical texts. The learner apparently imposes some conventional grammatical organization on the text. For all three types of text there is an immediate first assignment of meaning, however vague. That is the initial schema. It then becomes enriched with additional detail. The anomalous texts were surprisingly easy to learn, because the bizarre elements became dominant in the initial schema, facilitating recall.

One cannot help thinking that "schema" is used here as a *deus ex machina*, taking care of organization: "the first stage of learning is the formation of a very general scheme which incorporates a certain amount of dominant detail" (p. 26). Still, Bartlett, Köhler, and their European colleagues demonstrated a refreshing open-mindedness in dealing with verbal learning and memory.

That could not be said about their American colleagues. Welborn and English had raised the issue of "gist" or "substance" in the memory of verbal materials. But what exactly is "gist"? It was far safer to continue concentrating on "stimulus generalization," following Razran and Cofer, than to answer that question. Underwood and Hughes (1950) therefore continued in their pursuit of generalization, but now concentrating on *errors* of recall. Their subjects learned lists of paired associates, consisting of a nonsense syllable and a word (e.g., DUP–TOILSOME). After a week the subjects were presented with the nonsense syllables and had to produce the associated words. Sometimes they made errors, such as responding *tiresome* to DUP. It turned out that most errors were of the synonym or antonym type. The next category was sound-related errors, including homonymity. In short, memory is organized in terms of stimulus generalization, both semantic and phonetic generalization.

Meaning remained the odd man out in the behavioristic world. It was generalized away, or as Weiss put it: "For the behaviorist the meaning of a stimulus is defined by the

responses that are made to it." Or Bloomfield: "We have defined the *meaning* of a linguistic form as the situation in which the speaker utters it and the response which it calls forth in the hearer." This immense context is not (yet) amenable to systematic study, hence: "It follows from all this that in all study of language, we must start from forms and not from meanings." (see Chapter 8).

That is the way Miller and Selfridge (1950) decided to approach the apparent role of meaning in the recall of verbal materials. Maybe the underlying factor is not so much meaning, but "contextual determination" (p. 184), which is a form factor. The speaker's production of a particular word depends on a host of factors, but many of these factors have also worked in the past. When you select a word in speech, you utilize previous learning. That history of learning has created verbal contexts which make it more or less probable that some particular word is uttered. The word *play* is a likely continuation of *children like to . . .* There is, of course, a meaning relation here. But the likely transition to *play* may simply be guided by learned transitional probabilities among verbal items, not by their intangible "meanings."

How are such transitional probabilities manipulated experimentally? Here Miller and Selfridge applied brand new notions from Shannon's (1948) information theory. They constructed nonsense materials for their memory experiment by varying the *degree of approximation* to text. This went as follows. For a *zero-order* approximation you pick words at random from Webster's dictionary, without regard for their frequency of occurrence in the language. Here is such a text:

Zero order: *splinter shadow dilapidate turtle pass stress grouse appropriate radio . . .*

For a *first-order* approximation you only take into account the words' frequency of usage. Take, for instance, every 13th word from a newspaper. Here is an example:

First order: *representing and speedily is an good apt or came can different natural here . . .*

For a *second-order* approximation your choice depends on the previous word selected. The way Miller and Selfridge handled this was by asking subjects to use a particular word in a sentence. From *another* the subject would construct *another method was needed.* The "next" word *method* was then given to a new subject, who would construct *the method for writing is by paper and pencil.* The next subject would get *for* to create a sentence around, and so on. Here is a text that could have resulted:

Second order: *that the character of this point is therefore another method for the letter that . . .*

Higher orders of approximation are constructed by presenting subjects with *sequences* of words to embed in a sentence: pairs, triples, etc. The higher the degree of approximation, the more "normal" a text becomes. Here is a seventh-order approximation:

Seventh order: *easy if you know to crochet you can make a simple scarf if they knew the color . . .*

In their experiment Miller and Selfridge presented "texts" ranging from zero to seventh order of approximation, plus a real text. The texts also varied in length; they were 10, 20, 30, or 50 words long. A subject would listen to such an experimental text, wait for a bell to sound and then write down all words recalled. Figure 12.8 shows the results. It is evident from the figure that text approximations of order four or higher were recalled just

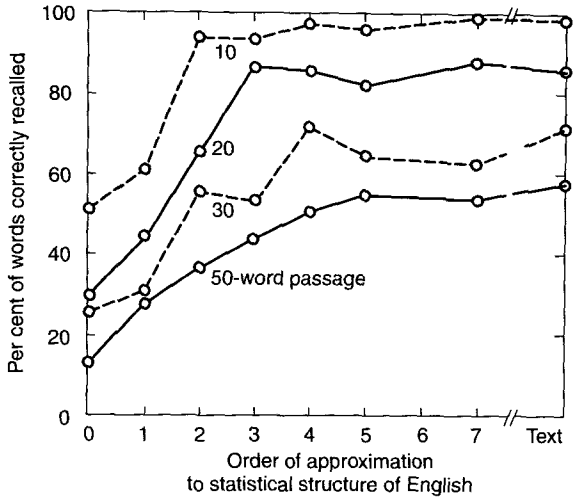


Fig. 12.8 Recall of "texts" of different orders of approximation to English. Figure 1 from Miller and Selfridge (1950). From George A. Miller and Jennifer A. Selfridge, Verbal Context and the Recall of Meaningful Material, *American Journal of Psychology*, *63*, 176–185, figure 1, © 1950 by the Board of Trustees of the University of Illinois. Used with permission of the author and the University of Illinois Press.

as well as real text. So, is it "meaning" that counts or transitional probability? Probably the latter, according to the authors: "the nonsense is as readily recalled as is meaningful material . . . it is these familiar dependencies, rather than the meaning *per se*, that facilitate learning" (p. 184). Deese and Kaufman (1957) only presented zero-order and real text for their subjects to recall. Real text was of course easier, but their new finding was that the reproduction of text largely preserved word order, whereas for a random word list the subjects first recall the last words in the string, then the initial words and finally whatever they can reproduce from the middle part. In other words, the recall *organization* is different in the two cases, a far echo from Bartlett.

The statistical approach

Modern psycholinguistics is unthinkable without its language statistical tools. We take for granted extensive lexical and textual databases and phonological, morphological, and lexical frequency data from several languages. But here, too, we stand on the shoulders of pioneers going back to the late nineteenth century. These pioneers had practical aims. Friedrich Wilhelm Kaeding (1843–1928) was a stenographer who conceived of, but also executed, the megalomaniac plan of composing a frequency dictionary of the German language based on texts encompassing some 11 million words. There were over 600 people involved in reading, counting, and tabulating words, syllables, and letters. The dictionary appeared in 1898 and served the further development and teaching of his preferred shorthand system. It set a standard which would not be matched for almost a century.[29]

[29] The CELEX lexical database for German, English, and Dutch (Baayen *et al.* 1996). It was developed, from 1993 onwards, at the Max Planck Institute for Psycholinguistics in Nijmegen, the Netherlands.

In France, leading stenographer Jean-Baptiste Estoup (1868–1950), author of a best-selling stenography text (1902 and many further editions), had analyzed 75 texts, totaling 30,000 words for their word frequencies. He took successive 1000 word samples from this text and counted how many *new* words appeared in these samples. The first sample contained 336 different words, the second 233 new ones, and so on. The 30th and final sample added just 36 new words. He also drew the histogram of these data and got the characteristic hyperbolic falling curve. In 1935 Vander Beke published frequency tables for French, based on just over a million words of text.[30] In the United States, another developer and teacher of shorthand systems, Godfrey Dewey,[31] performed word, syllable and sound counts on a mixed text base of 100,000 words (peanuts compared to Kaeding's enterprise). His (1923) tables became widely used and cited.

Another practical context for the collection of word frequency data was, of course, school education. Eldridge (1911) published a slim booklet *Six thousand common English words*, frequency counted, for teachers. Edward Thorndike (1874–1949) of Teacher's College, Columbia University, published in 1921 *The teacher's word book of 10,000 words*, with frequency counts of the most frequent words in English. In 1932 he published his upgraded 20,000 words and in 1944, together with Irving Lorge, an expanded 30,000 words. The teaching of English, both to native children and to foreigners, was to profit from such word-frequency data. The business of counting words, in various languages, for educational purposes was immense during the first half of the twentieth century. It has been well reviewed by Fries and Traver (1950).

We will not consider these and other developments in quantitative applied linguistics, but turn to studies explicitly addressing psycholinguistic issues. Two closely related statistical distributions play a core role throughout the literature: the rank-frequency distribution and the number-of-words-frequency distribution. Who were the first to publish them?

The rank-frequency distribution

Edward Uhler Condon (1902–1974) was an assistant professor of physics at Princeton University and briefly worked as a public relations agent for Bell Telephone Laboratories, at which time he published a note in *Science* (1928) on the relation between the frequencies of words in a text sample and the ranks of these frequencies. His plot is reproduced in Figure 12.9. Condon had used two published word counts, one from Dewey (1923) and the other from Ayres (1915). Here is how he described his procedure: "Suppose one takes a large representative sample of written English, counts the number of times each word appears and arranges the words in order of decreasing frequency of occurrence. The n^{th} word in such a list will then occur with an observed frequency which is a function

[30] Also including the earlier but unpublished, mimeographed tables by Henmon (1924), based on 400,000 words.

[31] Dewey was also a missionary of spelling reform. Here are some of his "newwrites": *analitic, derivativ, fonetic, givs, hav, markt, sillable, sistem, telefone.*

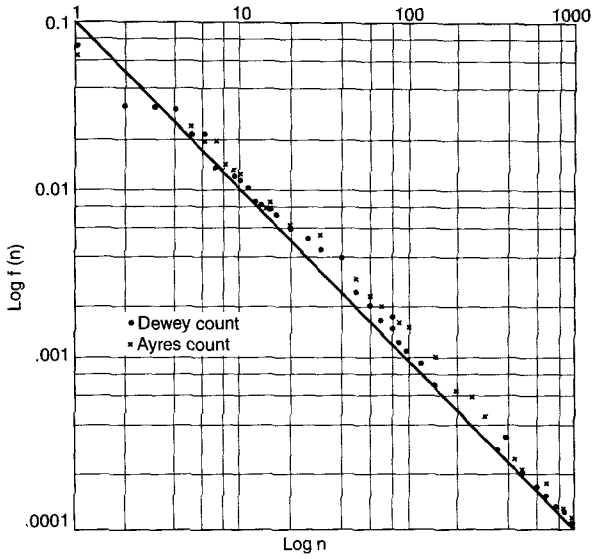


Fig. 12.9 The very first plot of the rank-frequency distribution.[32] *From Condon, E.U. Statistics of vocabulary. Science, 67,* 300 © 1928 The American Association for the Advancement of Science. Reprinted with permission from AAAS.

of n, call it f(n)." The (double-logarithmic) plot suggested to Condon that the function is simply f(n) = k/n, or n.f(n) = k. In words: the product of rank and frequency of a word is constant (k). How large is constant k? Just about 1, as appears from the 45° slope of the plot. At this point, Condon closes his one-page paper with the suggestion that there may exist a Weber–Fechner law for language. The added "idea-transmitting power" of a new word in the language diminishes with the size of the vocabulary, following a power law. And that was Condon's last word on this entire issue. Condon made a lightning, exemplary career as a physicist, becoming director of the National Bureau of Standards, and then for years was haunted by the House Un-American Activities Committee.

The number-of-words-frequency distribution: Zipf's law

George Kingsley Zipf (1902–1950) spent all of his (too short) academic career at Harvard University, except for a year's study visit in 1925, right after graduation, to the Universities of Bonn and Berlin. From 1936 he was university lecturer (a supra-departmental university professorship) at Harvard, teaching German. Zipf began publishing on phonemic frequency distributions in 1929 (his language comparative dissertation project), followed in 1932 by his wider analyses of lexical, syllabic, and phonetic frequency distributions in Latin and Chinese texts. But the monograph that established his great reputation was

[32] Felix Auerbach (1913) had noted the same distribution for city sizes. When you rank cities according to their size, he noticed, the product of rank and population size is a constant. Zipf rediscovered that population statistic and published a book about it: Zipf (1941).

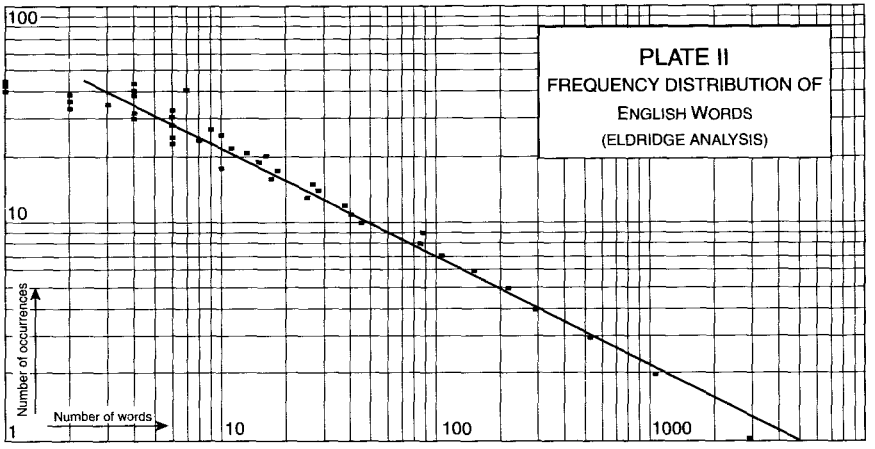


Fig. 12.10 Zipf's plot of word frequencies in English newspaper data. Reproduced from George K. Zipf, *The Psycho-Biology of Language: An Introduction to Dynamic Philology*, Plate II: Frequency Distribution of English Words, © 1965 Massachusetts Institute of Technology, by permission of The MIT Press.

The psycho-biology of language: An introduction to dynamic philology (1935). Figure 12.10 shows one of Zipf's frequency curves. It is based on Eldridge's (1911) data on newspaper English. The vertical axis in this figure is the logarithm of word frequency, just as in Figure 12.9, but the horizontal axis is the number of different words sharing that frequency.

Figure 12.10 shows that there are only a few words of very high frequency (*the* being the most frequent one) and large numbers of words of low frequency. In particular, there are usually many words that appear only once in the whole text. Zipf was impressed by the apparently linear relation in the plot. The slope indicates an exponent of about 2. The simple function describing it is $nf^2 = k$. In Zipf's own words: "the product of the number of words of given occurrence, when multiplied by the square of their occurrences, remains constant for the great majority of the different words of the vocabulary in use, though not for those of highest frequency" (pp. 41–42). Zipf was aware of the fact that the exponent 2 would not hold for short or for very long texts. So, how to find a *constant* measure for any length text?

Here Zipf recognized the advantage of using *ranks* as a measure. Without referring to Condon,[33] he presented his own rank-frequency plots for English and Latin (Plautine) texts, which (over rank 10) looked much like Condon's plot. He then introduced the new notion of lexical *wave length*, which is the average interval length from a word till its reoccurrence in the text (expressed in number of words). The wave length for *the* is very short in an English text (about ten words), but the wave length for the low-frequency word *democracy* is very long. A word's average wave length is, of course, the length of the

[33] Zipf refers to his source as: "as suggested by a friend" (p. 44). There are, however, full references to Condon's paper in Zipf (1946, 1949).

text divided by the word's frequency in the text. It is, then, very easy to compute the wavelength for words of any rank, Zipf argues, because *empirically* a word's wave length is ten *times its rank*: $w_i = 10r_i$. This was called the *harmonic series* relation. So *the* (rank 1) repeats on average every tenth word, *of* (rank 2) repeats every 20th word, and so on. Martin Joos (1936) in his quite critical review of Zipf's book shows that, under reasonable assumptions, the w = 10r relation follows mathematically. In other words, we are dealing with a formal necessity rather than a deep ("overwhelming") empirical disclosure. This did not convince Zipf, as is evident from his 1943 paper in *Science*,[34] in which he presented the rank-frequency curve for a girl approaching the age of 5;0. It has the "standard" shape and Zipf announces an analysis of how the constant C develops with age and how it relates to the development of intelligence.

For Zipf the regularities observed are the outcome of a deep property of mind, "the principle of least effort." We met that principle or "law" before in the work of Schultze (1880), an evolutionary principle he applied to the child's gradual acquisition of speech sounds. *The principle of least effort* became the title of Zipf's 1949 (and last) book. There he exemplifies the principle as follows. Words are tools for the communication of meanings. Take first the perspective of the speaker, "who has the job of selecting not only the meanings to be conveyed but also the words that will convey them." From this viewpoint "there would doubtless exist an important latent economy in a vocabulary that consisted exclusively of one single word" (p. 20). If there are *m* possible meanings to be expressed, that one word would have *m* meanings (this is somewhat like your dog barking). No vocabulary to be learned or maintained, no words to be selected, etc. But this would result in "the acme of verbal labor" (p. 21) for the auditor, who has to detect the intended meaning (as dog owners know). The hearer's economy would be optimally served by the existence of a different word for every meaning, with "one meaning per word." Clearly, the two economies are in conflict. The speaker's economy is a *force of unification*, the hearer's economy is a *force of diversification*. The former force will push to decrease the number of different words, resulting in a small vocabulary of high-frequency words. The latter force will push to maximize the number of different words. The result is a compromise, which minimizes the joint expenditure of effort (Zipf does not mention that the language-user is a speaker-hearer). There will be a small number of high-frequency words with multiple meanings and a large number of rather unambiguous low-frequency words. That in itself explains the global characteristic of the above frequency curves as in *Fig. 12.10*.

Zipf spared no pains to present further evidence for his economy principle. Here are his major cases:

(1) High-frequency words have more different meanings than low frequency words. That is in the speaker's interest. But there will be many low-frequency words with few meanings. That is in the auditor's interest. Thorndike (1941) presented Lorge's count of word meanings. Zipf plotted them against their ranks (up to 20,000)[35] and found

[34] In response to a critical *Science* paper by Carroll (1941).

[35] And averaging over sets of *1000* words.

a slope of −0.5. In other words, the number of meanings of a word is proportional to the square root of its frequency. That, Zipf argues, is what one would expect if the two forces are in equilibrium.

(2) Easy phonemes should be more frequent than complex ones. Zipf (1935) confirms this prediction, among others, by presenting a table (p. 75) listing the frequencies of six phonemes in 12 languages. The table shows that the voiceless consonants *t, p, k.* are substantially more frequent than the corresponding voiced ones *d, b, g.* And that is as it should be: the voiced plosives are "more complex" in articulation than the unvoiced ones. But are they? Not according to Joos: "a voiced stop is easier to manage than a voiceless one, since it does not require cessation of voice after a preceding vowel" (pp. 207–208). Clearly, Zipf first listed the phoneme frequencies and then categorized the low-frequency ones as "more complex," according to Joos. Whatever the truth of that, Zipf stood in a longer tradition here (which he does not cite). Schultze had also categorized the voiceless consonants as less complex (and earlier acquired) than the voiced ones. Tracy (1892) had confirmed that order for *t, p* versus *d, b,* but not for *k* versus *g.* Zipf could not know about the impending Trubetskoy–Jakobson developments in phonology here. In that system, voicing becomes a marked, distinctive opposition to the default unvoiced articulation of consonants.

(3) High-frequency words should be shorter than low-frequency words, Zipf's *law of abbreviation.* This will obviously reduce effort and increase rate of communication; the abbreviation is *intentional.* The statistical data confirm this expectation (Zipf 1935, pp. 26–28; 1949, p. 64). A word's number of phonemes and syllables decreases with its frequency. For Zipf there is an obvious causality here: increasing frequency causes abbreviation for the sake of reducing effort; there is an intentional striving for economy. In his review of Zipf (1935), Joos ridicules this argument. If two statistical variables are correlated, either one may be the cause of the other, or there may be a third factor affecting both. Zipf, Joos argues, is prejudiced, "he has already decided what it ought to be" (p. 204), whereas Zipf himself, would you believe, provides the argument to reverse the causality: according to Zipf the same meaning can be expressed with different words, of different length. Why would the speaker then not choose the shortest one (minimizing effort), such (causally) increasing its frequency? The principle of minimal effort will always work, one way or another, in explaining the curves, according to Joos.

"Zipf's law," in either version ($rf = k$ or $nf^2 = k$), acquired a life of its own in quantitative linguistics. But what came of Zipf's psychological explanations? Does the law express a deep principle of mental economy or is it, rather, a stochastic necessity? The tide turned with Benoit Mandelbrot's (1954) mathematical formalization of the rank-frequency relation and the host of ensuing literature. We will not follow this in detail, but rather cite George Miller's (1965, p. vii) masterly summary of its outcome: "Suppose that we acquired a dozen monkeys and chained them to typewriters until they had produced some very long and random sequence of characters. Suppose further that we defined 'word' in this monkey text as any sequence of letters occurring between successive spaces. And suppose

finally that we counted the occurrences of these 'words' in just the way Zipf and others counted the occurrences of real words in meaningful texts. When we plot our results in the same manner, we will find exactly the same 'Zipf curves' for the monkeys as for the human authors. Since we are not likely to argue that the poor monkeys were searching for some equilibrium between uniformity and diversity in expressing their ideas, such explanations seem equally inappropriate for human authors." In other words: Zipf's psychology may be correct, but it needs a different kind of empirical support.

Zipf's law in associations

Skinner (1937) was quick to apply Zipf's analysis to the association patterns in the Kent and Rosanoff (1910) tables. It contained 1000 associations to each of 100 stimulus words. Skinner did a rank-frequency analysis for the responses to each stimulus word. A decent Zipf curve arose when ignoring associations that occurred only once, i.e., the *hapax legomena*. These, Skinner argues, are not really associations to the stimulus but just random responses. The best-fitting rank-frequency function obtained was $f.r^{1.29} = 300$. "Its bearing upon theories of speech could be stated only at greater length than this report will allow." But that never came.

Diversity of words in language usage

Edward Thorndike (1937) was quite sceptical about Zipf's sweeping claims. "It seems to me unlikely that the relation for such total usage, no matter how uniform it turned out to be, would be evidence of any uniform and ubiquitous tendency toward a certain equilibrium between frequency and variety. I should expect that it would be in some measure a statistical effect." (p. 405). The constant k in Zipf's $nf^2 = k$ will steadily increase with sample size, Thorndike argues on the basis of both empirical data and mathematical computations. Anyone concerned with the *diversity* of word usage of a person or group of persons will have to find a solution for how n (the number of different words of a certain frequency) is related to sample size. For Thorndike, the educational tester, this was certainly a practical problem.

John B. Carroll (1938), then still at the University of Minnesota, took a first step towards solving that problem. It is intuitively clear that if more words are added to a text, there will also be more *different* words. That had already been demonstrated by Estoup. If you assume that, although the vocabulary of a language can be large, it must be of finite size, then the growth of diversity with text size must approach an asymptote. Carroll derived a theoretical formula for the growth curve, making some simple assumptions. He then fitted the data from a simple production experiment in which students had to create a sentence, given a frame such as * Y F * (here a "correct" response would be *Is your father sick?*). It provided him with 10,000 different sentences. He then counted the number of *different* words for sample sizes of 200, 400, 600, etc. words, up to the full amount of 10,000. There was a perfect fit with the theoretical curve, approaching an asymptote of 2200 different words. So, in principle, there was a method here of predicting the limit of a language user's diversity from a restricted text sample, the person's "active" vocabulary.

The issue was taken up again by Wendell Johnson (1906–1965) and his team at the Ohio State University Departments of Speech Pathology and Child Welfare. In their 1944 Psychological Monograph *Studies in language behavior* they approached diversity differently. Johnson (1939) had introduced the notion of *type-token ratio* (TTR). It is simply the number of different words (*types*) in a text divided by the total number of words (*tokens*) in the text. This measure, like Carroll's diversity, is dependent on text size. Johnson's team's practical solution was to cut up the relevant text in 100-word stretches, compute the TTR for each and then average them. That measure is, by and large, independent of text length. As long as it is not your purpose to estimate the size of a person's active vocabulary, this is a fine measure for individual differences. Johnson's team compared adults and children, age groups, IQ groups, sexes, schizophrenics, and normal adults. TTR was not the only statistical measure they applied. They also computed the *proportionate vocabulary*, which is the number of *different* words making up 50% (or 25%, 75%) of a text, the *type proportions* in a text (the percentages of nouns, verbs, adjectives in the text), and the *adjective/verb ratio*. The latter was a late echo of Busemann's (1925) verb/adjective ratio, which we met in Chapter 10. David Boder had used it as early as in 1927 as a statistic for differentiating literary styles, such as in drama, fiction, law, and science. Apparently triggered by the sudden text statistics trend since Zipf (1935), he still published his old study in 1940. That, in turn, was read and cited by the Johnson team. They diligently collected spoken and written texts to the length of about 3000 words from their subjects, freshmen, schizophrenics (in the Fairbanks (1944) and Bachman-Mann (1944) studies), and from no less than 1000 children from public schools (Chotlos 1944). It all displayed the large-scale statistical thoroughness we are accustomed from the Ohio Child Welfare studies (see Chapter 10). But it also exhibited the usual predictability of the findings. The higher a child's IQ, the higher its TTR and the higher the proportion of nouns versus verbs. The freshman's TTR is a trifle higher than the schizophrenic's, and so on.

Yule on the statistics of style

British statistician Udny Yule (1871–1951) of Cambridge University, who had studied with Pearson and Hertz among others, turned to the statistics of style quite late in his career. A first paper appeared in 1939. Its aim was to decide on the authorship of the *Imitatio Christi* and one other text with disputed authorship. For the *Imitatio* the question was to distinguish between Thomas a Kempis and Jean Charlier de Gerson as potential authors. Yule decided to use sentence length as his statistic.[36] By comparing the length distribution in the *Imitatio* to those in admitted works of both authors, Yule could confidently assign the *Imitatio* to Thomas a Kempis. Yule's text leads one to suspect that its author was totally unaware of Zipf's work and the ensuing literature. That was not strictly true of Yule's very last book, which he wrote during the dark years of World War II and published in 1944. "Neither old age nor the anxieties of war are favourable to

[36] Yule's distributions of sentence length are quite skewed. Williams (1940) proposed the use of log(sentence length) as a statistic, which indeed makes the distributions more or less normal.

continuity and clarity of thought," he wrote in his preface. On p. 32 we read: "It was not until after the draft of the great bulk of this book had been completed that I was given a reference to the work of Zipf." And indeed, the book hardly makes reference to Zipf's work, except that Yule gives short shrift to Zipf's law on purely statistical grounds.[37] Zipf's (1946) response was mild: "Despite the admitted incompleteness of Yule's work, which may have resulted from Yule's strange unawareness (p. 32) that anyone else had compiled and studied word distributions, the analysis itself is of great didactic value both for its statement of statistical problems related to the topic and for its application of statistical methods to some of those problems." (p. 339). Yule's influence on the psychology of language remained minimal, not comparable to Zipf's.

Word frequency and recognition threshold

The measurement of visual word recognition latencies had been an active field since Cattell's first studies in Wundt's laboratory, but word frequency was never systematically varied in this tradition until Katherine Preston's (1935) landmark publication. She took 50 high- and 50 low-frequency words from Thorndike's (1932) list. All words were bi-syllabic and consisted of 6 letters. They were presented in a Dodge tachistoscope and the subject spoke the word into a voice key. The mean voice latency for high-frequency words was 578 ms, for low-frequency words it was 691 ms, a highly significant difference. Surprisingly, this excellent paper was hardly ever cited. David Howes ignored it in his (1951) Harvard dissertation. He was the first to find correlations (in the order of $r = 0.75$) between (log) word frequency and visual recognition thresholds. This study, co-authored with his supervisor Richard Solomon, was published in 1951. In 1957 Howes showed the same relation for the recognition of spoken words in noise.

Solomon and Howes (1951) also demonstrated that an earlier threshold study by Postman, Bruner, and McGinnies (1948), and by implication McGinnies (1949), had to be reinterpreted. These authors had shown that the recognition threshold of words correlates with the perceiver's system of personal values.[38] A word from a high-interest domain (such as "political" or "aesthetic") would have a relatively low recognition threshold. When Solomon and Howes (1951) controlled for word frequency in a recapitulation of these experiments, the correlation with value entirely disappeared. Only word frequency was effective. McGinnies (1950) rejected that conclusion,[39] but both Postman and Bruner accepted the critique as a further challenge.

Personality effects (interest, anxiety) on word recognition threshold may, in fact, be mediated by *personal* word frequencies, they argued. The subject may have had more

[37] $nf^r = k$ is an "unlikely sort of formula," Yule (1944, p. 55) writes, "since for values of r exceeding 1 but not exceeding 2 the mean of the theoretical distribution is infinite, and for values exceeding 2 but not 3 the standard deviation is infinite."

[38] As measured by way of the Allport-Vernon (1931) value scale.

[39] And so did Whispé and Drambarean (1953). In an experiment in which subjects were deprived of food and water, they found lower recognition thresholds for "need relevant" words, such as "water," over and above the general word frequency effect.

frequent recent experience with personally relevant critical words. Can it be demonstrated experimentally that *induced* word frequencies affect recognition thresholds? Solomon and Postman (1952) presented a pack of 100 cards to their subjects. Each card contained a "strange" non-word and the subject was asked to pronounce it. Words could reappear in the pack. The ten critical test words appeared 0, 1, 2, 5, 10, or 25 times, i.e., with different frequencies. Next, the time needed for correctly recognizing these words was measured by means of a tachistoscope. Indeed, the authors found that recognition thresholds decreased with increasing frequency of an item in the test pack, and so did King-Ellison and Jenkins (1954), who essentially repeated the Solomon–Postman procedure. Bruner *et al.* (1955) found a similar effect, but in addition showed that the thresholds varied with the number of alternative test items in the set. That raised the question of whether the threshold effect is one of perception or one of recall or responding. Howes (1954) opted for response bias. It is unlikely, he argues, that the frequency effect is mediated by how often one has read a word, but rather by how often one has spoken it. A student has seen any printed word so often that they have all reached "an asymptote of learning." Moreover, in reading texts, one skips many if not most of the words. Text-based frequency tables do not reflect perceptual frequencies. It is the frequency of personal usage that counts, the habits of responding. Rosenzweig and Postman (1958) endorsed this interpretation without further experiments, but Goldiamond and Hawkins (1958) invented a way of testing the response bias hypothesis.

They used Solomon and Postman's pack-of-cards procedure, where the subject had to spell and pronounce each "foreign word." The ten critical items occurred at different frequencies in the pack. The subjects were then tachistoscopically tested on recognizing these words. However, that was all fake. None of the words were ever actually presented, instead there was always the very same flash of a gray pattern presented briefly. The subjects were instructed to always make their best guess. A perfectly linear relation emerged between the response frequency and the frequency of preceding training. Here, only response bias could have done the work. The experiment is reminiscent of Skinner's (1936) work on the "verbal summator." This finding did, of course, not exclude the possibility that *in addition* perceptual factors play a role in the normal word frequency effect. Neisser (1954), in an experiment with a quite "modern" touch, took the opposite position. Neisser used homonym pairs, such as *no–know* and *threw–through*. The subject would first study a card containing ten words, all of them homonyms, but without their "twins." They were then tested for their tachistoscopic recognition of various words, some of them from the list they had studied. They were tested on 15 words. Five of these had occurred on their card. Five others were homonyms of the other words on their card and five more words were new control items. The outcome was crisp. The five words they had seen before were easiest to recognize. The five homonyms, however, were harder and not different from the five control words. Neisser argued that if the "set" words on the card had raised response probabilities, the homonyms should have profited, because as a response they are identical to five of the words they had seen on the card. But that was not the finding.

Word frequency and word association

Howes (1957) statistically compared the frequency distributions of words in texts and as associated words in the Kent–Rosanoff tables. The issue for him was whether producing words in an association experiment is statistically comparable to producing a word in running text. The issue is important for the theory that was dominant during the 1950s, namely that any spoken (or written) text is the outcome of a word-to-word chain of probabilistic associations, a Markov process. The outcome of Howes's computations by and large confirmed this notion. This was his conclusion: "the average probability that a given word will be emitted as a response in the word-association experiment is the same as its probability in general discourse. The language of association can therefore be said to be *stochastically equivalent* to the language of general discourse." (p. 84). Clearly, Howes' (1951) discovery of the word frequency effect had opened a productive new research paradigm.

Transitional probabilities

We have already considered Miller and Selfridge's application of Shannon's (1948) information theory to the study of text memory. Miller, Bruner, and Postman (1954) added a similar experiment, but now for letter sequences. They constructed non-words consisting of eight letters. These letter sequences varied in order of approximation from 0 to 1, 2, and 4. Examples of such non-words were: BHDTUNQK, NHIDCFRA, RIPRYPLI, and LYMISTIC, i.e., increasingly "word-like." The stimuli were flashed onto the screen for short durations and the subjects wrote down the letters they had identified in the eight letter positions. The scores increased with the order of approximation and hence with decreasing information value in the sense of information theory. Recognition was 90% correct for fourth-order non-words that were visible for half a second.

The orders of approximation approach certainly stirred the imagination. In Chapter 1 we discussed how the behavioristic theory of serial habits made it natural to apply the theory of Markov chains to verbal strings. But we also saw that the time became ripe for a change of perspective. We considered John Carroll's careful remark in the 1951a report that "the characteristics of messages containing a hierarchy of units have not been mathematically formalized in any satisfactory way" (p. 22). We also saw Lashley's (1951) paper becoming a turning point in the psychological perspective on serial structure in verbal behavior.

The statistical approach has doubtless enriched the psychology of language. Considering the statistics of speech sounds, syllables, words, and larger phrases in their contexts has become part and parcel of modern psycholinguistics. It is not an exaggeration to say that it became the most enduring contribution of behaviorism to the field, together with increased experimental sophistication. The approach thrived on the conviction that objective, scientific, and quantitative research could be done on language, without getting lost in unmanageable problems of meaning: "meaning and meanings do not lend themselves to quantitative measurement," Zipf writes. "Yet, by the isolation of other factors which can be measured, we may gain a considerable insight into the nature of meaning, and perhaps finally apprehend something of its nature and behavior." (Zipf 1935, p. 48).

Miller (1951, p. 172) seconded him: "In summary, the associationist theorist seems to have reasonable answers for most of the questions we can ask about meaning."

Individual differences

Applied psycholinguistics was much on the rise between the two world wars. We have already observed the development in aphasiology and in language acquisition research. Psychological testing of verbal abilities, or using standardized verbal materials, was good business for applied psychologists, both in Europe and the United States. Here we will consider some of these applications, more to show major directions than to be comprehensive on a vast literature.

Linguistic abilities

The previous section touched on statistical approaches to testing verbal abilities and Chapter 10 reviewed much of the literature on testing verbal abilities in children. Guy Whipple (1908) was probably the first to apply Kirkpatrick's (1891) classic procedure for testing vocabulary size on college students. The students' computed vocabularies had a size of about 20,000 words, just about the same as Kirkpatrick's own. Whipple also developed a kind of anagram test. The student would be given six letters, such as *a*, *e*, *o*, *b*, *m*, and *t*, and had to make as many words as possible, using each letter at most once in a word. Some students were very good at this, coming up with over 30 different words, but the average was about 20. This test showed moderate correlations with the Kirkpatrick test. An interesting observation was that subjects rarely produced one-letter words, such as *a* and *I*.

Since this early study, the testing of verbal abilities largely developed in the framework of intelligence testing. Marion Trabue (1916) of Columbia's Teachers College, for instance, tested over 10,000 children of grades II to VIII plus college students on sentence completion tests. In the introduction to her monograph she cited Professor Ebbinghaus, who had called this "a real test of intelligence." The tests consisted of sentences with open word slots to be completed, such as *Boys and* _____ *soon become* ____ *and women.* It is beyond the aims of this book to relate the story of testing verbal intelligence.[40] A central figure here was Louis Leon Thurstone (1887–1955) of the University of Chicago. He developed the factor analytic approach to mental testing. The various subtests of the test battery are correlated and how these tests cluster is computed.[41] These clusters or "factors" reveal the underlying mental abilities, it was supposed. Thurstone (1938) presents a multitude of language scales. Each of these scales had had its own history, but in 1941 two studies independently took up Thurstone's factor analytic approach, applying it specifically to language tests. Is linguistic ability "one" or does it involve a number of relatively independent underlying faculties? Johnson and Reynolds (1941)

[40] See, for instance, Gardner (1993).

[41] You essentially compute the principal components of the inter-test correlation matrix. A good approximation was Thurstone's so-called "centroid method," which you could run on a mechanical calculator (as I used to do myself).

used a total of ten subtests, some of them used in most standard tests, such as the aforementioned completion test. They tested 120 college students and extracted two factors from their correlation matrix. The first one involved tests which the authors labeled "flow." This included subtests such as writing as many different words as possible within two minutes and the completion test. The other factor was labeled "selection." A typical subtest here was "analogies," for instance *Irksome is to wearisome as engrossed is to* _____.

This study, however, was entirely overshadowed by that of John B. Carroll (1941). This publication was essentially Carroll's dissertation, as we saw in Chapter 1, which had been supervised by Thurstone at the University of Chicago. Skinner, Carroll's supervisor at the University of Minnesota, had helpfully directed him to Thurstone. Carroll's verbal abilities test consisted of no less than 42 subtests, including a half-dozen from Thurstone (1938). His factor analysis was based on the test results of 119 college undergraduates. Carroll listed as many as nine factors and interpreted them as underlying language abilities. Here are the first two of them: Number 1 was characterized by Carroll as "the stock of linguistic responses" available to the subject. It underlies scales that measure vocabulary, spelling, grammar, morphemes, and suffixes. Number 2 is a "verbal reasoning" factor, according to Carroll. The "verbal analogies" scale is a good example, with subtests such as the *Irksome* one.

Projective-clinical

Skinner (1936) had suggested the possibility of using his "verbal summator," as a diagnostic, very much like the Rorschach ink-blot test. This was first tried out by Mary Trussell (1939), who gave the test to 32 psychiatric patients (a mixed bag of schizophrenics, depressives, psychopaths, etc.) plus 32 control participants. Her sad but hopeful conclusion was: "Although few qualities of summated speech have here been found to differentiate abnormal from normal subjects, this technique doubtless has value in other directions." (p. 538). Shakow and Rosenzweig (1940) also explored the characteristic responses of schizophrenics. In this paper, however, they developed a full scoring system for verbal summator responses, in analogy to the available Rorschach scoring systems. This methodological step up was adopted by William Grings (1942), supervised by Wendell Johnson. He tested groups of schizophrenic, psychoneurotic, and manic-depressive patients and scored their responses in great detail. The upshot was rather limited. He found, for instance, that the responses of depressives kept very close to the stimulus. Psychoneurotics, on the other hand, only produced meaningful responses. But schizophrenics were masters in "subjective" responses with lots of "human reference." One of them gave as a final response: "I believe in Freud and Spinoza." It was precisely this extreme individual variability of response patterns that made the test hard and also quite time-consuming to handle, and that was probably the end of it.

Personality

"Speech as a personality trait" was the title of a paper published in 1927 by Edward Sapir. It presents a conceptual analysis of features of speech that are expressive of personality.

Sapir makes a careful distinction between the social norm and individual variation. Take the range of intonation used in speech. The social norm is not only implicated by the language community at large, such as the speakers of English, but also by the local community, such as that of the Kentucky farmer or the English country gentleman. But given that norm, there is always leeway for individual variation. Sapir then considered five levels of speech behavior where significant individual variation, expressive of personality, can take place. There is, first, the individual voice, which can be "nasal," "cracky," etc. This is largely innate, but also adaptive: one can make one's voice more (or less) "agreeable," for instance. There are, next, the "voice dynamics," its prosody-in-use. There are set cultural norms here, such as applying question intonation (which is not universal according to Sapir), but also significant individual variation, such as in speaking rate and "continuity." There is, third, "pronunciation," which is most conspicuous in foreign accents. Fourth, a person's vocabulary is highly expressive of personality, but it can only be appropriately appraised with reference to the received social norm here. Finally, there is "style," a person's method of arranging words. Here in particular, norm and individual variation are hard to disentangle. Sapir observes that hardly any of these levels has been systematically studied from this personality perspective.

That was also the opinion of Thomas Pear (1886–1972), professor of psychology at Manchester University, whose monograph *Voice and personality* (1931) was probably the first text on this topic. His Chapter II, on the characteristics of voices, is a further elaboration of Sapir's paper and chapters on personality, prejudice, and stereotypes follow. The essence of the book concerns two "media studies" as we would call them now. In the first and minor one, a total of about 200 subjects (university staff, students, workers) listened to a recording of a radio drama. After listening they completed a questionnaire with questions such as: "Can you visualize these events now?" (mostly "yes") or "Do you like or dislike the Cockney accent in general?" (mostly: "dislike"). In the second, major study nine very different speakers (such as a minister, a daughter of the author, an actor) read a short text on a skating incident. The reading was done in the Manchester radio station and broadcasted by the BBC. This broadcasting of nine times the same text, over three days, had been announced in the *Radio Times*, which contained a questionnaire to be completed and sent to the radio station. Almost 4000 listeners responded. The short questionnaire asked the respondent to judge the gender, age, and profession of the speaker, as well as locality of birth and "locality affecting speech." A final question was "Is the speaker accustomed to lead others?" The speaker's sex was mostly not hard to judge from the voice, except many listeners mistook the young girl's voice for a boy's. The strongest finding was the quite accurate estimations of the speakers' ages ($r = 0.91$).[42] Judgments of occupation were also often quite close as were the guesses with respect to the locality of the speaker's accent. See also Pear's (1932) keynote lecture on the same topic.

Pear's pioneering study was almost exactly replicated by Herta Herzog (1933) from Bühler's laboratory in Vienna. Using the studio facilities of the Viennese radio broadcast,

[42] My computation, the author provides no statistics.

she had nine different speakers read a message about a lost police dog. The nine speakers were quite diverse, including a priest, a female stenographer, a driver, an academic, a teacher, boy and girl high school students, a teacher, an innkeeper, and a university lecturer. The local newspaper announced these broadcasts, with a questionnaire that could be completed and sent to the broadcasting company. The yield in Vienna was 2700 responses. The questionnaire required ratings on Pear's chosen features (sex, age, profession, "accustomed to giving commands") and in addition asked about the size and "thickness" (i.e., fatness) of the speakers, as well as judgments about the pleasingness of the voice. Respondents also reported their own sex, profession, and age; they could also add further personal impressions. The results were similar to what Pear had found. Sex could be perfectly judged, except for the two young high school students. The judgments of age correlated significantly with the real age of the speakers ($r = 0.75$),[43] with a strong tendency "to the middle." But size was apparently harder to evaluate.[44] This was surprisingly better for "thickness" or fatness ($r = 0.61$, marginally significant). Herzog used as her measure for fatness the speaker's weight divided by size, kilos per linear meter. The respondents were also good at telling the manual workers from the intellectuals. There was no clear pattern in the "commanding" feature, but women were generally judged to be more sympathic.[45] The author also checked how these judgments depended on the characteristics of the respondents. Very little came out of this, and the same can be said about the introspectionist protocols that some of the respondents and the author herself (at an earlier occasion) had produced. It did, however, provide an impressive number of Würzburgian protocols, completing the paper.

Harold Taylor (1934), a student of Sapir's, concentrated on the auditory transmission of core personality traits, as measured by standard personality scales. The 20 speakers, who ranged widely on a neuroticism scale, completed the questionnaires and recorded the test text on a Speakophone aluminum disk. Each of these records was played to 20 auditors, who judged the speaker by completing the same scales. The sad outcome of this experiment was that, although the auditors agreed significantly in their judgments, there was negligible correlation with the speakers' own ratings.

Luckily, Allport and Cantril (1934) could offer more hope. They again replicated Pear's and Herzog's studies, this time in Boston with 18 different speakers and several personality items in the questionnaire. There were 600 judges. Again sex, age, and vocation could be reliably judged. The same held for "political preference," "dominant values," and "extraversion," on which the speakers had been pre-tested. Another pretest had been on a scale of "ascendence-submission." This was also picked up by the judges, apparently responding to the "forceful, aggressive voice" (p. 45) of some of the speakers. "Summary sketches" of the speakers' personalities also showed reliable correspondences with the

[43] My computation, the author is in fact rather negative about the correlation. She provided tables, but like Pear no statistics. The Americans, such as Wendell Johnson's team, were much ahead here.

[44] A non-significant $r = 0.41$.

[45] Not noticed by the author.

speaker tests. The statistical design of this American study was noticeably more sophisticated than that of the Pear and Herzog studies from Europe. It signals a general pattern.

But none of these studies asked the obvious question: What is it in the voice signal or the articulation that carries the transmitted information about personality? Indeed, the remark on "forceful, aggressive voice" was a lonely exception. Here Michael and Crawford (1927) had done better. A single judge (probably one of the authors) rated 500-word passages of text as spoken by 56 students on a variety of speech qualities. One was labeled "inflection," which stands for adequacy and economy of pitch movements. Others were "rate of phrasing" and "enunciation," i.e., distinctness of consonant and vowel articulation. These are the types of features studied and trained in departments of speech communication. For all 56 speakers measures of intelligence and scholarly achievement were available. It turned out that the triple "inflection," "intelligence," and "scholarship" was highly inter-correlated. If a trained listener can pick up such an intelligence-correlated feature of voice, it must somehow be in the signal.

Jon Eisenson (1907–2001) wrote his elementary and quite readable text *The psychology of speech* (1938) especially for students and workers in speech communication. It contains a major chapter on personality and speech, which is, however, not in touch with the literature discussed. It rather deals with the speech of different clinical groups. Another chapter deals with psychological aspects of rhetoric and how to approach the audience.

Reading

Edmund Huey's (1908) *The psychology and pedagogy of reading* is the one classic text on the subject. In his preface Huey states "Ten years has given a development here of which experimental psychology may be proud." After the pioneering studies of Javal, Cattell, Erdman, Dodge, and especially Huey himself, reading research had become a booming field. Still, the great promise was not redeemed in the years to follow. Rayner and Pollatsek (1989) wrote; "In essence, work on the cognitive processes associated with reading came to a standstill in the 1920s and did not begin again until the 1960s." This diagnosis is harsh, but by and large correct, as we will see. After a short review of Huey's book, we will consider the ongoing German research into the 1920s, which almost exclusively used the tachistoscope as the experimental tool. We will then turn to eye tracking, which became the dominant experimental methodology in American studies of reading. A single, very different study, however, became the most cited psycholinguistic paper on reading ever: J. Rigid Stroop's.

Edmund Huey's text

Readability of a text was one of Edmund Huey's main concerns as an applied psychologist. Not afraid of speculation, Huey, in his closing chapter, urged psychologists, linguists, and educators to research daring proposals for the improvement of printed communication. Why use the antiquated, highly confusable letter symbols of our alphabet, where we could design much better distinguishable characters? Why print text in horizontal lines, whereas printing successive words in columns has major advantages for

economizing on eye movements and on eliminating interference from adjacent lines? Why put things in words, when pictures are much more informative? And yes, why print at all when technology provides us with the means of transmitting information by way of recorded speech? Huey's textbook is exemplary in terms of readability. It is a contagious homage to the history and art of reading, an invitation to theorize about the mental processes involved in reading, a practical guide for applying such insights to the teaching of reading. And it displays "the art of plain talk," as Rudolph Flesch (1946) would call this writing style.[46]

The book is in four parts, the largest of which is Part I on the psychology of reading, our main concern here. It begins by reviewing "the work of the eye" in reading, the alternation between fast saccades and reading pauses, as already discussed in Chapter 5. Quite recently, Dearborn (1906) of Columbia University had added one further major eye movement study of reading, using Dodge's and Cline's (1901) method of recording (p. 465 here). He discovered that the more pauses made scanning a line, the shorter their average duration. In addition and within limits, shorter lines induce shorter fixation pauses, which is of practical use in designing column width in newspapers or books. But Huey rejected Dearborn's claim that the eye is psychologically blind during saccades; some information is still getting through.

In further chapters, Huey discusses the limits of attention in registering visual elements during a single fixation. We can simultaneously perceive many organized wholes during a fixation, such as letters or even meaningful words or short phrases. But what is it, visually, that makes a word a recognizable whole? Huey carefully reviews the recent German work on this issue (see next section). He then turns to the process and function of inner speech in reading. Purely visual reading is possible, but inner speaking is a constituent part of reading for most people. Try to whistle during reading; it will slow you down. A major function of inner speech is the grasping of larger linguistic units. The memory span for speech is far larger than for printed words, according to Huey. "It is of the greatest service to the reader or listener that at each moment a considerable amount of what is being read should hang suspended in the primary memory of the inner speech." It allows attention to freely "wander backward and forward to get a fuller meaning where it is needed. . . . the main focus of attention is often far behind the eye." (p. 148). This causes the often substantial eye–voice span in reading aloud. Grasping the meaning of what is read is sometimes accompanied by mental images, but that is inessential. The thinking goes primarily in terms of inner speech; usually there are no images involved. Here Huey makes reference to James, Binet, and Ribot; his book appeared simultaneously with the early Würzburg publications on imageless thought. In a final chapter, Huey discusses his own and Dearborn's experiments on reading rate. Rates differ substantially between educated adults. Each has his own habitual reading rhythm. But speeding up

[46] Flesch himself became a passionate advocate of phonic, as opposed to whole-word, methods in reading instruction. See Flesch (1955).

can be trained, the experiments show. This is of great practical importance for reading education.

Tachistoscopic studies

The end-of-century boom of reading studies in Germany, with pioneers such as Cattell, Erdmann, and Dodge, faded out during World War I. But during these final years there was still a concerted effort to sort out the determinants of visual word perception. The issues were rather well-defined. First, is the word perception process simultaneous or successive? Remember Grashey's (1885) strictly successive theory of letter-by-letter conversion to speech sounds and Wernicke's elated adoption of it. Second, what is the size of the attentional span in word perception, or how many of a word's (or non-word's) letters can be grasped in a single glance, a single tachistoscopic flash? Third, to what extent and in what way is the recognition of a word dependent on the perceptual identification of its letters? Fourth, to what extent are the global properties of the word, such as its optical shape, relevant for its perception? Fifth, what is the role of the word's sound form activation, the inner speech, in word recognition? We will take these issues up in turn.

Erdmann (1900) once more rejects Grashey's theory. The "sound word" cannot be composed by successively sounding its successive individual letters. They do not represent the acoustic transitions and modulations required for sounding the whole word. Zeitler (1900), however, in an extensive review, defends successivity in visual word perception. As a Wundt student, he strongly criticizes Erdmann's theory of simultaneous word perception. In word perception there is always focused apperception and a wider field of view. The attentional focus in word perception is on a word's "determining letters." This idea had been introduced by Goldschneider and Müller (1893) and Zeitler further specifies it as the set of marked *ascending* and *descending* letters. Hence, *l, j, b, g* can be determining letters, but *n, v,* or any of the vowels cannot. These letters form a "relief" and focused attention rapidly jumps from peak and trough to peak and trough. This is the core process of apperception in word reading. This also holds for rapid tachistoscopic perception of the word, where eye movements are impossible. In addition, there is assimilation. It fills in material between the marked letters in familiar words. It also provides a "dark" impression of the global word form. It is this impression which is experienced as simultaneous. Zeitler's long critical essay does not provide a single experiment of his own. He was probably the last representative of the successive attentional view of word perception. Becher (1904), in an equally extensive critical essay, ridicules Zeitler's fast successiveness: "The assumption, monstrous in itself, of a manifold attentional shift within 1/10th of a second is also incompatible with the experimental data."[47] He also deals convincingly with Wundt's attack, defended by Zeitler, on Erdmann and Dodge's procedures.

[47] "Die an sich monströse Annahme eines mehrfachen Aufmerksamkeitswechsels in 1/10 Sekunde ist also auch mit den Versuchsergebnissen unvereinbar." (Becher 1904, p. 50).

Psychologists kept measuring the number of letters you can grasp in a single tachistoscopic presentation. Here obvious variables were the meaningfulness of the letter string (words versus non-words), the number of syllables, word shape and length, and the word's familiarity. The number of correctly reported letters ranged from 4 to over 25. Most authors, such as Wirth (1902), Messmer (1904), Kutzner (1913), and Henning (1917), explain this variation in terms of a rather small set of four to seven "objectively" recognized letters and a variable larger set of seen and "interpreted" letters. The size of the latter set depends on the aforementioned variables. Wagner (1918) explicitly states that in his experiments with meaningful and meaningless letter strings the number of really seen and identified letters is the same in the two cases. But how can you know other than from the introspections of trained participants? Korte (1923), not using a tachistoscope but a projection screen, deduces from his experiments that Erdmann and Dodge were right: more letters are explicitly identified in meaningful letter strings. He showed that meaningful words can be identified at a greater distance from the participant than meaningless ones with the same number of letters. Wagner had induced his subjects to go for letters, not meanings, Korte presumes.

How do identified letters contribute to word recognition? Zeitler's idea that ascenders and descenders play a special role in word recognition is shared by Messmer (1904), but Wagner (1918) disagrees. His experiments show that the width of a letter, as in *w* or *m*, can be as conspicuous as its vertical extension. Measurements of letter conspicuity or identifiability were published by Korte (1923). He presented tables for all lower and upper case letters in Roman and (German) *Fraktur* script. Wide letters *w* and *m* fared quite well indeed. Height and width are, obviously, *features* of letters. Henning (1917) and Schumann (1920) argued that *any* optical features of the visual word can contribute to its recognition. They had in mind shapes larger than letters, i.e., clusters of letters or the word's whole form, which is our next topic. It is quite surprising that neither letter nor word frequency was a matter of much concern in these experiments. The excellent tables for German were available, but Kaeding (1898) is never cited. This had to wait for the work of Howes' (1951) doctoral dissertation.

More attention went to the global properties of words. *Ganzheit* and *Gestalt* were increasingly important notions in German psychology. Erdmann had already stressed the importance of the whole form (*Gesamtform*) of the word for recognition. Just as we recognize a letter from the optical configuration of its features, we recognize a word by the optical configuration of its letters, according to Erdmann. And this is a *simultaneous* event. Wirth (1902) is one of the first to mention the word's *Gestalt* quality. Messmer (1904) defines the word's "whole character" as a black horizontal strip with sticking out vertical stripes. Kutzner (1916) tries to quantify this by computing the ratio of ascending and descending letters to total number of letters in a word. The higher the ratio, the more "landscape," he assumed, which is of course not entirely correct. The spatial distribution of ascenders and descenders must play a role as well, he concedes himself. But how much does a word's whole form contribute to its recognition? Here controversy reigned. Wiegand (1908) performed a masking experiment to find out. The observer would see two words, in quick succession, the second one masking the first one. The second word

was always easily recognized, but the subjects' task was to guess the first, masked word. Their (faulty) guesses were apparently largely based on just a few correctly recognized letters, never on the word's whole form (length, for instance, was not preserved in the erroneous responses). Wagner (1908) decided to ruin the whole word form by presenting his words in capitals. Recognition was just as good, he claimed, as for the lower case words. But Kutzner (1916) found a substantial advantage for lower case words. According to him, for familiar words, the whole shape triggers activation of the word's sound form. Grossart (1921) presented words at a great distance from the observer. Even when they could not discriminate any letters, they would occasionally recognize the word, which must have been mediated by the whole form. Similarly, Heller (1911) projected words in the periphery of vision, using a tachistoscope. Here only the whole form could be effective, but now word recognition was all but absent. There is one non-tachistoscopic American study, related to this category, by Hollingworth (1935) of Columbia University. He does not make a single reference to the research discussed here, but decided to test whether a word's *Gestalt* prevents it from being decomposed. He instructed his subjects to compose an animal name (such as *badger*) from a sequence of letters. The sequence could consist of (other) words (such as *rag bed*), or of senseless letter strings (such as *rga bde*). If words are *Gestalts*, the former condition should be harder, because Gestalts must be broken up. However, it was not. Hollingworth concluded that both words and letter strings are just letter configurations, but not *Gestalten,* subject to principles such as closure. One late German experiment in this area deserves special mention. Stein (1928) studied word recognition under temporal conditions in which the whole word, the *Gesamtform*, was never in view. This was achieved by moving, from left to right, a narrow window over the word. Word recognition was hampered this way, but still possible. Two further conditions Stein used led to an interesting discovery. In one, the movement over the word was made from right to left. Here recognition was just as good as for the left-to-right direction. Most naive subjects in this experiment had not even noticed the difference with the other condition. According to Stein, they perceived the same *Gestalt* quality, residing somewhere behind the screen. This, however, did not occur in the third condition. Stein now fixed the vertical window and slid the word behind it. Here, word recognition was completely blocked and no whole, moving *Gestalt* appeared at all. It foreshadowed modern studies in rapid serial visual presentation.

Most people read with active inner speech. Kern (1929) showed that over 70% of 9–11-year-olds made overt lip movements during silent reading. We saw that Huey considered inner speech to be quite useful in reading, because it extends the window of working memory. Judd, however, found the use of inner speech detrimental; it slows down reading and it should be trained away. This is in agreement with Kern's finding that silent articulation in reading correlates negatively with intelligence. Meumann (1914, p. 441) conjectured that our access to a visual word's meaning was always mediated by the inner word sound, be it shortened and ephemeral. Schumann (1920) explains Wiegand's masking results in the same way. Just a few letters of the masked word manage to activate a whole word sound. That becomes the word guessed/reported by the subject. For example, the masked word *Landungsbrücke* ("landing stage") is perceived as *Lunge* ("lung").

But Schumann adds that in normal, unmasked visual word recognition, there is normally a tacit control process, which verifies whether the word sound matches the optical word image. The word sound activates "residual" optical word features in memory that should match the presented visual word. Also Kutzner (1916) argued for the mediating role of the sound image. The "whole image" of a familiar visual word activates its sound image. That, in turn, may raise consciousness of the individual letters.

These were the five major issues in the rounding up of the German work in visual word recognition. Jakob Hoffmann (1926) provides a more detailed review for the interested reader.

Eye-tracking studies

Huey (1900) had been the pioneer in the technology of recording eye movements during reading. His technique was to attach a recording device to the anaesthesized cornea. This was somewhat cumbersome and also affected the angular accelerations of the moving eye. Soon after, ever inventive Raymond Dodge, then at Wesleyan University, improved on this by introducing the corneal reflex method of eye movement recording (Dodge and Cline 1901). Nothing was attached to the eye. Instead the apparatus recorded on a moving film the horizontal movements of an eccentric white vertical line, as reflected by the subject's cornea. Dearborn, a former student of Dodge, used this method for his (1906) doctoral thesis on reading. The Columbia University thesis largely confirmed Huey's earlier findings, but added several more. Subjects differed in "motor habits"; each had a preferred rate and duration of fixations, making an almost constant number of fixations per line of text. The number of fixations decreased with the familiarity of the text. There is no evidence whatsoever of words being progressively fixated letter by letter (as had been claimed by Grashey). Such small symbol-by-symbol fixations, if they exist, can be recorded by the corneal reflex method, because they do emerge in the reading of Arabic numerals. Instead, words are perceived as wholes. That also holds for short, familiar phrases, which only need a single fixation.

Eye-tracking studies of reading, with their complicated technology, remained, with one exception, an American specialty. After Dearborn's dissertation, the work was first concentrated in Chicago and Stanford, and then found its home at the University of Minnesota. The work was suffused with the same spirit as the American studies in language acquisition. The core issue was the measurement of reading ability, in particular reading rate. Standardized reading tests were developed for this purpose, such as the Iowa Silent Reading test, the Chapman–Cook Speed of Reading Test and the Tinker Speed of Reading Test. They were used in both mainstream and special needs teaching. The eye-tracking studies mostly served three purposes. The first was to secure statistical norms and reliability measures for parameters such as fixation duration and frequency during the reading of texts varying in difficulty. The second was to determine the validity of such parameters as measures of reading skill, in particular reading rate. The third purpose was to determine the legibility of print, as a function of typography, line length, line spacing and so on. The complicated equipment was rarely used to decompose the reading process as it goes from measurable fixation patterns to the comprehension of text. There was no effort to develop

models of reading; the work was entirely a-theoretical. In other words, it hardly contributed to the psycholinguistics of reading. That relieves us from the task of reviewing it in detail. Suffice it to mention the main developments and the occasional interesting finding.

In Chicago, Wundt's student Charles Hubbard Judd (1873–1946) led a team in educational psychology dedicated to the psychology and teaching of reading. Their eye-tracking studies, Judd (1918) claimed, proved the superiority of silent reading over overt reading. In overt reading speech governs the reading process, which makes it slow and cumbersome. Silent reading is visually controlled and much faster. Team members Schmidt (1917), Gray (1917), and Buswell (1921) used the corneal reflex method to study the development of reading ability.[48] They specialized in measuring the readers' eye–voice span. Buswell (1921), for instance, measured an obvious relation between eye–voice span and reading ability, but also showed the eye–voice span shrinking from the beginning to the end of a sentence, not from the beginning to the end of a line: "the eye–voice span is determined by thought units rather than by printed line units" (p. 222). He also constructed texts with ambiguous words in such a way that they would be disambiguated at different delays. In *She had tears in her dress and also tears in her eyes,* a three-word eye–voice span would be minimally needed to grasp the two meanings of *tears.* Subjects with short eye–voice spans showed relatively more backtracking around such words, whether in loud or silent reading. The two text books by O'Brien (1921, 1926) are excellent reviews of the Chicago team's work and are also practical sources for teachers of reading.

At Stanford, studies by Miles, Shen, and Tinker also used Dodge's corneal reflex method, but now further developed to record either horizontal or vertical eye movements. Miles and Shen (1925) used it to analyze the reading of Chinese. That allowed them to compare horizontal and vertical reading. They found somewhat longer fixation durations in vertical than in horizontal reading, but each pause in the vertical reading covers a greater number of words. The result is a slight advantage for vertical reading, a result Huey would have liked. In a comparison of reading English and (horizontal) Chinese, the authors found that for Chinese reading pauses are shorter and more frequent than for English. But more words are read per pause in Chinese than in English, with the outcome that Chinese is read faster than English. There were, however, no serious controls for the balance of language skills in the bilingual subjects.

Tinker (1928a) measured the ratio of eye movement duration to fixation duration. His measurements confirmed the earlier findings of Erdmann, Dodge, and Huey, a ratio of about 1 to 20. The ratio decreased with increasing difficulty of the text, because fixation durations increased. Tinker (1928b) also recorded fixation patterns while subjects were solving simple problems, such as *If a boy can run 6 miles per hour, how far does he run in 14 hours?* When the numbers appeared as words (*six, fourteen*) instead of Arabic numerals, the subjects made more fixations and were slower.

After these initial studies, Miles Tinker (1893–1977) moved to the University of Minnesota, where he built a well-equipped laboratory for the study of reading. Over the following three decades, his team put out a stream of publications on eye movements

[48] See Judd *et al.* (1905).

and reading skills.[49] Tinker published, between 1927 and 1936, a series of five reviews of reading studies, not only his own, in the *Psychological Bulletin*.[50] Tinker (1951) provides a concise review of the measurement of pause and movement durations in reading. The eye movement/fixation ratios, and their covariation with text difficulty, had been reconfirmed time and again. Much of Tinker's work was reviewed in his 1965 text *Bases for effective reading*. In 1932 Tinker had published the list of Wundt's PhD students; he was no doubt familiar with reading research in Wundt's laboratory. But his own papers reflect very little of the original ambitions to understand the mental processes involved in reading. In retrospect, the most important contribution from Tinker's laboratory was Katherine Preston's (1935) discovery of the word frequency effect.

In Europe, Magdalena Dorothea Vernon (1901–1991), at the Psychological Laboratory of Cambridge University, constructed a variant of Dodge's corneal reflex apparatus (Vernon 1930). However, it was not much used, except in her (1931b) study of proofreading. Her book *The experimental study of reading* (1931a) presented an excellent review of both the tachistoscopic and eye-tracking studies of reading. Vernon, later professor of psychology at Reading University, became the British leading expert in the study of reading difficulties in children. Her 1971 book *Reading and its difficulties: a psychological study* is her ultimate review of this field.

The Stroop paradigm

The impact of one single study in reading was going to overshadow all the others reviewed here. It was John Ridley Stroop's 1933 dissertation at Peabody College in Nashville, now Vanderbilt University. In 1935 it was published, hardly modified, as a journal paper. Stroop had set out to study experimentally the mechanisms of interference and inhibition. This was an active topic in (mostly) American psychology. Münsterberg (1892), for instance, had observed inhibiting effects among his daily habits, such as opening a door or taking a watch out of his pocket. Interference would depend on the amount of practice, the association strength between a stimulus and the habitual response to it. Some, such as Garrett and Lemon (1924) and Ligon (1932), had approached the issue by studying interference in naming different colors. How strong are the associations between color and color name? Stroop had replicated Cattell's (1886) original finding that naming color words was faster than naming colors. Is the difference due to strength of association? Not so, according to Brown (1915). If the association between color and spoken name is weaker than between color word and spoken name, the former should profit more from intensive practice than the latter. But a well-designed training experiment ruled this out. The gain in response time from practice was the same for color naming and color word naming. It barely helped to print the initial of the color name, or even the full color name, in the color chip; color naming remained relatively slow. The underlying processes must be distinct, Brown concluded. Cattell had already suggested a crucial difference. In color

[49] Wright-Sutherland (1988) published most of Tinker's bibliography.

[50] See, for instance, Tinker (1936), the last one in the series.

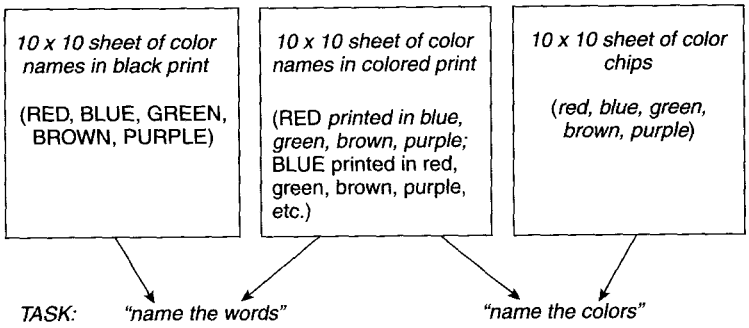


Fig. 12.11 Stroop's experimental design. Subjects named as quickly as possible all 100 items on a test sheet. There were two control conditions: the left test sheet contains color words printed in black, just five different ones, and the right test sheet contains color chips, again just five different ones. The two test conditions made use of the middle sheet. Here color words (the same five different ones) were printed in color, always in a color they did not name. In the one test condition these words should be named, just like the black words on the left sheet. In the other test condition the colors should be mentioned, just as the chips on the right sheet.

naming the route is from color to concept to spoken word, whereas in color word naming it is directly from printed word to spoken name.

Stroop's brilliant experimental innovation was to study naming interference by what we now know as the Stroop paradigm. This makes it possible to let the word naming process interfere with the color naming process. Stroop's experimental design is shown in Figure 12.11 and explained in the legend.

Stroop found that it took his subjects 41 seconds to quickly read the square of 100 black color names. It took them 63 seconds to name 100 color chips, substantially longer, confirming Cattell's and Brown's findings. It required 43 seconds to read the 100 colored color words, only slightly longer than reading the black ones. But the classical finding was that it took them 110 seconds to name the colors of the colored words, much longer (74%) than naming those words. Stroop concluded that the word–name associations are much more effective than the color–name associations. Stroop concluded his study with a practice experiment, somewhat like Brown's. Practicing the naming of the color chips over eight sessions led to a 14% speeding up of the naming responses. But when subjects practiced the highly interfering task of naming the colors of color words, the gain was 35%. Hence, it is possible to reduce the interference by practice, but at a cost. When Stroop's subjects directly after this practice had to *read* the colored words (which had been quite easy before), they slowed down by 90%. But that effect was not long-lived; by the next day it was largely gone.

The enormous potential of Stroop's paradigm for the study of attention and interference in general and for the psycholinguistic study of naming[51] in particular was not immediately apparent. George Miller, for instance, always perceptive to innovative moves, did not mention Stroop's paradigm in his 1951 text. And John Ridley Stroop (1897–1973)

[51] See MacLeod (1991b) and Roelofs (2003) for reviews of experiment and theory in the enormous Stroop literature.

himself was not helpful either. His dissertation was essentially his last major contribution to psychology. According to MacLeod (1991a) and Logan (2008), Stroop devoted his further life to teaching bible study classes, writing religious books, and preaching in local Nashville. As a student he had built his own house there, where he stayed for the rest of his life.[52]

Retrospect

Developments in experimental psycholinguistics showed both losses and gains during the first half of the twentieth century. The most conspicuous loss was the general theoretical erosion. The great promise of theory-guided experimentation, as developed by founders, such as Donders, Ebbinghaus, Wundt, Cattell, Erdmann, Dodge, Bagley, and Huey, was not really redeemed. There was no lack of experimental work, but it reflected the growing aversion toward mental modeling. The mental process had stood in the heart of the German psycholinguistic tradition. The vitality of that tradition suffered severely as a consequence of World War I and disappeared entirely during the Nazi period. With the center of gravity of the field shifting to the United States, it became submitted to the tunnel vision of local behaviorism. Objectivism was the methodological maxim. Collecting data, preferably large-scale, establishing statistical norms, and designing tests of verbal and reading abilities was the honorable home work for the practical, applied psycholinguist. The loss of focus on the mental process explains the disappearance of mental chronometry in psycholinguistic experimentation. It also explains the total eclipse of speech-error analysis. Meringer-style mental modeling of the speaking process had become an anathema. Even Stroop's brilliant paradigm remained without impact till long after the cognitive revolution.

However, modern psycholinguistics cannot be imagined without considering the gains made during this remarkable transition period. The standards of experimental design were greatly raised. The longish introspectionist, theory-ridden protocols of "trained" subjects disappeared from the scene. Factorial experimental design with appropriate statistical analysis became the norm. Zipf's influence is still felt in the statistical modeling of text data, which has now become indispensable in psycholinguistics. But the influence of Shannon and Weaver's information theory is unmatched. It is to George Miller's lasting credit to have introduced the theory into the experimental analysis of verbal behavior. We will focus on this feat in Chapter 15. The effects of frequency, transitional probability, the number of alternatives, and entropy are unavoidable in modern psycholinguistics, either to be studied or to be controlled for. Howes' dissertation on frequency effects in word recognition was the first in the never-ending research tradition, just as, much later, Oldfield and Wingfield's (1965) study would become for word production research. Finally, the period under concern left us with remarkable evergreens of experimental psycholinguistics, such as Stroop's paradigm and Lee's delayed auditory feedback.

[52] Both MacLeod and Logan came in contact with Stroop's son Fred, who still lived in his father's house. Stroop's last paper in psychology was published in 1938.

Chapter 13

A new cross-linguistic perspective and linguistic relativity

> We know of no people that is not possessed of a fully developed language. The lowliest South African Bushman speaks in the forms of a rich symbolic system that is in essence perfectly comparable to the speech of the cultivated Frenchman . . . Many primitive languages have a formal richness, a latent luxuriousness of expression, that eclipses anything known to the languages of modern civilization. (Sapir 1921, p. 22).

Sapir's statement followed Wundt's (1920) intellectual testament by just one year, which claims that the traces of language and thought evolution, from primitive to highly cultivated, are still accessible in the languages of the world. "Here the psychologically most important data are there for the taking, leaving for psychology only the main task of subsuming them under the sum total of psychological laws of development."[1] And "the treasure is hidden which is to be unearthed, if we are to acquire a true psychological developmental history of the more complex processes of thought."[2] In 1920 Wundt's credo was still the genetic, evolutionary perspective on language and thought that had been characteristic of most of nineteenth-century language research, but it had become much more sophisticated. Wundt had rejected Humboldt's and Steinthal's view that languages evolve in a hierarchy from "isolating" to "agglutinative" to "inflectional," instead arguing that there are many more features with which languages can evolve and they make different "choices." As a consequence, every language "represents its own characteristic organization of human thought."[3] Nevertheless, existing languages reflect different evolutionary states, not biologically but psychologically. They are to be studied comparatively "for the taking", and thus reveal the laws of mental development. One should study non-Indo-European languages, which are less highly developed. The languages of Hottentots, Bushmen, Mande-Negroes, and the Papuas, for instance, are quite low-level according to Wundt, because they entirely miss case relations.

[1] "Hier sind vielfach die psychologisch wichtigsten Ergebnisse geradezu mit Händen zu greifen, für welche dann der Psychologie nur die Einordnung in die Gesamtheit der psychologischen Entwicklungsgesetze als die Hauptfrage übrig bleibt." (Wundt 1920, p. 213).

[2] "Gerade hier, auf dem Gebiet der Sprache, liegt aber, . . ., der Schatz verborgen, der gehoben werden muß, wenn wir in den Besitz einer wahren psychologischen Entwicklungsgeschichte der zusammengesetzteren Vorgänge des Denkens gelangen sollen." (ibid. p. 217).

[3] "jede Sprache repräsentiere eine nur ihr eigentümliche Gestaltung des menschlichen Denkens." (ibid. p. 215).

In this chapter we will contrast Wundt's, and the more general nineteenth-century, "vertical" perspective, with the newly evolving "horizontal" perspective expressed in the quotation from Sapir. On this view, the world's living languages cannot be arranged on an evolutionary scale ranging from simple or elementary to complex and sophisticated. They are just different. The hot psycholinguistic issue then becomes whether their users are, correspondingly, cognitively different, not "more" or "less." Is it indeed the case that language is a "prepared road or groove" for thought (Sapir 1921, p. 14)? It is the issue of "linguistic relativity."

We will first consider "verticalism" and some of its ramifications. Verticalism did not entirely disappear during the period we are considering, emerging time and again in various guises. We will turn then to the pioneers of "horizontalism," Boas, Sapir, Lee, Whorf, and others. Horizontalism gave rise, not logically but factually, to the notion of linguistic relativity. We will consider some of its stronger and weaker versions, which were initially entirely untested. Only during the 1950s were the first psycholinguistic experiments designed to test (versions) of linguistic relativity. The emergence of this new, important paradigm will be the closing topic of this chapter.

Verticalism

Wundt's belief that the history of language and thought, from primitive to sophisticated, is still at play and accessible to comparative research, was still widely accepted during the entire ninteenth century. Humboldt, like Wundt, had no doubts about the hierarchy of languages, from the "languages of the savage" all the way up to Greek at the zenith. The historical reconstruction required comparative research, beyond the limits of Indo-European languages, which became a project Humboldt essentially initiated. In this context, the writings of anthropologist Joseph-Marie Degérando, whom we met in Chapter 4, are remarkable. For the Napoleonic Baudin expedition to Australian waters (1800–1803) Degérando composed an anthropological field guide so detailed and well-conceived that it stayed in use for over a century.[4] The explorer must learn the language of the savage people he is studying. "The best means of understanding savages is to become in a manner of speaking like one among them," i.e., through participant observation. As an enlightened philosopher, Degérando assumed human nature to be universal. But peoples are in different states of development. "The *voyageur-philosophe* who sails towards the extremities of the earth traverses in effect the sequence of the ages; he travels in the past; each step he makes is a century over which he leaps."[5] This was still Wundt's belief some 120 years later. It was also an invitation to what we would now call "foreign development": "what more touching purpose than to reestablish the holy knots of universal society, than to meet again these ancient parents separated by a long exile from the rest of the common

[4] Degérando (1801). Citation from Stocking (1968, pp. 26–27).

[5] Ibid.

family, than to extend the hand by which they will raise themselves to a more happy state."[6]

During the course of the nineteenth century, verticalism merged with theories of biological evolution. Are races accidental variations on a monogenic base or are they, rather, polygenic with limited interracial fertility? Both Broca and Darwin were inclined to the polygenic view, a view which raised the question of different races having different "ceilings" on their developmental potential; see Stocking (1968) for an excellent review of these issues. The relevant observation here is that the vertical perspective became deeply entrenched in psychology, including the psychology of language. Haeckel's biogenetic law linked the development of the species to the ontology of the child, and also to the laws of mental development. As Taine (1877) put it: "the child presents in a passing state the mental characteristics that are found in a fixed state in primitive civilisations."[7] It was especially Darwin's young friend George Romanes who worked this out in his *Mental evolution in man* (1889, p. 327): "To use biological terms, this the earliest evolution of speech has been greatly foreshortened in the ontogeny of mankind, as compared with what it appears to have been in phylogeny."

Other connections were also made. In aphasiology authors stressed levels of severity as a dominant dimension in the typology of aphasias. Among them were Bastian and Freud, and especially Hughlings Jackson, who, as we saw, explicitly linked the severity dimension to Spencer's evolutionary notion of "dissolution." In dissolution, later-acquired volitional functions of mind give way to the older, more primitive automatic processes. Isserlin, as was cited in Chapter 11, quite explicitly called the telegraphic speech of agrammatics: "the sentence form of sign language, it is the form of expression of primitives, deaf mutes, children at certain developmental stages and normal people in particular states of need." Deaf sign language was almost universally characterized as occupying a low or basic rank in the language hierarchy. Friedenberger, we saw in Chapter 4, likened it to the incomplete language of "savage Africans." Indeed, "Deaf-mutes also make themselves easily understood with savages," according to Kussmaul (1877, p. 5). Wundt was much more subtle in characterizing the grammar of sign language, but for him also it occupied a lowly place in the language hierarchy.

The vertical, hierarchical view, linking the language of "savages," children, deaf people, people with aphasia, farmers,[8] women,[9] and the "mentally disordered", lingered during the period we are considering in this chapter. William White (1925) put it this way in his address to the American Psychiatric Association: "Just let me indicate along this line some of the things philology suggests as being of significance: We know that the distinction or one of the distinctions between the child and the adult; one of the distinctions between

[6] Stocking 1968, p. 27.

[7] Taine (1877, p. 259).

[8] "history of civilization teaches, that up to a certain point savages and barbarians are like what our ancestors were and our peasants still are" Tylor (1881, p. 75).

[9] "Man höre einmal naiven Personen und Frauen zu." (Wegener 1885, p. 162). See also Thomas's (1907) early discussion of this issue: "The mind of women and the lower races."

the defective and the person not defective; one of the distinctions between the savage and the civilized, is that in each instance the more highly developed individual tends in his thinking to be more abstract and less concrete; tends in his thinking to differentiate unrelated and to relate analogous situations more accurately than do those of less development. Take, for example, in the language of the savage, the expression meaning, 'I am washing my face.' Here is a definite sentence composed of several words. Vary it a bit and say, 'she is washing her face,' and they have to use an entirely different sentence made up of entirely different words. Here is a primitive language situation which corresponds precisely to the manner of language of the more primitive types of people." (pp. 665–666). The sophisticated audience applauded White's presentation, as one of them put it: "I wish we had at least a dozen more talks by Dr. White, who presented to us a viewpoint which clearly explains the fundamental workings of the mind." (p. 670).

New versions of verticalism were formulated by Roman Jakobson (1941) and Heinz Werner (1940). Jakobson, we saw, compared the child's acquisition of phonological distinctions to the aphasic's gradual loss of these distinctions. A language's distinctive phonological contrasts are hierarchically organized. Whereas the child is "climbing" the hierarchy, the aphasic is descending that same hierarchy. This is a respectable empirical hypothesis. It is also a carefully restricted hypothesis. Jakobson would be the last to claim that existing "primitive" languages range along the same phonological hierarchy.

Heinz Werner, in his often reprinted *Comparative psychology of mental development* (1940), took the wider perspective. One might even call it the last comprehensive formulation of verticalism in psychology, including the psychology of language. In 1933 Werner, like the Sterns, was dismissed from the University of Hamburg and moved via Holland to the United States. He wrote his book while at the Wayne State County Training School in Michigan. His empirical work there focused on mental development and learning disabilities. The stated aim of the book was to compare mental development across different fields, in particular child psychology, ethnopsychology, and psychopathology, with occasional outings to animal psychology, and to establish "developmental levels" and "their genetic relationships" (p. 24).

Werner intended to establish a "principle of parallelism, development in mental life follows certain general and formal rules" (p. 26). Earlier or lower levels, for instance, would exhibit concrete and magical thinking in both children and "primitive peoples." Indeed, "There are cases of a pathological primitivation of language (. . .) known as aphasia which, so far as formal characteristics are concerned, bring the language very close to that concrete language found among primitives." (p. 34). Werner was not claiming that the history of human language and thought is still "at play" in the "primitive peoples" of this world. You cannot travel to meet your ancestor. But you can find different levels of development in the languages of the world. These are not different states of some underlying development, instead they are *specialized* states. They are, as it were, premature forms of specialization. This "all too perfect adjustment is the sign of a lower form of behavior, rather than of an advanced. A primitive, highly balanced, 'one-track' culture lacks that friction between individual and environment, that flexibility and freedom in unceasing attempt to readjust, which is the very life and essence of higher,

advanced cultures." (p. 19). Still, the formal properties of different levels of mental development can be observed "live" in existing cultures and peoples. Werner explicitly distances himself from the "horizontal" perspective: "Another misinterpretation centers about the concept of *primitive mentality*. Some anthropologists claim that there is no essential difference between civilized and "primitive" man. The difference, such as it is, lies only in the cultural sets; the one people lives in an advanced culture, the other one in a primitive one, but the mental functions of the individual are fundamentally the same." However, it is misconceived "to reduce differences in mentality to differences in culture." That is entirely circular, because culture is only "the objective aspect of this mentality."

How does Werner apply these conceptions to psycholinguistic levels of development? Primitive levels of development are characterized by *physiognomic* and *concrete* naming. For both "primitives" and children the word is a property of the thing, like any other quality. That is the basis of all word magic. There is no arbitrariness in naming. This appears both from neologisms in children that are strongly *physiognomic* with lots of sound symbolism, and from the "sound-images" (now called "ideophones") that are freely created in certain African languages. Werner adds further examples from pathology to complete his case. At the basis of reference in language is, at least during development, some degree of similarity between word and referent. In a later book, Werner and Kaplan (1963) work this out as a general theory of symbol formation, which was devastatingly reviewed by Fodor (1964).

Naming is also *concrete* at lower levels of development, according to Werner. "Names are above all individual names" in primitive cultures (p. 267). Here Werner produces the traditional list of examples: the Bakaïri of Brazil having lots of names for different types of parrots, but no generic name "parrot," the Laps having 41 words for different kinds of snow, but without a generic term for snow, the Tamo having words for going north, south, east, or west, but having no word for "to go," etc. Why is this? Here is Werner's explanation: "The most important function of language among primitives is to describe, to copy reality, whereas in advanced cultures it is to serve as an instrument of judgment." (p. 268). The Ponko Indian does not say "The man killed the rabbit," but rather "The man, he, one, having a soul, standing, deliberately killed, having shot an arrow, the rabbit . . . him, the one, having a soul, sitting down." Copying reality in every detail is, apparently, the sad fate of the speaking "savage." We will not further follow Werner in his ruminations, but rather turn to the "horizontal" perspective.

Horizontalism

Arthur Hocart

Some three decades earlier, Arthur Hocart (1883–1939) had considered the same issue: "The doctrine seems fairly well established among comparative psychologists that savage languages are encumbered with useless distinctions which civilised man has learnt to discard and that they are poor in general concepts and rich in many subdivisions of the species, dearth of ideas and superfluity of words are their failing." It is as if answering Werner: "Even eminent psychologists have fallen into this pitfall from which few

pioneers escape." (Hocart 1912, p. 267). In fact, he was addressing Professor Stout's *Analytic psychology*.[10] "I give them a caution," he added, "and advise them to desist from the psychological interpretation of language unless they are conversant with life and language of the people." Hocart was himself conversant with the life and language of the Fijians in Melanesia. In his paper he turns the tables and lets a native Fijian complain about the lack of abstraction in English. The Fijian language, for instance, has a generic term (*rai*) which is a root in all words that express the concept of sight, whereas English uses a different term for *to see, to seem, to look at, to show*, etc. English makes minute distinctions, where Fijian simply uses a generic term. In Fijian there is one abstract term *tá* for *to chop, hew, fell, hack, notch, slash*, but English, it is said, has a general term too: *to cut*. "Has it? Does a sculptor cut wood, a surgeon cut his patient, or a shepherd cut wool?" (p. 277). If a particular term applies, the general one often does not. Hocart then argues that all languages work on the same principle: "Is it not the rule always to use the lowest possible genus, and does not insipidity result from the use of a general term when there is a more particular one? Good prose is to a great extent the use of the exact word, and the exact word is the narrowest that will fit." (p. 275). This is Grice's maxim of quantity *avant la date*.

Franz Boas

There is no disagreement about the crucial role Franz Boas played in replacing the "vertical," evolutionary notion of human culture (singular) by the "horizontal" perspective on human cultures (plural). Boas (1858–1942) studied chemistry and physics in Heidelberg and Bonn, taking his PhD in Kiel with a dissertation on the perceived color of water. Boas's main interest was in geography, but his broad academic training had also made him conversant with Fechner's psychophysics (he cannot have missed Fechner in Heidelberg), with Kantian philosophy, with Wundtian psychophysiology, and with Darwinian theory. In addition, Boas took a post-doctoral post with Virchow in Berlin to study anthropometrics. This led to Boas's participation in the German polar research expedition to Baffinland (1883–1884), from which he returned in 1885, after a longish stay in New York—his first effort to find a job in the United States. Scientifically, Boas had set out to study the geographical, environmental determinants of human conduct, but his first experiences in participant observation among the Eskimos raised his interest in ethnology and culture. Not being able to find a decent job in Germany, Boas joined a Canadian expedition in the Hudson Bay (1886), after which he stayed in America. After a further range of temporary jobs and short expeditions, Boas got, in 1896, a position at the *American Museum of Natural History* in New York. From there, he organized a four-year systematic exploration of the coast peoples of the North Pacific and Siberia. In 1899 Boas was appointed professor of anthropology at Columbia University, a position he would hold till his death in 1942.

[10] Hocart refers to G.F. Stout (1902), Vol. 2, p. 231.

Boas was among the first to argue against the notion of a hierarchy of languages. A first, technical paper on this issue was Boas (1889), *On alternating sounds*. Here Boas addressed the widely held belief that primitive languages lacked a fixed phonetic repertoire. Their sounds "fluctuate" and "alternate." They have a vagueness that is absent from more advanced languages. Brinton had expressed this in his 1888 address to the American Association for the Advancement of Science (AAAS). Here is how he states it in his 1890 book (p. 393): "In all European languages, the mere letters of the alphabet, by themselves, have no meaning and convey no idea; furthermore, their value in a word is fixed; and, thirdly, arranged in a word, they are sufficient to convey its sound and sense to one aquainted with their values. Judged by certain American examples, all three of these seemingly fundamental characteristics of the phonetic elements were absent in primitive speech . . . We find tongues in which the primary sounds are themselves significant, and yet at the same time highly variable; and we find many examples in which they are inadequate to convey the sense of the articulate sound."

The empirical argument was that such fluctuations appear from the field notes of vocabulary collectors. Their transcriptions vary between collectors, between speakers, and even between different occasions with the same informant. Boas does not deny this. On the contrary, he provided examples of his own alternating transcriptions, such as *Operníving*, *Upernívik*, and *Uperdnívik* for the same Eskimo word. But how does this variability arise? Here Boas applies his Fechnerian knowledge of differential thresholds. You perceive a stimulus in terms of similar stimuli you have classified before. "It is well-known that many languages lack a term for green. If we show an individual speaking such a language a series of green worsteds, he will call part of them yellow, another part blue, the limit of both divisions being doubtful." (p. 50). Similarly, philologists are at a loss for a speech sound that figures somewhere between the sounds of their own languages. A philologist "perceives the unknown sounds by the means of the sounds of his own language." (p. 51). The "alternating" spellings "are due to a wrong apperception, which is due to the phonetic system of our native language. For this reason, I maintain that their occurrence is in no way a sign of primitiveness of the speech in which they are said to occur; that alternating sounds are in reality alternating perceptions of one and the same sound." (p. 52).

In 1911 Boas published the *Handbook of American Indian languages*, an almost 1000 page volume, a project of the Bureau of American Ethnology. The ten chapters treated ten American-Indian languages, with Boas himself taking care of Tsimshian, Kwakiutl, Chinook, and, together with Swanton, Siouan. In an extensive introductory chapter, Boas outlines his views on race, language, and culture, arguing that the three are essentially independent. A language can spread over quite different cultures. The members of a race, however defined, usually speak many different languages and display many different cultures. Relations between race, culture, and language are historical accidents, not the necessary result of some natural law. Boas then provides a treatise on linguistic structure, discussing in detail how languages can vary in their phonetics, morphology, syntax, lexicon, and grammatical categories. The traditional notion of a hierarchy of languages is totally absent from this chapter. Michelson, another member of the Bureau,

puts it this way: "There is no such thing as superiority in language. They all possess the necessary machinery to express the most complicated ideas. . . . Nor is the language of primitive peoples indicative of low mentality." [11]

Rather, Boas adds an interesting discussion of relations between language and thought. Here he addresses the same issue of "concreteness" in American-Indian language use that Hocart had discussed for Fijian. And his response is similar, although less scintillating. The lack of expression of "generalized ideas . . . is due to the lack of their need. Primitive man, when conversing with his fellowman is not in the habit of discussing abstract ideas." (p. 64). He then discusses cases from his personal experience, where Kwakiutl speakers spontaneously produce generalized expressions (without suffixing for place etc.), if that is useful in the conversation. Indeed, "the device for generalized expression is present, but the opportunity for its application arises seldom, or perhaps never. I think what is true in these cases is true of the structure of every single language." (p. 65). So, can a language be an impediment to thought? Here Boas expresses a position which is far from the linguistic relativism we will consider: "Thus it would seem that the obstacles to generalized thought inherent in the form of a language are of minor importance only, and that presumably the language alone would not prevent a people from advancing to more generalized forms of thinking if the general state of their culture should require expression of such thought; that under these conditions the language would be moulded rather by the cultural state. It does not seem likely, therefore, that there is any direct relation between the culture of a tribe and the language they speak, except in so far as the form of the language will be moulded by the state of culture, but not in so far as a certain state of culture is conditioned by morphological traits of the language." (p. 67). It should be added that Boas was very well aware of the fact that languages classify experience in quite different ways and that speakers have to deal with it: "it will be recognized that in each language only a part of the complete concept that we have in mind is expressed, and that each language has a peculiar tendency to select this or that aspect of the mental image which is conveyed by the expression of thought" (Boas 1911a, p. 43). Only by the end of his life, after Sapir and in particular Whorf had made strong claims of linguistic relativity, was Boas willing to consider the issue, although most cautiously: "There is little doubt that thought is thus directed in various channels . . . . Such a tendency pervading the language may well lead to a different reaction to the incidents of everyday life and it is conceivable that in this sense the mental activities of a people may be in part directed by language. I should not be inclined to overestimate this influence because devices for expressing . . . [various ideas] are ever-present, and may rise into idiomatic use. In this sense, we may say that language exerts a limited influence on culture." (Boas 1942).[12]

Boas is often identified with "relativism," but his is not linguistic, but rather cultural relativism. This is a trivial position if it means that what people think or believe is co-determined by their culture. It is less trivial if it is given the ethical implication that

[11] Michelson (1917, p. 234).

[12] As cited by Lucy (1992, p. 15).

moral values lack universality, being relative to the culture in which they figure. Boas was certainly inclined to this view, being a staunch defender of non-interference in other cultures. But this is not an issue for the present chapter.

Instead we should discuss Boas's views on the cultural stages of development. Is it justifiable to assume "that the present races of man may be considered as standing on different stages of the evolutionary series"? (Boas 1911b, p. 104). In other words, can you "meet your ancestors" in remote areas of this world? Boas rejects the notion of parallelism in development, one race or society being at a different stage to another. By considering crucial steps in technical development, such as creating pottery, agriculture, and metallurgy, Boas shows that such achievements by no means follow a fixed order. "A comparison of their languages, customs and activities suggests that their faculties may be unequally developed; but the differences are not sufficient to justify us to ascribe materially lower stages to some peoples, and higher stages to others. The conclusions reached from these considerations are therefore, on the whole, negative. We are not inclined to consider the mental organization of different races of man as differing in fundamental points." (p. 123). Notice, that this was written almost a decade before Wundt's ultimate "vertical" testament. The laws of mental growth, in both thought and language, are regrettably not "for the taking," in Boas's view, by voyaging the world.

Boas's impact on the course of modern anthropology was as much due to his own writings, as to the excellent students who received his thorough training at Columbia. Among them was Alfred Kroeber, the first person to receive a PhD in anthropology there, in 1901. In 1916 Kroeber published a study of the speech of a Zuñi child, 23–24 months old, a primer in linguistic anthropology. Over two months, the boy increased his vocabulary from 18 to some 30 words. He produced all five Zuñi vowels, but used a fairly limited range of consonants, evading palatal stops and glottalized sounds (as most infants do). However, nearly every syllable ended in a glottal stop. Kroeber noticed the child adopting characteristic Zuñi intonation of syllables and multisyllabic words. The boy, finally, produced one real sentence: *mle'mle'po*, meaning "the American is smoking." Among Boas's students was also Edward Sapir, who became the leading anthropological linguist and a philosopher of linguistic relativism.

Edward Sapir and linguistic relativism

Edward Sapir (1884–1939) was born in Lauenberg, now Poland, of Lithuanian Jewish parents, with Yiddish as his native language. The family moved to England when Edward was three or four and to the United States when he was six. He grew up in New York and received all of his academic education (linguistics, music, anthropology) at Columbia University. Boas closely supervised Sapir's graduate training as an "American Indian linguist," granting him his doctoral degree in 1907.[13] Sapir then became involved with some

[13] Boas's training was rather teutonic. The relations with his students always kept a formal, German touch. I went over the extensive, life-long correspondence between Boas and Sapir, in the library of the American Philosophical Society. The letters deal almost exclusively with administrative and organizational matters and the two authors continued to address one another as "Dr. Boas" and "Dr. Sapir."

of Boas's field projects, which was only the beginning of a life-long occupation as a field linguist in a wide variety of American-Indian sites. Sapir became the first director of the Anthropological Division of the Canadian government, at the tender age of 26. After many highly productive years there, Sapir became disenchanted with his position in Ottawa and after several speculative applications moved to the University of Chicago. There he would spend the next six years in the joint Department of Sociology and Anthropology, from 1927 on as a research professor. In the same year Leonard Bloomfield returned to Chicago from Columbus, Ohio. Both of them had been founders of the Linguistic Society of America in 1925. The two great men never became friends, but their professional relations were adequate and helpful. Sapir moved to his final academic position at Yale in 1931, where he became head of the newly established Department of Anthropology, but his teaching was largely in the Linguistics Department, where Sturtevant was one of his colleagues. The department boomed, becoming home to young linguists such as Swadesh, Hockett, Trager, and, of course, Benjamin Whorf. One of Sapir's courses was in the psychology of language. Sapir kept a deep interest in relations between language, culture, and personality. Sapir died from a wasting heart condition in 1939.

Sapir was pre-eminently an ethnolinguist. His ongoing field work is reflected in publications on some 30 different American-Indian languages. Early in his career, Sapir noticed the amateurish linguistics of most field anthropologists and decided to write a basic, readable book on the nature of language. It became his *Language. An introduction to the study of speech* (1921). It is indeed an attractive non-technical introduction to language and its use in its larger cultural context. The book was not an immediate success, but ultimately became a classic text. The citation opening the present chapter is characteristic for the position Sapir takes in this book. It is the "horizontal" perspective, considering all languages as fully fledged, complex systems for the expression of thought. He explicitly rejects the traditional evolutionary, hierarchical classifications of the world's languages. That is a "fruitless undertaking," Sapir states. "It is probably the most powerful deterrent of all to clear thinking. This is the evolutionary prejudice which instilled itself into the social sciences towards the middle of the last century and which is only now beginning to abate its tyrannical hold on our mind." (p. 130). He then ridicules the often expressed belief that Sanskrit, Greek, Latin, and German "are the 'highest' development that speech had yet attained and that all other types were but steps on the way to this beloved 'inflective' type." (p. 131). In a footnote he cites a "celebrated American writer" who "delivered himself of the dictum that, estimable as the speakers of agglutinative languages might be, it was nevertheless a crime for an inflecting woman to marry an agglutinating man." (p. 131). Of course, Sapir does not deny the stunning variation of morphologies in natural languages, but he introduces a refreshing generative, algebraic account of them (pp. 29–32).

Language also presents Sapir's first unsystematic thoughts on linguistic relativity. Discussing relations between culture and language, Sapir writes: "Culture may be defined as *what* a society does and thinks. Language is a particular *how* of thought." (p. 233). But the two are causally unrelated: "we shall do well to hold the drifts of language and of

culture to be non-comparable and unrelated processes. From this it follows that all attempts to connect particular types of linguistic morphology with certain correlated stages of cultural development are vain. When it comes to linguistic form, Plato walks with the Macedonian swineherd, Confucius with the head-hunting savage of Assam." (p. 234). But then, two pages further we read: "Languages are more to us than systems of thought transference. They are invisible garments that drape themselves about our spirit and give a predetermined form to all its symbolic expression." Still, it took a few years before Sapir began working out the idea that these garments also *shape* the spirit to some extent.

At the basis of all further discussion of linguistic relativity, whether Sapir's, Whorf's, or Carroll's, is the comparative linguistic observation that the same type of experience is expressed differently in different languages. How do you communicate that you saw a stone "moving through space towards the earth"? Sapir (1924)[14] works this out by first mentioning the naïve theory that the viewer makes a quick inventory of all the elements of the experience (such as "stone," "fall," and "earth") and then goes for the words that express these elements. It would not matter much whether you speak English, German, French, or Kwakiutl. But it does. The French speaker expresses that the stone is feminine, the German speaker that it is masculine, the Kwakiutl speaker that it is visible, the English speaker none of these. Where the English speaker says "the stone falls," a Nootka speaker talks about a stony movement, something like "it stones down," And so on. "It would be possible," Sapir then adds, "to go on indefinitely with such examples of incommensurable analyses of experience in different languages. The upshot of it all would be to make very real to us a kind of relativity that is generally hidden from us by our naïve acceptance of fixed habits of speech as guides to an objective understanding of the nature of experience. This is the relativity of concepts or, as it might be called, the relativity of the form of thought." (p. 176).

In 1926 Sapir added two further considerations to this undeniable type of linguistic observation. The first one is completeness of each language as an expressive tool. According to Sapir "there is no known language, whether spoken by a culturally advanced group or by one of the more backward peoples of the world, which has not its perfectly defined phonetic system, its clearly developed and often very complex system of forms, and its established rules and idioms of usage. In other words, there is no such thing as a language that has not its well-defined grammar."[15] This is not only a clear statement of "horizontalism," but also of all languages being complete and independent *systems* of communication. Languages are "systems of behavior that are rather definitely marked off from all other organizations within the general range of conduct." (p. 422). Languages do not incidentally tune the expression of experience one way or another, but as complete, independent, self-contained systems they express experience continuously and in an ordered fashion. Sapir's second consideration is that we are normally not conscious of

[14] Reprinted in *The collected works of Sapir*, Vol. I. The reference is to pp. 174–176.

[15] Also in *The collected works*, Vol. I (pp. 421–422).

this perspective-taking: "it still remains characteristic of language as a whole that of all the great systems of social patterning it is probably the one which is most definitely unconscious in its operation." (p. 423). For civilized and primitive man alike, the "marvelous formal intricacy" of languages works underground.

Sapir (1929b) makes the claim that this unconscious system of expressing experience is in fact a way of viewing the world: "The fact of the matter is that the 'real world' is to a large extent unconsciously built up on the language habits of the group. No two languages are ever sufficiently similar to be considered as representing the same social reality." (p. 209). And this is not limited to the social world alone. "Even comparatively simple acts of perception are very much more at the mercy of the social patterns called words than we might suppose."(p. 210). Hence, language "*is a symbolic guide to culture.*" (p. 210).

The most explicit statement of Sapir's relativism appears in 1931: "Categories such as number, gender, case, tense, mood, voice, 'aspect' and a host of others not recognized systematically in our Indo-European languages, are, of course, derivative of experience at last analysis, but, once abstracted from experience, they are systematically elaborated in language and are not so much discovered in experience as imposed upon it because of the tyrannical hold that linguistic form has upon our orientation in the world. Inasmuch as languages differ very widely in their systematization of fundamental concepts, they tend to be only loosely equivalent to each other as symbolic devices." (p. 578).

The development in Sapir's thought nicely lays out the "levels" of relativism one might conjecture. First, and non-controversial, is the fact that the same type of experience is often expressed differently in different languages. From the standpoint of the speaker, this is what Slobin (1987) was to call "thinking-for-speaking." If a language, such as English, requires the expression of tense, the speaker must consider the temporal perspective of the event to be expressed, even if that is completely irrelevant for what needs to be communicated. Second, and certainly controversial, is the claim that certain concepts are language-relative. Languages differ in the way they analyze experience conceptually. Third, and most controversial, is the thesis that *all* experience is affected by our linguistic habits. Language exerts a "tyrannical hold" on both our perceptual and conceptual framing of the world. A language imposes a world view. Notice that this third level incorporates both the perceptual and the conceptual world of experience.

Sapir never made a clear choice among these levels, but his sympathy was with the second level, the idea that languages differ in certain ways of conceptualizing experience. Increasingly, however, Sapir sympathized with the all-out, "tyrannical" version, language imposing a "world view" on all levels of experience. Weisgeber was the staunchest defender of level 3 linguistic relativity.

Whorf concentrated on level 1 and 2 relativity, and almost fully rejected the level 3 version. In particular, he excluded perceptual experience from linguistics relativity. However, Brown, Lenneberg, and their followers who, in the 1950s, developed the experimental approach to linguistic relativity, were testing level 3 relativism with respect to visual perception. It was still called the Whorfian, or worse Sapir–Whorf hypothesis, a label which led to much misunderstanding. Let us now turn to these further developments.

The world view approach and linguistic relativism

Wilhelm Humboldt, we saw in Chapter 2, formulated a moderate form of linguistic relativity. It was nothing new in that era to believe that nations and peoples differ in their *Weltanschauung*, their world views. Language can play a role in this, according to Humboldt. It does not impose a world view, but it can invite particular ways of thinking: "It is not what can be expressed in a language which determines its advantages or deficiencies, but rather that which it incites and inspires." We also found the *Volksgeist*, the collective mind at work in Jacob Grimm's explanation of Germanic sound change. Here, the language was affected by the "violent progress and craving for freedom," the current world view. The collective mind (*Volksgeist*) was also at the center of the Lazarus and Steinthal programmatic *Völkerpsychologie*, ethnic psychology, in which language and thought are inextricably bound. The evolutionary hierarchy of languages corresponded closely to a hierarchy of intellectual functioning, of civilization, and *Weltanschauung*, the "vertical" view for short.

Still, one rarely comes across texts proclaiming language to be a "cage for thought." Ludwig Wittgenstein (1921) came quite close when he wrote: "The limits of my language mean the limits of my world. All I know is what I have words for."[16] But Wittgenstein's linguistic determinism did not have a comparative language dimension. He was not interested in the variety of world views embodied in different languages. The idea of linguistic determinism was around in Europe, independent of the Sapir–Whorf developments in the United States. Authors such as Lipps, Cassirer, and Trier expressed similar ideas, but its main representative was Leo Weisgerber.

Johann Leo Weisgerber

Born in what still was German Metz, Leo Weisgerber (1899–1985) served during World War I as a soldier at the German front in Flanders. He then studied linguistics in Bonn, specializing in Celtic. At the age of 27 he became professor of comparative linguistics and Sanskrit at the University of Rostock. In 1942 he moved to the University of Bonn, where he taught till the end of his professional career. Most relevant for our present discussion is Weisgerber's book *Muttersprache und Geistesbildung* ("Mother tongue and mental education") of 1929. Its core theme is the relevance of language for thought. Fully admitting the communicative functions of language, Weisgerber focuses on the other main function or achievement of language: to provide "the conceptual representation of things"[17] In a first analysis, Weisgerber refers to Gelb and Goldstein's studies of amnesic patients, which we discussed in Chapter 12. The patients had to sort colored threads of wool, putting like colors together. This non-linguistic task is quite easy for people without disabilities, but not for these patients. Weisgerber's interpretation, following the authors, was that the sensory impressions were the same for patients and people without disabilities, but patients had lost their color *concepts*. They could not put "red" with "red" or "blue"

[16] Wittgenstein (1921), *Proposition 5.6.*

[17] "die begriffliche Auffassung der Dinge." (Weisgerber 1929, p. 18).

with "blue" due to a loss of these linguistic categories. And indeed, these patients were also unable to *name* these colors. In other words, the color name is not associated with a sensory impression, but with a color concept.

Weisgerber also criticizes (on p. 24) Clara and William Stern, who attribute the late acquisition of (appropriate) color naming in children to a lack of interest and attention on the part of the child. Rather, the child's difficulty is to form the quite abstract concepts of "red," "blue," "brown," and so on. There is hardly an independent, objective basis for these concepts. It is only through *language* acquisition that these concepts are formed. And what is true for the acquisition of color concepts also holds for the acquisition of other concepts. Between the word and the referent is the concept. What the child acquires and the adult uses is a "linguistic conceptual world"[18] This conceptual world is not only based on the lexicon of the mother tongue, but also on its grammar; Weisgerber refers to Ach's and Selz's notion of sentence schema. It is entirely due to this language-based conceptual common ground that mutual understanding is possible in a language community.

After providing this psychological base, Weisgerber turns to the language comparative perspective. Here the ultimate question is "to what extent the typical quality of a people is created by its language."[19] Weisgerber considers various conceptual systems, showing their great variety over the languages of the world. In particular, he examines number systems, kinship terms, color term systems, verbs of motion, prepositional systems, and more. Here, Weisgerber was doubtless inspired by Lévy-Brühl's (1910) book *Les fonctions mentales dans les sociétés inférieures* (later translated as *How natives think*), to which he refers. Lévy-Brühl had argued that primitive thought is "concrete" and "mystical," and so is primitive language. American-Indian and Melanesian languages, for instance, often show a proliferation of plurals, concretely indicating singulars, duals, and trials. The use of an abstract plural is much harder. Weisgerber in particular aligns with Lévy-Brühl on "primitive" number systems. There are languages that have different number systems for people, trees, houses, and other categories. For kinship systems, Weisgerber argues that they are often not "natural," they do not reflect true biological relationships. Rather it is the kinship terminology that imposes the kinship categories. Although Weisgerber's approach is mostly well-taken, he clearly lacks the hands-on experience, which is so characteristic of Sapir's and Whorf's work. Without any sign of hesitation, Weisgerber presents the case of an unnamed "negro tribe," "from which it is mentioned, that it possesses 500–800 color terms for tinges of brown."[20] His conclusion from this comparative review is that "the essence of language variety" is the variety in "conceptual structure of the languages."[21] Using Humboldt's terminology, Weisgerber calls this a variety of "internal

[18] "dieser Aufbau der sprachlichen Begriffswelt" (Weisgerber 1929, p. 29).

[19] "wieweit die Eigenart eines Volkes durch seine Sprache geschaffen ist." (ibid. p. 100).

[20] "bis zu dem Negerstamm, von dem berichtet wird, daß er 500–800 Farbworte für braune Farbtöne besitzt." (ibid. p. 83).

[21] "So sehen wir also (...) das Wesentliche der Sprachverschiedenheit darin, daß der inhaltliche Aufbau der Sprachen ein verschiedener ist." (ibid. p. 85).

speech form" (*innere Sprachform*). The task of comparative linguistics is to research "the structure and the characteristics of the world views"[22] expressed in different languages. The world view idea kept coming up in Germany. Hans Lipps (1936), for instance, wrote about the difference in conceptual systems between languages: "The 'partitioning' of different languages media is different."[23] This is followed by a Humboldt citation on differences in implicated world views.

Towards the end of his book, Weisgerber proclaims not just linguistic relativity, but full linguistic determinism. The child grows up in the "mental world of the mother tongue."[24] Indeed, "that we think in *such* a way as we do, can only be understood from the mother tongue."[25] Here Weisgerber refers to a similar statement by Lipps: "The individual remains trapped in the captivity of what is understood in terms of the vocabulary of his language."[26] These language tools deeply affect human thought and action: "after language acquisition, it is not man who thinks, but the mother tongue for him."[27] This is an omen indeed: "In this sense the mother tongue is fate for the individual, the language of a people fatal power for the community."[28]

After World War II, Weisgerber (1950) returned to the *Weltanschauung* embodied in the German language. He repeated many of his 1929 arguments and pleaded for the comparative approach, but without applying it himself. He also splendidly ignored all comparative work in the Sapir–Whorf tradition. German linguistics had lost contact with the wider world.

Benjamin Whorf, self-taught linguist

In 1918 Benjamin Whorf (1897–1941, Fig. 13.1) obtained a bachelor degree in chemical engineering at MIT. Shortly afterwards he was employed by the Hartford Fire Insurance Company, where he specialized in fire prevention engineering, a job he kept for the next 22 years till his untimely death in 1941. As a linguist, Whorf was entirely self-taught, at least until Sapir moved to Yale in 1931. There, Whorf enrolled in Sapir's class on American-Indian linguistics. Whorf had always been an avid reader. As a child he had been interested in Maya culture and fascinated by Maya hieroglyphs. As a young man he studied

[22] "Sprachvergleich ist demnach das Mittel, den Aufbau und die Eigenart des Weltbildes der verschiedenen Sprachen zu erforschen." (Weisgerber 1929, p. 155).

[23] "Die 'Brechung' der Medien der einzelnen Sprachen ist verschieden." (Lipps 1936/1951, p. 92).

[24] "so müssen wir auch die kindliche Spracherlernung einschätzen als Hineinwachsen in diese muttersprachliche Geisteswelt." (Weisgerber 1929, p. 119).

[25] "aber daß wir *so* denken, wie wir es tun, das kann nur von der Muttersprache aus verstanden werden." (p. 125).

[26] "Der einzelne bleibt befangen in dem Bann dessen, was in dem vokabularen Bestand seiner Sprache als 'begriffen' enthalten ist." (p. 125).

[27] "daß nach der Spracherlernung nicht mehr der Mensch denke, sondern die Muttersprache für ihn." (p. 121).

[28] "In diesem Sinne ist die Muttersprache Schicksal für den Einzelnen, die Sprache des Volkes Schicksalsmacht für die Gemeinschaft." (p. 164).

Fig. 13.1 Benjamin Whorf. © Whorf, Benjamin Lee, Pps Manuscripts & Archives, Yale University.

Hebrew as a quest for biblical meaning, which for him was often at odds with modern science. Quite accidentally he came across an 1815/1816 book on Hebrew by Antoine Fabre d'Olivet. It proposed that the triliteral Hebrew roots had an underlying semantics, derived from the inherent meanings of their constitutive letters. This secret code idea struck Whorf's imagination and never fully left him. Time and again, Whorf would write that linguistics is essentially "a quest for meaning," deciphering the often covert, underlying codes of a language.

At Hartford, Whorf could, after office hours, frequent the Watkinson library, which was well-stocked in American-Indian languages. His first writings were on Aztec and Maya scripts. He even managed to spend a few months on a field trip to Mexico, supported by the Social Science Research Council. But from 1931 on, under Sapir's wise guidance, Whorf involved himself with mainstream American-Indian language research. Sapir put him on the study of Hopi, with access to a native Hopi speaker, and provided him with limited field work in Arizona. Whorf became something of an expert on Hopi, publishing some detailed, technical papers on Hopi grammar. He spent about a decade, always part-time, with Sapir's productive team at Yale, which involved linguists such as Newman, Trager, Swadesh, Haas, and Voegelin. Whorf was much esteemed among these colleagues. He was an open-minded, unselfish, and brilliant intellectual partner. When Sapir died in 1939, Whorf was recovering from his recent cancer operation. These depressing circumstances did not prevent him from almost undiminished writing and publishing for both the professional and the general public, till his death two years later at the age of 44.

Whorf's "horizontalism"

Whorf, as it were, completed the change of perspective towards "horizontalism." He had major misgivings about authors, such as Lévy-Bruhl, who lump together all "so-called

primitive peoples."[29] They do not constitute a unit class of mentality, and this is taken further when "primitive" is equated with "infantile," which Freud and Jung[30] do. "Hence, for the concept of a 'primitive mentality' we should substitute that of 'cultural mentalities.' What has been mostly meant by 'primitive mentality' is 'any cultural mentality other than SAE cultural mentality,'"[31] where SAE stands for standard average European (next section). Biology, Whorf argues, developed a sophisticated, world-wide taxonomy before conceiving of evolution theory. Linguistics, most regrettably, followed the reverse order. "The evolutionary concept was dumped upon modern man while his notions of language and thought were based on knowledge of only a few types out of the hundreds of very diverse linguistic types existing, has abetted his provincial prejudices and fostered grandiose hokum that this type of thinking and the few European tongues on which it is based represent the culmination and flower of the evolution of language! . . . The eminence of our European tongues and thinking habits proceeds from nothing more. On the contrary, it takes little real scientific study of preliterate languages, especially those of America, to show how much more precise and finely elaborated is the system of relationships in many such tongues than in ours. By comparison with many American languages, the formal systematization of ideas in English, German, French, or Italian seems poor and jejune." (pp. 84–85). This is verticalism in reverse, almost.

Whorf on linguistic relativism

Whorf's major paper on linguistic relativism is his contribution to the Sapir memorial volume, edited by Leslie Spier. Whorf wrote this paper, entitled "The relation of habitual thought and behavior to language," in 1939. It opens with a long citation of Sapir himself on relativism. Here is part of it: "The fact of the matter is that the 'real world' is to a large extent unconsciously built up on the language habits of the group . . . We see and hear and otherwise experience very largely as we do because the language habits of our community predispose certain choices of interpretation." (p. 134). While Sapir never became very specific on these matters, Whorf went into great detail. His approach was to compare Hopi, the American-Indian language he had mastered so well, to SAE, comprising languages such as English, French, and German, which, in comparison to Hopi, are sufficiently alike to be lumped together. Whorf's purpose here, as in all his other writings on relativity, was to demonstrate that the "conceptions" or "pictures of the universe," the world views of different cultures as coded in their languages, differ substantially. Conceptions of space, matter, time, and number are not universal. They differ, as appears

[29] Whorf (1956, p. 79). Here and in the following, the references are to pages in *Language, thought and reality*, John Carroll's (1956) edited volume of selective writings by Whorf. It also includes Carroll's equally excellent biography of Benjamin Whorf.

[30] Whorf occasionally cites Jung on his four basic psychic functions: sensation, feeling, thinking, and intuition. Freud he denotes as "an erratic genius with a faculty of apperceiving deep but obscure truths, and [his psychoanalysis] is notion obsessed and cluttered with weird dogma." (p. 42).

[31] This citation is from Whorf and Trager's so-called "Yale report" (p. 266), reprinted as an appendix in Lee (1996), which is the other major source on Whorf's (unpublished) writings.

from the habitual "fashions of speaking" about these matters, and they are often an integral part of habitual behavior patterns in those cultures.

Here are some of his analyses. On the conceptual categories of plurality and number, Whorf notices that Hopi cannot objectify time as SAE does. In Hopi, for instance, one cannot count and hence not pluralize days like objects. One cannot say "They stayed ten days," but only something equivalent to "They left after the tenth day." This is closely connected to the Hopi conception of time. Where SAE easily objectifies time as a countable series of temporal slots, such as hours, days, or weeks, Hopi only conceptualizes the perpetual "getting later" aspect of time. In SAE you can objectify or "spatialize" time by saying "in the morning," just like "in the house." That is not conceivable in Hopi. One will rather say something like "while the morning-phase is occurring." More generally, the deeply ingrained habit in SAE to use spatial metaphors for other domains is simply absent in Hopi. Tones are "high" or "low," we "grasp" an elusive idea, we even make the corresponding gestures. Hopi does not display such conceptual synesthesia. Rather, it has an elaborate system of "tensors," parts of speech expressing intensities, rates, repetitions, interruptions, etc. Another comparison Whorf works out concerns the concept of matter. SAE distinguishes count nouns from mass nouns. Count nouns denote individual bodies: "a tree," "a man," etc. Mass nouns denote continuous quantities: "water," "sand," etc. This conceptual distinction is often quite inconvenient, Whorf argues, because we do not often speak about unbounded quantities, such as air or rain. In most other cases we resort to binomial constructions: "a glass of water," "a heap of sand," container plus matter. And this we extend to binomials such as "a stick of wood," "a piece of cloth." In other words, we resort to a container metaphor when we speak about bounded quantities. This is entirely different in Hopi. It has no count/mass distinction. There is no specific reference to unbounded substances. You say "a water" as easily as "a tree." The binomial SAE construction is not present in Hopi. It has no "container-plus-substance" conceptualization of matter.

Whorf's main conclusion from these analyses is this: "Concepts of 'time' and 'matter' are not given in substantially the same form of experience to all men but depend upon the nature of the language or languages through the use of which they have been developed." (p. 158). In other words, there is a directed dependency of thought on language, at least in these higher level concepts. Psychologically, language and thought are inseparable. But language is the conservative system here. It is a "frame of consistency" which is adopted by its users. In his next paper Whorf put it this way: "We dissect nature along lines laid down by our native languages" and "We cut nature up, organize it into concepts, and ascribe significances as we do, largely because we are parties to an agreement to organize it in this way – an agreement that holds throughout our speech community and is codified in the patterns of our language." (pp. 212–213). This, indeed, is Whorf's principle of relativity, as he put it in 1940: "We are thus introduced to a new principle of relativity, which holds that all observers are not led by the same physical evidence to the same picture of the universe, unless their linguistic backgrounds are similar, or can in some way be calibrated."[32]

[32] Cited by Lee (1996, p. 86).

Did Whorf's relativism become deterministic in his final papers, as we saw in Weisgerber's final remarks a decade earlier? This conclusion seems warranted from this citation: "The agreement is, of course, an implicit and unstated one, BUT ITS TERMS ARE ABSOLUTELY OBLIGATORY; we cannot talk at all except by subscribing to the organization and classification of data which the agreement decrees." (pp. 213–214). Or was Whorf only saying that if a concept is encoded grammatically, we have no choice but to recurrently use it, i.e., nothing more than "thinking-for-speaking"? The next sentence, however, seems to implicate that freedom of thought is definitely illusionary: "This fact is very significant for modern science, for it means that no individual is free to describe nature with absolute impartiality but is constrained to certain modes of interpretation even while he thinks himself most free." (p. 214).

Whorf's universalism

Like Weisgerber, Whorf acknowledges the great variety in *conceptual structure* encoded in the languages of the world. More than anyone before him, Whorf stresses the endless variability in the categorizations different languages impose. In his 1937 paper on grammatical categories, Whorf essentially argues that there is not a single category that is universal to all languages. This even holds for the noun/verb distinction or more generally for subject–predicate organized syntactic structure. The power of predication is sometimes, as in the Nitinat language, an entirely word-internal affair. Still, the claim that different languages segment experience differently does not mean that all experience is relativistic, on the contrary. Whorf always claimed that there is a universal, non-linguistic or pre-linguistic experiential base from which all languages "take off." Here, Whorf was much inspired by Gestalt psychology: "A discovery made by modern configurative or Gestalt psychology gives us a canon of reference for all observers, irrespective of their languages or scientific jargons, by which to break down and describe all visually observable situations, and many other situations, too." (p. 163). It is in particular the perceptual figure-ground organization which obeys universal laws. All observers, for instance, will divide the appearance of a running boy "into (1) a figure or outline having more or less of motion (the boy) and (2) some kind of background or field against which, or in which, the figure is seen." (p. 163). Also: "Visual perception is the standard, norm, and framework of all experience. The forms and laws of visual perceptions are the same for all individuals." (Lee 1996, p. 107). This presumably also holds, *mutatis mutandis*, for non-visual experience. "unvisual experience is introjected and makes up what we shall call, following some Gestalt psychologists, *the ego* field, or *egoic field*." These are "non-linguistic and non-semantic classifications": "*An isolate experience in either the external or the egoic field, e.g., a shape or a noise, is not a meaning.*" (ibid. p. 107). The conceptual variability among languages should always be measured against this universal "canon of reference." Whorf is entirely consistent on this point, with maybe one exception. It concerns the perception of speech sounds. In an unpublished draft, discussed by Penny Lee (1996), Whorf stated that "phonemics is a relativity principle." (p. 88). We perceive the allophones of a phoneme as alike, in spite of huge acoustic differences. And what is alike for one language can be phonemically unlike, opposed in another language. (It was

Whorf, by the way, who introduced the useful notion of allophone. In English the initial sound of *lean* and the final sound of *cool* are allophones: very different, but perceived as the same phoneme *l*. In Russian they are perceived as different phonemes). This inconsistency may in fact be the reason why Whorf decided to exclude the paragraph from the ultimate, published paper.

Whorf and the public interest

Whorf was not a missionary, but he was certainly motivated to address a non-professional audience on matters of language and thought. In fact, the last four papers published during his lifetime dealt with these issues, in his always smooth style of writing. Three papers in the *Technological Review*, appearing in 1940–1941, presented, with well-chosen linguistic examples, the issue of linguistic relativity, the notion that the grammars of languages incorporate underlying "logics" that are different between languages, and the claim that linguistics is an exact science. The fourth paper appeared, in 1942, in the journal of the Theosophical Society of Madras, India, under the title "Language, mind and reality."

These papers were all well received, in particular the one on linguistics as an exact science. Here, one of Whorf's worked-out examples was the underlying phonological structure of the word. He showed how each language has its specific structural "word formula," which children pick up at a very young age and to which native speakers unconsciously adhere when they coin new words (such as *glub* or *squonk* in English). Almost the same example is worked out in the *Theosophist* paper, which also returns to the different "logics" involved in the grammars of different languages. Neither of these papers is missionary in tone, but it is clearly dear to Whorf's heart to express the function of linguistics in "raising linguistic awareness." As he expressed in a short note, it can contribute to realizing "the ideal of worldwide fraternity." Although the "West has attained some emotional understanding of the East," the "intellectual gulf" has not been bridged; "we are no nearer to understanding the types of logical thinking which are reflected in truly Eastern forms of scientific thought or analysis of nature. This requires linguistic research in the logics of native languages, and realization that they have equal scientific value with our own thinking habits." (p. 21).

Especially counterproductive here, according to Whorf in his "logics" paper, is the move to promote the use of Basic English as an international language.[33] "Basic English, with its concealed premises working harder than ever, is to be fobbed off on an unsuspecting world as the substance of pure Reason itself. . . . I believe that those who envision a future world speaking only one tongue, whether English, German, Russian, or any other, hold a misguided ideal and would do the evolution of the human mind the greatest disservice. Western culture has made, through language, a provisional analysis of reality and, without correctives, holds resolutely to that analysis as final. The only correctives lie in all those other tongues which by aeons of independent evolution have arrived at different, but equally logical, provisional analyses." (p. 244). Here Whorf not only kept at

[33] Basic English was proposed by C.K. Ogden (1930).

a distance from his teacher Sapir, promotor of the artificial language Interlingua, but also from Jespersen, a staunch promotor of Ido and Novial.

Clear language

Movements to "purify" language were everywhere during the 1920s and 1930s, not only in academic philosophy, but also in wider intellectual circles, with Korzybski's General Semantics movement as a prominent example. General Semanticists also pleaded for more linguistic awareness and they "adopted" Whorf during the 1940s. Whorf, however, kept them at a distance because they lacked all linguistic sophistication.[34] Before we turn to the further reception of Whorf's ideas, we will give some attention to these intellectual movements, which form, at best, a colorful chapter in folk psycholinguistics.

Lady Welby-Gregory

The Hon. Victoria, Lady Welby-Gregory (1873–1912), godchild of Queen Victoria, almost single-handedly and without previous formal education developed a philosophy of what she was to call "significs," the study of meaning in everyday language usage. All signs, including linguistic ones, acquire their values in the context of specific use. Here she carefully distinguished between *sense*, *meaning*, and *significance*. The sense of a sign, in particular of a word, is variable and depends on the current state of discourse, the referential domain, and the states of mind of the interlocutors. The meaning of a sign is determined by the speaker's (or writer's) intention. Paraphrasing is a means of expressing the same meaning in different words. Language is highly flexible, with the implicated risk of becoming ambiguous. The significance of a sign is its effect on the hearer (or reader), independent of the speaker's intention. It is the pragmatic consequence of the sign's expression, which often has an ethical dimension. Everyday language is pervaded by the use of figurative expressions, similes, and tropes. These are powerful rhetorical means, but also sources of the fallibility of language.

Lady Welby-Gregory became influential not only through her books (especially *Significs and language: The articulate form of our expressive and interpretive resources* of 1911) and articles in journals such as *Mind* and *Monist*, but also by her avid correspondence with scholars such as James, Peirce, Russell, and Ogden. She also entertained thinkers and writers in her mansion in Lincolnshire, for intensive discussion meetings.

Dutch Significa

Among Lady Welby-Gregory's frequent guests was the Dutch psychiatrist and writer Frederik van Eeden (1860–1932), who was the originator of the Dutch Significa movement. It involved the lawyer Jacob Israels-de Haan, the Amsterdam mathematician and philosopher Gerrit Mannoury (1867–1956) and through him the brilliant founder of intuitionistic mathematics and philosopher Luitzen "Bertus" Brouwer (1881–1966). At a later stage the linguist Jacques van Ginneken also became involved. We will not review

[34] Lee (1996, p. 16).

the tortuous course of Significa getting off the ground organizationally. Relevant for our present discussion is what Mannoury called the "significs relativity principle." The Significa movement was one of the earliest to consider the speech act nature of language and its members studied the establishment of mutual understanding. In any spoken communication there is the speaker's speech act and the hearer's speech act. The speaker meaning is everything that precedes/affects the speaker's act and the hearer's meaning is the effect on the hearer's mental state, expectations, and future acts. Speaker and hearer meanings are never identical, but relative to these contexts. It is, in particular, impossible to capture both speaker and hearer meanings under formal definitions. A speech act cannot be exhaustively analyzed in terms of its constituent linguistic signs. Rather, speech acts are conglomerates of psychological memories and experience. The ultimate analysis of speech acts is a chapter of psychology according to Mannoury, which will involve methods of introspection and experimentation. The Significa members, however, never managed to get such a research program off the ground. With the exception of the temporary and somewhat elusive member van Ginneken, they lacked any methodological sophistication in either psychology or linguistics. Still, it is interesting to notice, as Schmitz (1985) does in his authoritative history of the Significa movement, that Mannoury's distinction between emotional, volitional, and indicative aspects of *any* speech act essentially coincides with Bühler's three functions (cf. Chapter 9). The unavoidable relativity of meaning is also a reason for misunderstanding. Communication is normally achieved at a particular "language level." Language levels are defined by the social community involved with the speech act. At the scientific level, for instance, it is the indicative aspect of meaning which dominates the communication; it is the common ground within a scientific community. At the family level, the emotional aspect of meaning is far more at work in establishing understanding. Misunderstandings easily arise when levels (and thus implicit communities) are confused. There was regular discussion within Significa about measures to prevent such misunderstandings. Brouwer, for instance, proposed introducing new words that expressly (not implicitly) denote basic emotional meanings and values.

General Semantics

A really great scatterbrain in popular philosophy and psychology was Alfred Korzybski (1879–1950). In his *Science and sanity* (1933) and other publications he engendered a movement in mental sanity which is still around under labels such as General Semantics and Neuro-Linguistic Programming, and extracting big money from gullible institutions and individuals. Korzybski promised such goodies as a new foundation of mathematics, better diagnoses in psycho-somatic medicine, treatment of stuttering and dyslexia, as well as the elimination of stage fright. One of the causes of historical cultural disasters such as war and revolution, must be sought in the structure of our forms of representation, in particular our languages. Our languages do not provide a veridical map of reality, the way that mathematics does in engineering sciences: "The map is not the territory!!!" The issue is semantic in nature. An important step had been taken by "Lady Welby", Korzybski notes, but a further step was required. We need a *general* semantics which "deals with the nervous reactions of the human organism-as-a-whole-in-environments" and which

"is much more general and organismally fundamental than the 'meanings' of words as such, as Significs."[35] Indeed, "Our knowledge today indicates that all life is electro-colloidal in character, the functioning of the nervous system included," Korzybski writes in his very last paper (1951, p. 172), in which he repeats all his incantations of using hyphens, quotes, indexes, "etc.," and so on. And he cites Whorf in support of establishing a "non-Aristotelian language system." If you cannot follow this, you are not alone. But Korzybski did have many followers, of which two should be mentioned in the present context.

Samuel Ichiye Hayakawa (1906–1992) was a professor of English at the Illinois Institute of Technology in Chicago when he published his book *Language in action* (1939). "The semantic principles in this book," he writes on p. 337, "have been drawn mainly from the 'General Semantics' (or 'non-Aristotelian system') of Alfred Korzybski." And he cites many others, such as Welby, Ogden, Richards, and Carnap. The 1941 edition of the book reproduces as an appendix Whorf's *Science and linguistics.* The book elaborates the adage "The map is not the territory" for language use. The intention is to provide a foundation and practical guide for non-misleading usage of language. Like the Significa movement, Hayakawa distinguishes between the informative and affective uses of language. Both are involved when you hear somebody scream "the house is on fire," but the affective connotation is often "sold" as informative. This is mostly the case in advertisement, but it works much more generally too, as in "Congressman Blank is a demagogue." What does "demagogue" refer to (what is the "territory")? That is not specified. More generally, we often speak in terms whose referents are not specified. Such expressions should be avoided if talk is to be informative. The more fundamental fault is, according to Hayakawa, to speak with "intentional orientation," which is "the habit of guiding ourselves by words alone," speaking in terms of "Bolsheviks," "farmers," "Jews," and so on. Such terms have strong affective connotations, but the issue is what you are really referring to ("John" or "Mary," etc.). There is Korzybski's plea for the use of indexicals: Jew_1, Jew_2, which individualizes your reference. More generally, go for "extensional orientation." If we do not, we will keep reacting like "primitives" or children. "Primitives," especially, are totally locked in. Their intentional orientation is entirely two-valued; "strict compulsions about 'good' and 'bad' govern every detail of life. . . . A bird flying over the village is either 'good luck' or 'bad luck.' Nothing is meaningless or accidental to a savage, because everything he sees, if he notices it at all, *must* be accounted for under one of the two values." (p. 167). And so it goes on. Hayakawa ultimately became a Republican member for California of the US Senate, fighting for English as the official language of the United States.

Stuart Chase (1888–1985), an economist and popular writer, contributed to General Semantics with his book *Tyranny of words* (1938). In its introduction (p. vi) he writes: "Into the subjective relationship inside the 'me,' the psychological motives, association paths, complexes, fixations, and the rest, I have not seriously ventured. I do not know enough." This, regrettably, holds generally for his competence in linguistics, philosophy, and psychology. Joining Korzybski, as well as Ogden and Richards, his diagnosis is that

[35] Citations from *General semantics* in Korzybski's (1990) *Collected writings.*

we have two sins to fight against. One is identifying words with things ("the map is not the territory"), which only leads to magic, as in "tribal language use." The other is the misuse of abstract words. These are "bad semantic habits." The meaning of words is relative and the issue should always be: "Find their referent" (p. 4). That is the goal of semantics. "*If we wish to understand the world and ourselves, it follows that we should use a language whose structure corresponds to physical structure.*" (p. 56). This is violated in such expressions as "Does communism threaten the world?" or "We never get anywhere since the masses are so stupid." They are without meaning, because "communism" or "the masses" have no referent. Such meaningless use of language is all over the place in philosophy and the social sciences. Chase then sets the example by quickly going over the history of philosophy. It has been a promenade of terms without referent, such as "idea of good," "truth," "freedom," "natural ability," "free will," "materialism," "realism," and so on. The same holds for the social sciences. What is the referent of abstract terms such as "labour," "purchasing power," "the economic man," and "capitalism"? We should stop using meaningless expressions such as "Democracy is the best form of government" or "You can't change human nature." These are expressions without referents. Regrettably, Chase himself hardly does any better. What is the referent of: "We should use a language whose structure corresponds to physical structure?" (p. 56) or "If Americans were devoid of rigid principles, it is conceivable that poverty would have been virtually liquidated about 1925." (p. 80).

Chase was a great admirer of Whorf, but this was not mutual. Whorf was aware of General Semantics and Chase's work in particular. He did not want to get the scientific arguments in his *Science and linguistics* confused "with various popular bromides about the misleading use of words, which amounts to going off entirely on the wrong track," or with things like "the recent popular stultification of a similar subject by Mr. Stuart Chase, whom I consider utterly incompetent by training and background to handle such a subject." "For the immediate future, probably the loose-thinking 'semanticists' à la Stuart Chase, will introduce many popular clichés and make [the] term 'semantics' a hissing and byword, so that it will cease to be used by serious scientists." (Lee 1996, p. 16). By a curious and most unusual lapse of attention on the part of wise John Carroll, Stuart Chase was asked to write the preface to his 1956 edition of Whorf's articles.

George Orwell

Chase was also a target in Orwell's essay *Politics and the English language* (1946). The essay is a plea for using clear language. "Political language . . . is designed to make lies sound truthful and murder respectable, and to give an appearance of solidity to pure wind." "When there is a gap between one's real and one's declared aims, one turns as it were instinctively to long words and exhausted idioms, like a cuttlefish spurting out ink." Orwell provides wonderful examples, such as "*a consideration which we should do well to bear in mind*" (p. 354) and ". . . have turned to acts of provocation, to foul incendiarism, to medieval legends of poisoned wells, to legalize their own destruction of proletarian organizations, and rouse the agitated petit-bourgeoise to chauvinistic fervour on behalf of the flight against the revolutionary way out of the crisis" (p. 350). He then provides

a few rules of clear writing, such as "Never use a metaphor, simile, or other figure of speech which you are used to seeing in print." and "Never use a long word where a short one will do." (p. 359). However, "Stuart Chase and others have come near to claiming that all abstract words are meaningless, and have used this as a pretext for advocating a kind of political quietism. Since you don't know what Fascism is, how can you struggle against Fascism? One need not swallow such absurdities as this, but one ought to recognize that the present political chaos is connected with the decay of language, and that one can probably bring about some improvement by starting at the verbal end. If you simplify your English, you are freed from the worst follies of orthodoxy." (p. 359).

One returning target in Orwell's essay is Soviet and Communist propaganda, such as "*The Soviet press is the freest in the world.*" (p. 353). For completeness' sake a few words should be added on language-thought relations as conceived in the Soviet Union.

Some Soviet thoughts

When Stalin started his collectivization program, formerly isolated, illiterate villages in Central Asia were radically restructured. Vygotsky and his co-workers then took the initiative to go there for field work to study how the stages of historical change would go with changes in the *mechanisms* of thought (not only its content). Luria (1971) reported on some of this work, which took place during the early 1930s. In their experiments, Vygotsky and co-workers compared residents of the same village that had or had not yet gone through the process of literacy training and participation in the newly-formed collective farms. In one experiment, subjects were given four cards depicting different objects and they were asked to select the three "that go together," for example a saw, an ax, a shovel, and a piece of wood. Among the "traditional" residents there was no preference for selecting the three tools. They rather thought up a concrete situation in which three of the objects would go together. The "historically changed" residents, however, preferred to go for abstract categories, such as "tool." Similar results were obtained in a test of syllogistic reasoning. The "traditionals" reasoned in terms of concrete situations, the "changed" residents operated at a more abstract level. It is regrettably impossible for the reader of Luria's paper to check the data and their statistics. In fact, the paper radiates Vygotsky's apodictic style. Although the paper is not about linguistic relativity it is about "historical" relativity, a basic feature of Marxist thought.

Josef Stalin (1950) added his bit of thinking about linguistic relativity. The issue was whether any language is essentially a *class* language, preserving its "thoughts and ideals," or whether a language is independent of the social system that uses it, of the "superstructure" as Stalin called it. The latter is the case, according to Stalin. Language is not itself part of the superstructure. A language "exists immeasurably longer than any basis or superstructure" (p. 5). Take Russian. It has been Pushkin's language; "in all essentials it has remained as the basis of modern Russian." (p. 5). It survived the bourgeois, aristocratic era. "Languages owe their stability to their grammatical systems and basic word stocks." In other words, one should not be concerned about linguistic relativity (although Stalin does not discuss this concept). What one should guard against is "dialects," the

jargon of classes and factions. They "are confined to a narrow sphere" (p. 5). They have "no grammatical systems or basic words stocks of their own – they borrow them from the national language." (p. 7). The stable factor is the "national language," like Russian. Stalin recognizes that the USSR has a collection of national languages, such as Ukrainian, Byelorussian, Uzbek, Kazakh, Georgian, and others. Since none of them is part of any superstructure, they will equally well serve as common languages for communication within the Soviet empire. Stalin then harshly condemns Soviet linguist N.Y. Marr (1864–1934), whose disciples had until recently exerted full power in USSR linguistic circles. "No, save us from N.Y. Marr's "Marxism!,"" Stalin writes, "N.Y. Marr introduced into linguistics the incorrect, non-Marxist formula that language is a superstructure, and got himself into a muddle and put linguistics into a muddle." Marr was, apparently a dangerous Whorfian. The Soviet press responded predictably. Stalin now became the "great linguist," next to remaining "the greatest genius of mankind."[36] Stalin's longer perspective was the establishment of a "socialist world language." It would inevitably initially exist next to the national languages.

Studies of relativity after Sapir–Whorf

The issue of relations between language and world view, whether directly influenced by Sapir–Whorf or not, remained on the anthropological research agenda. Here we will not review the relevant work by Dorothee Lee (1944), Harry Hoijer (1953), and others.[37] The issue also arose, hesitantly, on the psycholinguistic agenda. John Carroll in his 1951 report on the *Interdisciplinary Summer Seminar in Psychology and Linguistics*, discussed in Chapter 1, addresses the relativity issue. Language categories may "provide a ready-made organization of experience"; this is to be studied by means of cross-linguistic experiments.

The 1953 conference on language in culture

The issues began to focus again during the 1953 conference organized by Harry Hoijer and colleagues in Chicago on the topic of *Language in culture*, of which carefully edited proceedings appeared in 1954. The explicit goal of the meeting was to review the state of the art with respect to Whorf's hypotheses and to plan for further research. The participants were mainly linguists (such as Greenberg and Hockett) and anthropologists (such as Kroeber and Newman). There was one cultural psychologist (Fearing) and a psycholinguistic doctoral student (Lenneberg); John Carroll in particular was not involved. The useful papers and discussions addressed issues such as: Does a language impose a world view or rather suggest semantic structures for particular domains, such as time, kinship, and number? Does a language determine what *can* at all be said, or rather what is relatively easy to say in the language? Are our perceptions universal, culture-free as Whorf

[36] Citations from Karel van het Reve (2008, p. 489).

[37] See for a review Lucy (1992).

suggested, or are there "fashions of perceiving" just as much as "fashions of speaking"? There were also methodological issues. How can we put our hands on cognitive processes that are not contaminated by speech? Greenberg proposed the experiment to transpose an African Baganda tribe to Eskimo country and study what views and perceptions are kept with their language and how the language would adapt to the new living conditions. The proceedings celebrate the victory of horizontalism but are rather subdued with respect to linguistic relativism. There were, however, two bits of discussion which contained the seeds of the experimental approach that was about to develop. Harvard linguist and psychologist Eric Lenneberg criticized the approach in which native speakers of different languages describe or name the same experience. The different reports are just different reports; the "greatest trap" is that you have no extra-linguistic check of whether the cognitions had differed. Do not have them speak, but test them on their non-linguistic behavior. He then made a few remarks on the color sorting and memory experiments he had began to perform (pp. 197, 266).

The codability experiments: Eric Lenneberg and Roger Brown

Eric Lenneberg (1921–1975, Fig. 13.2) was to become a leading psycholinguist. Born in Germany, he left with his parents for Brazil in 1933, then moved to the United States in 1945, where he studied linguistics at the University of Chicago. He then went to Harvard (1951) and obtained a PhD there in both linguistics and psychology (1955), pioneering the first "coding" experiments, together with Roger Brown. Subsequently he held positions at Harvard, MIT, and Cornell. His most important legacy for our field is his classic *Biological foundations of language* (1967).

Roger Brown (1925–1997, Fig. 13.2) received his PhD in psychology from the University of Michigan (1952). He then took a position at Harvard (1952–1957), joining Jerome Bruner's Cognition Project. He worked together with Lenneberg, who shared with him a fascination for Whorf's linguistic relativism.[38] In 1957 Brown moved to MIT, where he wrote his *Words and things* (1958), a primer on the psychology of linguistic reference. Brown returned to Harvard in 1962, making a career as a leader in both social psychology and psycholinguistics. His most important psycholinguistic legacy is *A first language* (1973), which, like *Die Kindersprache* of the Sterns six decades earlier, became a standard of diary-based developmental psycholinguistics.

Lenneberg, together with Brown and Roberts, set out to develop an experimental approach to the issue of linguistic relativity. Or rather, they developed an interesting new paradigm of studying specific relations between language and (sensory) experience, which they called *codability*. The paradigm, we now know, hardly addressed Whorf's world-view issue.

It began with Lenneberg's (1953) programmatic paper. Here Lenneberg first addresses what he called the "translation method" in ethnographical linguistics. His example is Whorf's analysis of the Apache expressing the experience which in English would be

[38] See Brown (1976), his memorial tribute to Eric Lenneberg.

(a) (b)

Fig. 13.2 (a) Roger Brown and (b) Eric Lenneberg. © Kris Snibbe/Harvard Staff Photographer.

formulated as "It is a dripping spring." A transliteration of the Hopi's expression would be something like "as water, or springs, whiteness moves downward." Whorf reaches this literal translation by analyzing the meaning of all constituent morphemes in the Apachean utterance. "How utterly unlike our way of thinking," Whorf concluded. Lenneberg objects that it "makes no sense to equate the global meaning of an utterance with the sequence of abstracted, general meanings of the morphemes that occur in the utterance." (p. 465). The way in which Apachean grammar requires a particular formulation should not be confused with the thought content of the utterance. The latter needs independent determination. Lenneberg does not deny that there is a metaphorical element in language usage. Individual morphemes and their combinations do activate associations, but there is no evidence that the full semantics of constituent terms is active in formulation. Lenneberg cites Cassirer's (1923) example of a Sudan African tribe where you must say, and apparently also think, that you are "inside" an action: "I am on the inside of walking." Such glossing proves nothing. In addition, we usually know very little about the exact semantics of words in an exotic culture. Brown (1958a) exemplified Lenneberg's translation argument by citing Mark Twain, who had ridiculed the German mind by providing an English transliteration of a German text.[39]

Lenneberg then proposes a new methodology for studying the language-thought relation. His point of departure is a "basic maxim in linguistics," that we can express anything in any language. So, we should not worry about the content, the "what" of communication, but on the "how." This "how" Lenneberg calls *codification.* Here one should go for

[39] Here is part of it: "That has even Germany itself aroused, and one has Schiller the permission refused the History of the Hundred Years' War to compose - God be thanked! After all these reforms established be will, will the German language the noblest and the prettiest on the world be." (Brown 1958a, p. 232).

an explicit, measurable aspect of the linguistic expression, for instance its length in words or syllables. That measure should then be related to some non-linguistic behavior, such as a measure of recognition, memory, or learning. Such codification behavior can be entirely studied inter-culturally. The example Lenneberg introduces is the color naming paradigm. Is a color that is "highly codable" (i.e., has a one-word name on which all subjects agree) more easily recognized, better remembered, etc? If that is found for speakers of English, it would "add weight" if it is also found for other languages.

There is indeed a new (and productive) paradigm here,[40] but it has little to do with Whorf's linguistic relativity. It is not remotely testing the existence of some underlying world view or semantic code, which expresses itself in a variety of grammatical aspects of a language.

The first experimental paper on codability was Brown and Lenneberg's (1954) study of color naming. They asked five judges to pick out the best examples of red, orange, yellow, green, blue, purple, pink, and brown from a large set of Munsell color chips. There was almost full agreement among judges. To these eight "focal" chips 16 other, less focal ones were added. The 24 chips were then one by one presented to each of 24 subjects, whose task it was to name them (not necessarily with a single word). How "codable" are these colors? Brown and Lenneberg had five measures of codability. There was, first, the length of response in syllables or, second, in words. There was, third, the subject's naming latency, i.e., the reaction time of the response. Fourth, there was the measure of agreement between subjects on the given name and, fifth, there was the degree of agreement of subjects with themselves during a second naming trial. These five measures were highly correlated. Their common factor was called the linguistic "codability" of the color. What was the non-linguistic measure? The authors opted for recognizability. The (new) subjects saw a board with 120 different color chips. Among them were the 24 experimental chips of the naming experiment. The subjects were then shortly exposed with four colors simultaneously (all four from the original 24 set). After removal they had to indicate the four colors on the board. No color was named during the entire experiment. The issue was, of course, this: would a linguistically highly codable color be more easily identified on the board than a low-codable one? That was indeed the case.[41] Why was this so? In the post-experimental debriefing most subjects reported that they had silently named the four test colors and tried to remember the four names; they then made their selections from the board on the basis of these names. This issue of silent verbal rehearsal kept coming up in subsequent codability experiments. The codability effect is mediated by verbal short-term memory.

In a subsequent paper Lenneberg and Roberts (1956) extended the paradigm to a cross-linguistic comparison. The authors assume that the (sensory) experience of color is universal, independent of language or culture. Here they are in good company with Whorf,

[40] As early as the late nineteenth century, Dr. Lehmann had performed an experiment in Wundt's laboratory in which subjects had to recognize ten shades of gray. They performed much better if they had first associated the gray-chips with the numbers 1–10. Study cited in James (1890), Vol. 1, p. 674.

[41] Even after correction for the *discriminability* of the relevant chips on the board.

for whom sensory experience can function as the universal "canon of reference" between languages. In addition, the color domain displays a good "range of variation" among languages; color vocabulary varies substantially from language to language. In the experiment, colors not only varied in hue, as in the previous experiment, but also in brightness. In other words, they were sampled from a two-dimensional "color map." The cross-cultural comparison was between 24 English-speaking Harvard/Radcliffe students and 12 Zuni speakers, who were tested in the Zuni reservation of western New Mexico.

With the help of Zuni specialist Stanley Newman, the authors first collected the color vocabulary of the Zuni community. In the main experiment (both the English one and the Zuni one) subjects were presented with a plate of 120 colors, two-dimensionally arranged according to hue and brightness. A transparent sheet was laid over the plate and a subject would then map out areas of color that shared a color name. They also indicated the "focal center" of each area, i.e., the most typical chip for the color name they had chosen to map. The resulting maps were then used to select a sample of test colors, namely the focal ones plus an equally large set of non-focal ones, which nicely spread over the entire two-dimensional color space. This final set of color chips was then presented for naming to the experimental subjects. From the naming agreement between subjects, it was possible to localize the highly codable colors on the two-dimensional color map, for both the English and Zuni subjects. The main finding was that the distribution of focal, codable colors was comparable between English and Zuni, with one marked exception. Where English has quite focal and distinct *yellow* and *orange*, the monolingual Zuni subjects do not distinguish them and apply a single category to them both. In a subsequent recognition experiment, along the lines of the Brown and Lenneberg procedure, there was no correct recognition of either orange or yellow in the monolingual Zuni subjects, a fine cross-linguistic contrast to the English-speaking subjects.

The authors conclude that "named" colors differ in "weight" from unnamed ones in a culture's color cognition. Landar *et al.* (1960) replicated the study for Navajo color categories, but without the recognition part. Lucy (1992) discusses various problems with the Lenneberg *et al.* approach, both procedural and theoretical. Many of these problems became handled in subsequent, ever more sophisticated work (Lucy's included). But that is beyond our present time window. We should conclude here by noting that Eric Lenneberg and his associates planted the seeds for a new, still productive cross-linguistic research paradigm. In addition, the new concept of codability and its relation to Zipf's law and to short-term memory storage triggered a major advance in memory research (Miller 1956).

The coding of facial expressions

There was one non-color application of the Brown and Lenneberg codability paradigm. The Dutch psychologists Frijda and Van de Geer (1961) applied it to the naming and recognition of facial expressions. The application is quite interesting, because facial expressions are hardly "sensory," as colors are. Many quite different stimuli can make the impression "astonished" or "frightened." The authors argue that one should therefore,

if anything, expect a greater codability effect than in the sensory color domain. They did find a strong, significant correlation between measures of verbal codability (such as inter-subject agreement) and measures of facial recognition, but these did not exceed the correlations in the Brown–Lenneberg study. Also here, the subjects reported that they coded and memorized the facial expression by means of an appropriate adjective (such as "astonished" or "frightened") when they were performing the recognition task.

Grammatical categories and cognition

Quite probably, Whorf would not have recognized the coding experiments as tests of his relativity claims. He had never argued his case from color vocabularies and he would certainly have considered color recognition as a *perceptual* process, subject to the universal laws of visual perception. His focus had always been on *grammatical* categories, their implicated "logic" or semantics. The 1950s also saw the first attempts to develop an experimental approach to these matters.

Whorf and other anthropologists had by then extensively studied the part-of-speech structures of various languages, demonstrating substantial variation, even among American-Indian languages. The conjecture was that the more than 20 form classes in Navajo, for instance, relating to round, long and thin, granular etc. objects, would probably induce the Navajo speaker to attend to such features. Likewise, the English speaker is likely to acknowledge a distinction between extended substances without size or shape, such as air or water, and more circumscribed objects such as animals and tools, because English makes a grammatical distinction between mass and count nouns. However, as discussed in Chapter 8, mainstream American linguistics had explained such seemingly meaning-related distinction away as entirely formal and distributional. Roger Brown (1957) refers to the distributional part-of-speech tests proposed by Fries (1952). A noun, for instance, is any term acceptable in the sentence frame "(The) _____ is (are) good.," and similarly for verbs, adjectives, etc. That, however, Brown argues, does not diminish an apparent correlation of form class with semantic features and that is exactly what Whorf had been arguing.

To explore this further, Brown took the 1000 most frequent words in the Thorndike–Lorge list (based on adult usage) and compared it with the 1000 most frequent words in the Rinsland list, which is based on first-grade children's conversations. He then selected all nouns and verbs in the adult list that did not occur in the children's list and vice versa. It turned out that 67% of the "exclusive" childrens' nouns denoted concrete, picturable objects, such as *apple* or *table*. For the "exclusive" adult concrete nouns amounted to no more than 16%. They were rather more abstract words, such as *experience*. Similarly, exclusively children's verbs named animal and human movement or action in 67% of the cases, whereas this was only 33% for exclusively adult verbs. In other words, for children the noun/verb distinction is highly correlated with a semantic object/action distinction. Brown suggests that this may work as an attentional filter in the acquisition of noun and verb meanings. If children hear *Where is the sib?*, they will attend to concrete objects. But if they hear *Is doggie sibbing?*, they will rather focus on some action. To test for this type

of bootstrapping, Brown performed an experiment where pictures were shown denoting an action (like kneading), a mass (like confetti), and a particular object (like a container). The children were induced to attend to one of these features by telling them "Do you know what it is to sib? In this picture you can see sibbing." or "Do you know what a sib is? In this picture you can see what a sib is." or "Have you ever seen any sib? In this picture you see some sib." To check whether the syntactic part-of-speech information had indeed induced the children to pick a particular kind of referent (an action, a mass, or a particular object), they were asked to select from a picture set another picture with either the same action, the same mass or the same particular object (where the other two were always different). The trigger questions were of the kind "Now show me another picture of sibbing." The 3–5-year-old children performed highly above chance. So, here is a Whorfian case where the underlying "logic" of a grammatical category directs thought. Was this effect present in adults? Would it be different for other languages?

John Carroll, who had just published Whorf's collected papers, performed an experiment on the non-linguistic behavior of Navajo children (Carroll and Casagrande 1958). The idea was to check whether these children would especially attend to perceptual features that correspond to a mandatory category in verb stem selection. That selection depends, as discussed, on characteristics of the object, such as being long and rigid (like a stick), flat and flexible (such as paper), or long and flexible (such as a piece of string). Would experience with their language make these children attend more to such verb-form critical features of objects than to other properties? The test the authors designed was simple and elegant. The children would be presented with two objects differing in two features simultaneously, for instance in color and size or in shape and color. In the test cases one of those features was a "verb-critical" feature such as long-and-flexible versus long-and-rigid. The child was then presented with a third test object that was similar to each of the objects in just one feature, but a different feature for the two objects. The child had to tell the experimenter which of the pair went best with the test object. For instance, the object pair consisted of a yellow stick and a piece of blue rope. The child was then shown a yellow rope. Would the child match for color or for flexible string? Only the latter corresponds to a grammatical feature.

Of the Navajo children, all tested in the Arizona Navajo reservation, 59 were Navajo-dominant and 43 were English dominant. They formed the test and control groups. The results confirmed the predictions. The Navajo-dominant children far more often matched on the grammatically relevant feature than the English-dominant controls. This is a fine, non-trivial result. We saw that the codability data could always be explained by the subjects memorizing the stimulus by way of a linguistic label. That kind of linguistic mediation is less likely (though not excluded) in the present experiment, because the tested grammatical feature has no *name*. Casagrande continued this type of experiment together with Fishman. The latter published one of the best early reviews of Whorfian studies (Fishman 1966).

A final, related study in the present category was published by Maclay (1958). It is based on his dissertation, supervised by Stanley Newman. Maclay had Navajo, English, and a

control group of Pueblo speakers do a sorting task. They always received a set of four objects, which differed on a few features, but one of them was a verb stem feature, as in the Carroll and Casagrande study. The subjects were asked to partition the four objects into two groups of two. Would the Navajo speakers prefer to sort them in terms of the verb stem relevant feature, at least more so than the English or Pueblo speakers? The outcome was entirely negative. Maclay nevertheless made an excellent career in psycholinguistics. In the following year he published, together with Osgood, a seminal early paper on hesitation phenomena in spontaneous speech.

Retrospect: John Carroll's verdict

John Carroll, as we saw in Chapter 1, had as a boy been Benjamin Whorf's dedicated library assistant in Hartford over a three-year period. Whorf, and through him Sapir, were Carroll's major life-long sources of linguistic inspiration. Following Sapir's advice, Carroll became a psychologist, but none of his contemporary psychologists were so well versed in linguistics. He became a wise mediator between psychology, linguistics, and anthropology. Given Carroll's life-long personal relation to Whorf, his thorough editing of Whorf's papers and his own experimentation in linguistic relativity, it is appropriate to close this chapter with a few remarks on Carroll's (1963) own assessment of the state of the art in linguistic relativity.

Carroll, after acknowledging the long history of relativity notions, going back to Herder and Humboldt, notices that Whorf never provided a clear statement of the linguistic relativity hypothesis, but only exemplifying instances. "A proper statement of a linguistic relativity hypothesis demands the most exacting knowledge of psychological and linguistic theory." After sketching the communication theory, following his 1951 report, he assesses the state of the art in the relativity issue. It is two-pronged: harshly negative on the world view aspect of Whorf's hypothesis, but positive on the differential codification aspect.

Here is his conclusion on the world view issue: "At the present time we must draw the conclusion that no satisfactory evidence exists for thinking that languages reflect particular world views. Indeed, the evidence from common knowledge would seem to point in the opposite direction, namely, to the effect that any world-view can be expressed in any language." (p. 10). As one demonstration, Carroll analyzed Whorf's glosses of different modes of action in Hopi. The relevant verbs encode a contrast between momentary and repetitive events. For instance *ri' 'pi* Whorff glossed as *it gives a flash*, whereas *rïpi'pita* means something like *it is sparkling*. From this, Whorf infers that the Hopi language is "better equipped to deal with vibratory phenomena than is our latest scientific terminology." (p. 8). It is "a long jump," writes Carroll, to go from "these linguistic data to the inference" that it endows the Hopi speaker with "the ability to see the world in terms of the physicist's contrast between particle and field of vibrations." (p. 9). Rather, "the basic concept is one which can easily be conveyed in English." Carroll also ridicules Hoijer's analysis of the Navajo "world view," which is in a nutshell that "Nature is more powerful than man." Only elaborate ceremonies can regulate that precious relationship. This is

reflected, according to Hoijer, in the Navajo verb system, which expresses actions as participations in more encompassing events. Carroll easily reverses the argument, to conclude that the Navajo entertain an "excessively physicalist view," rather than the ritual one Hoijer presupposes. "We would be well-advised to abandon the notion that languages impose world-views on their speakers or that a language tends to reflect a world-view of its own." (p. 11). And what about Whorf's warning against the spread of English, or any other language, as a world's language? We "may be assured," Carroll writes, "that there is insufficient evidence for thinking that such a language bears within itself, like a Trojan horse, a particular view of the world." (p. 19).

Here is Carroll's formulation of a positive, weak version of the linguistic relativity hypothesis: "*Insofar as languages differ in the ways they encode objective experience, language users tend to sort out and distinguish experiences differently according to the categories provided by their respective languages. These cognitions will tend to have certain effects on behavior.* The speakers of one language, for example, may tend to ignore differences which are regularly noticed by the speakers of another language. This is not to say that they *always* ignore them, but they are differences which are not always salient in their experience." (p. 12). Carroll illustrates this by comparing the utterance *he went to town* to its German equivalent *Er ging in die Stadt.* The English expression is entirely neutral with respect to the mode of travelling, but the German one implicates some form of self-propelled motion. So where you can be neutral on the mode of action in one language, it is habitually expressed in the other language. Speakers "must pay attention to whatever discriminations are required in their respective languages" (p. 13), i.e., thinking-for-speaking. If the language has a tense system, for instance, it is mandatory for the speaker to check the relevant time relations in the state of affairs to be expressed. If it is mandatory to mark number on any count noun phrase, the speaker must check the relevant cardinality in the state of affairs. Carroll's weak version of relativity, correctly or incorrectly, includes more than thinking-for-speaking alone. There is in addition the tendency of language users to attend anyhow to aspects of reality that are habitually expressed in their language (such as, in the examples, time relations and numerosity). Carroll adds a developmental aspect here: children will more easily attend to new concepts which are encoded in their language.

Meanwhile Carroll has been proven right on both scores. The Whorfian search for language-implicated world views has dwindled away. However, the study of tendencies, preferences in conceptualizing states of affairs in agreement with mandatory or habitual distinctions in the native tongue, has become a rich and productive field of research.[42]

[42] As is nicely sketched in Levinson's (2012) new *Foreword* to Whorf's selected papers.

Chapter 14

Psychology of language during the Third Reich

Karl Bühler was rarely annoyed. "But I do remember an occasion where his indignation bordered on fury. The German Psychological Society held its 11th Congress in Vienna in 1929. Some psychologists (I don't remember who they were) appeared with the swastika on their lapels. Bühler was not just disgusted by this nationalistic movement; what triggered his rage was—as he said openly—the arrogance with which these participants made a show of their political alliance at a non-political gathering."[1] This Kardos wrote in 1984. Bühler's outrage anticipates the dramatic developments leading to the end of Bühler's academic career, his emigration to the United States, and more generally to the end of German psycholinguistics.

Bühler's story is just one among a terrifying number of personal fates suffered under Hitler's regime. When we consider this era in the present chapter, we will drastically limit ourselves. Utz Maas (2010) has published, what I consider, the definitive work on the fates of victimized language scholars. His book, Hutton's (1999), and Knobloch's (2004, 2005) are also rich treatments of the language sciences during and preceding the Third Reich. Graumann (1985), Ash (1988, 1995), and Mandler (2007) are among many excellent sources on the corresponding history of psychology. Our focus will only be on contributors to the psychology of language. Most of them have already featured in previous chapters. But others, such as Friedrich Kainz and Frieda Goldman-Eisler, began their careers during this fateful period. Before turning to these personal histories, however, we should consider some psycholinguistic issues that became deeply politicized during the Nazi period. They were largely the issues discussed in the previous chapter: relations between language, race, and *Weltanschauung*.

Language, race, and world view

During the nineteenth century, "verticalism" was all but the exclusive view in linguistics. The world's languages show a hierarchy of complexity, which in turn reflects the evolution

[1] "Auch sonst habe ich ihn selten verärgert gesehen. Ich erinnere mich allerdings an eine Gelegenheit, bei der sein Ärger bereits an Empörung grenzte. Die Deutsche Psychologische Gesellschaft hielt 1929 ihren XI. Kongreß in Wien. Einige Psychologen (ich erinnere mich nicht mehr daran, wer es war) erschienen mit dem Hakenkreuz an ihrem Rockaufschlag. Bühler war diese extreme nationalistische Bewegung nicht nur zuwider; was seine Empörung hervorrief, war – wie er uns offen sagte – die arrogante Art, mit der jene Teilnehmer ihre politische Zugehörigkeit bei einer unpolitischen Zusammenkunft zur Schau trugen." (Kardos 1984, p. 37).

of the human mind. This dogma influenced, and easily merged with, the biological theory of evolution according to which human races differ in developmental potential, which is reflected in the complexity of their languages. Also during the nineteenth century it became *bon ton* to consider ethical implications of such genetic views. Galton's eugenics, for instance, aimed "to give to the more suitable races or strains of blood a better chance of prevailing speedily over the less suitable than they otherwise would have had."[2]

The Dutch linguist van Ginneken, we saw, had his own opinions on race and language. Races differ in their "articulation base." i.e., the innate shape of their vocal tracts. When races mix, these shape features enter into Mendelian combinations. In his major work *Race and language* (1935), van Ginneken develops, in excessive detail, the theory that the articulation base of the "pre-slavic race" affected, by interbreeding, the north-eastern dialects of the Low Countries. In themselves, such speculative theories are hardly obnoxious. But factually, they easily combined with more or less outspoken racism and anti-Semitism. Van Ginneken (1914), for instance, includes a chapter on the language of Dutch Jews. It begins by sketching the Jewish character, which is reflected in their syntax. "No periods, no richly structured, deep thought-out language constructions, but sober, naïve sentences, loosely connected together, by the eternal and . . . and . . . and . . . for us a maze, for them the flowering garden in which their thoughts ripple and play like butterflies."[3] "The inner life of the Jews is not fine, however passionate in full power. . . . Many go straight for the sexual and straight for the money."[4] And so it goes on and on. A reputed Dutch linguist, a Jesuit priest, soon a member of the Dutch Royal Academy of Sciences and a professor at Nijmegen University, could apparently get away with such blatant anti-Semitism. It was widespread indeed, not only in Germany.

The other psycholinguistic issue that became politicized was the relation between language and world view. The Humboldtian view that any natural language "incites and inspires" a particular world view was granted a long life, as we saw in the previous chapter. These world views need not be better or worse, more or less developed. Whorf stressed that they are just *different* in the first place. But again, linguistic relativism is easily politicized. Some world views are nobler than others. A people's culture is conserved in its language. A great people owns a great language. Such views were nourished, in particular in Germany, long before the Third Reich (Knobloch 2005). Youth leader and largely self-made linguist[5] Georg Schmidt (1890–1945), for instance, published an essay in 1917 entitled *Our mother tongue as weapon and tool of the German idea.* Schmidt argues that the

[2] Galton (1883, p. 17).

[3] "Geen perioden, geen rijk geconstrueerde, diepdoordachte taalgewrochten, maar sobere, naïeve, nuchtere zinnetjes, los aan elkaar gevoegd, door het ewige en . . . en . . . en . . . voor ons een doolhof, voor hen de bloeiende tuin waar hunne gedachten als vlinders wiegelen en spelemeien." (Van Ginneken 1914, Vol. II, p. 7).

[4] "Het gevoelsleven van de Joden is niet fijn, hoe hartstochtelijk ook in volle kracht. . . . Velen gaan recht op het sexuele af en recht op het geld." (ibid. pp. 9–10).

[5] Schmidt had a 1914 undergraduate degree from Jena in German studies, modern languages, and education.

ultimate gunshot of the World War (i.e., World War I) will not be the end of the battle (he was himself repeatedly wounded). "Today we also fight for the non-terrestrial *Heimat* of all Germans, also those in far-away countries: for German art and German essence, for German thought, for Germanhood as such. . . . This essay will clarify *one* aspect of this consideration and awareness, it will demonstrate: the significance of our mother tongue as weapon and as tool of the German idea."[6] The German language, Schmidt keeps saying, is the essential carrier of German culture. German will ultimately be one of two or three world languages. Schmidt also considered the relation between language, race, and mental constitution. A person's mental constitution, Schmidt writes, is determined "by both factors, race and language. I am personally inclined, to consider language as the more important and essential factor."[7]

Weisgerber (1929), we saw, continued and elaborated this rant about the mother tongue, claiming full linguistic determinism, but never citing Schmidt (who by then called himself Schmidt-Rohr[8]). Schmidt-Rohr, in his turn, became one of the main linguistic activists in the National-Socialist movement, although not immediately. His 1932 book *Language as builder of peoples*, reappearing as *Mother tongue* in 1933, again stresses the primacy of language over race in shaping the minds of a people. In fact, Schmidt-Rohr's book is an all-out attack, in great detail, on the Nazi race theory of language, "the misconception of a species-specific language."[9] Even a trained phonetician cannot tell the German of a Japanese child or a Negro child that grew up in a German-speaking environment, from that of a German child (p. 231). Both race theory and anti-Semitism are counterproductive in pursuing the noble goal of establishing a grand and vital German *Volk*. Both will undermine the "community awareness within the German *Volk*" (p. 289). Race ideology, only based on a rather accidental set of bodily features, will split the community rather than unify it, according to Schmidt-Rohr. It is for "the conception of the German *Volk* an unusually damaging delusion."[10] And the same holds for anti-Semitism. The Jews form a special sub-community of the German people, just like the urban communities or the Methodists (p. 291). "I want to establish that the Jews are Germans, a

[6] "Wir kämpfen heute auch für die nicht erdenhafte Heimat aller Deutschen, auch derer in fernen Ländern. Wir kämpfen für unsere größere Heimat: für deutsche Art und deutsches Wesen, für den deutschen Gedanken, für das Deutschtum schlechthin. . . . Diese Darstellung will einen Gegenstand dieses Besinnens und Erkennens klären, sie will zeigen: die Bedeutung unserer Muttersprache als einer Waffe und eines Werkzeugs des deutschen Gedankens." (Schmidt 1917, p. 1).

[7] "daß die seelische und geistige Verfassung des Einzelmenschen und die ganzer Völker bedingt ist nicht durch den einen *oder* den anderen Factor, Rasse oder Sprache und Sitte, sondern durch beide Faktoren, Rasse und Sprache. Ich bin persönlich geneigt, die Sprache für den bedeutenderen und wesentlicheren Faktor zu halten." (ibid. p. 27).

[8] Adding his wife's family name Rohr.

[9] "Wahnbegriff der arteigenen Sprache" (Schmidt-Rohr 1932, p. 302).

[10] "Aber die Rassevorstellung . . . ist ein dem deutschen Volksgedanken ganz ungewöhnlich schädlicher Wahnglaube." (ibid. p. 291).

sub-community of our dear German *Volk*."[11] In his hefty style, Schmidt-Rohr even ridicules his racist opponents with their cult of the "Nordic race." "The Nordic fanaticism, the anti-Semitic libel, strongly prevent our sleeping *Volk*, divided by self-destructive delusion, to become a unified, awake nation."[12]

This was, of course, not acceptable to the core Nazi hardliners. A years-long linguistic battle resulted, which we will not follow in detail. The most vicious attack, also on Weisgerber, came from Edgar Glässer (1939): "If however Schmidt-Rohr comes to the requirement of integrating Jewry in the front of the nation, as a consequence of his language view, then he has himself condemned this language view."[13] It is no more than "Jewish fanaticism."[14] Every self-respecting language consideration must be "filled with the will of destiny of a racial providence which, in the historically evolved, recognizes the eternal return of the blood and the eternal return of its racial values."[15] And Glässer makes due reference to "the exemplary treatment of the total culture-political connections of our issue in the Führer's work Mein Kampf."[16]

Schmidt-Rohr, however, did not give up his activist program, contributing to nation building by promoting German, "the most perfect of all languages," in the endangered-border regions, in Europe, and in the world at large. But that required him to tone down his language. In a paper *Race and language* (1939) he dealt anew with the question of how the conceptual world and the world of people's values is formed and preserved. This is mediated by the people's language, however, only to the degree that the people's race allows. There are six races in Germany, and their potential suffices to carry the German language. Belonging to an authentic race is conditional for the acquisition of the full "mental world" (*Geisteswirklichkeit*) embodied in a language. Race, in its turn, can only work through language. No mention of Jews or anti-Semitism this time. The ambiguous relation between Schmidt-Rohr and the officials interfered with his efforts to complete his academic career. In 1943 he became head of a probably irrelevant "Secret Political Language Bureau," which advised the political leadership on the promotion of the German language. By 1944 he had finally entered a habilitation procedure in Vienna,

[11] "wenn ich zu erweisen suche, daß die Juden Deutsche sind, Untergruppe unseres lieben deutschen Volkes." (Schmidt-Rohr 1932, p. 301).

[12] "Die nordische Schwärmerei, die antisemitische Verleumdung verhindern besonders stark, daß aus unserem schlafenden oder von selbstzerstörerischem Wahn zerrissenen Volk eine einheitliche, wache Nation wird." (ibid. p. 303).

[13] "Wenn aber Schmidt-Rohr zur Forderung des Einbaues des Judentums in die Front der Nation (. . .) infolge seiner Sprachauffassung gelangt, dann hat er diese Sprachauffassung selbst gerichtet." (Glässer 1939, p. 52).

[14] "jüdische Schwärmerei " (ibid. p. 54).

[15] "erfüllt sein von dem Schicksalswillen einer rassischen Vorsehung, die in dem geschichtlich Gewordenen die ewige Wiederkunft des Blutes und die ewige Wiederkunft seiner rassischen Werte weiß." (ibid. p. 62).

[16] "Nur hingewiesen zu werden braucht hier auf die vorbildliche Behandlung der ganzen kulturpolitischen Zusammenhänge unserer Frage in des Führer's Werk "Mein Kampf"" (ibid. p. 59).

supported by Bühler's successor Ipsen and by Friedrich Kainz. But by January 1945 the procedure dwindled out and Schmidt-Rohr lost his exemption from military service. He was killed in February 1945, leading a *Volkssturm-Einheit*, a reserve militia.

Weisgerber similarly played an active role in war-time language policy, but never in writing. As a Celtic scholar, he considered the Bretons as a sister people, who should shake off the French yoke. A task force was established to promote the Breton autonomist movement, in which Weisgerber played an essential role. He became, in particular, responsible for broadcasting from Radio Paris a weekly program on Breton culture. After the war, Weisgerber managed to hide his active role in the regime's propaganda machine till his death in 1985. Hutton (1999) presents a lucid account of these and other developments in Third Reich linguistics.

The 1931 Hamburg Congress of the German Psychological Society

Bühler's misgivings about the 1929 congress did not recur during the 1931 congress. On the contrary, the proceedings (Kafka 1932) display a rather peaceful atmosphere. William Stern's institute in Hamburg hosted the congress; Stern himself was the current president of the society. The chair of the congress was Karl Bühler, whose opening speech was optimistic: "Youth has confidence in the future of psychology." he noted and he congratulated the "world-wide known Hamburg research site in psychology." He announced a keynote lecture by Krueger, Wundt's successor in Leipzig. And he was especially proud of the interdisciplinary "speech day," the first of its kind, which would take place during the congress. That speech day was remarkable indeed. There were talks by Bühler himself, by Ach, Cassirer, Goldstein, Grünbaum (1885–1932, developmental psychologist and aphasiologist from Utrecht, former student of Külpe), Gruhle (psychiatrist), Ipsen (sociologist), Weisgerber, and Werner.

What if Karl Bühler had worn magic glasses, telling him the short-term fates of all these colleagues? He would have seen William Stern dismissed from his university in 1933 and his world-wide known institute dismantled. He would have seen Gustav Kafka resign as chairman elect of the next congress in Dresden and lay down, in 1934, his professorship in protest against the anti-Jewish measures. He would have seen Felix Krueger happily taking over organizing the 1933 congress in Leipzig, the first one under the Nazi regime. He would have seen Narziß Ach there, dedicating to the Führer his theory of action. He would have seen Cassirer, former rector of Hamburg University, dismissed from that university, being Jewish. He would have seen Kurt Goldstein in 1933 being imprisoned in Berlin and then emigrating to Holland. He would have registered Grünbaum's early death the following year. He would have noticed Gunther Ipsen publishing a book *Blut und Boden* in 1933 and in 1939 becoming his successor in Vienna after his own dismissal and emigration to the United States. He would have observed Leo Weisgerber contributing to the new regime's propaganda machinery. And he would have noticed Heinz Werner's 1933 dismissal from Hamburg University and emigration to Holland and the United States.

The 1933 Leipzig Congress of the German Psychological Society

The board of the Psychological Society, consisting of Stern (chair), Ach, Bühler, Kafka, Katz, Krueger, and Poppelreuter, had scheduled the next biennual meeting for April 1933 in Dresden with Gustav Kafka as congress chair and local organizer.

However, not long after Hitler was sworn in as German chancellor on 30 January 1933, a ferocious paper appeared in the *Freiheitskampf* of 18/19 March. It mentioned the names of some 30 leading psychologists in Germany and Austria and concluded: "In only one-third of the German universities the representation of this science is in German hands. Most of the just mentioned representatives were not even born in Germany! Their cribs stood in Russia or in the Balkans! And is it not the case that the repulsive pansexualism of the psychoanalysts of all denominations is a Jewish-Viennese growth, just as the Marxist individual psychology? . . . The Dresden Congress shall and must introduce the cleansing process! In the realm of Adolf Hitler no incoming East-Jew should any longer be a teacher of the *Volk* at German Universities!" Kafka then immediately checked, through a colleague and party member, whether the Congress would be able to proceed without disturbance. The answer was negative. Kafka then proposed, in writing, to the other board members that they cancel the congress. He added that, if they wanted to proceed nevertheless, they should appoint a different congress chair "because I will at once cease all congressional preparations." The Board accepted, in writing, Kafka's proposal with a 5:2 vote and Kafka called an immediate Board meeting for March 28 in Berlin. There the Jewish members Stern and Katz resigned and left the meeting. Bühler, not present, also resigned. A new Board was formed consisting of Ach, Handrick, Jaensch, Klemm, Kroh, Krueger, Poppelreuter, and Rieffert, all of them loyal to the new regime. They decided to move the congress to 16–19 October in Leipzig. Krueger (not yet a party member) agreed to function as the local organizer. Four days later this still secret decision was victoriously announced in the *Freiheitskampf*, clearly leaked by some Board member. It expressed its satisfaction that "not only the two race-Jews, but also Prof. Kafka, who called our German liberation movement 'party politics' which should be kept away from science, were eliminated." This occasioned Kafka to withdraw as a member of the Society and others followed suit, such as Stern and Katz.[17]

At the congress in Leipzig, Poppelreuter, Rieffert, and Volkert appeared in *Sturmabteilung* uniforms. The congress was opened by the Saxonian minister of education, who expressed this wish: "May this first meeting of the German psychologists in the new realm of our *Führer* Adolph Hitler, this great psychologist by introspection, build a foundation and point of departure for salutary accomplishment for the German Volk."[18] Narziß Ach, whom we met in Chapter 7 as the theoretician of the "determining tendency" in the Würzburg School and as a major source of inspiration to Otto Selz, has a one-page contribution in the proceedings (Klemm 1934). It concerns the Führer problem. The text is

[17] This section profited from Mandler (2002, 2007) and from documents in the Köhler Archive of the American Philosophical Society.

[18] Translations from the Congress Proceedings (Klemm 1934).

intentionally ambiguous in its denotation of "Führer." It is used both in its generic sense of "leader" and as a specific reference to Hitler. "The creative side of the determinative talent, the objectivation skill and with it the liberation of the *Ich* and the ability to accept ever new responsibilities, are, together with the persisting force of the persevering man, the typological requirements for a heroic *Führer*."[19] These are the transparent flatteries of a devotee. Ach had co-signed the spontaneous 1933 declaration of approval of Adolf Hitler and the National-Socialist state by professors at German universities. His final 1935 monograph on volition theory, however, is unmitigated science. In his longish contribution to the 1936 congress of the German Psychological Society, Ach claimed that "the major part of members of the Nordic race are predisposed to introversion." which is "of decisive importance to racial psychology."[20] In the 1938 congress he stressed the importance of taking responsibility, for which the "essentially introverted Nordic race is genetically better preconditioned."[21] His concluding sentence is: "That is, in striking manner, shown by the present *völkisch* state, which is fully conscious of its responsibility."[22] Ach became emeritus of Goettingen University in 1937. Then, in the midst of the war, he engaged in editing a four-volume *Lehrbuch der Psychologie*. Volume 3, "Practical psychology" was the first one to appear in 1944. It was also the last one to appear. Even here, Ach kept his contributors' flirtations with Nazi philosophy within bounds. But, of course, it became obvious in various chapters. "The body is animated, it is not a tool of mind, but carrier of its own values, expression of the racial inheritance, it doesn't need justification by the mind, but carries a value in itself."; "We experience as it were, mostly triggered by National-Socialism and its militant philosophy of body, a resurrection of the years-long suppressed vital values and nature-based instincts"; "The mind is as much German as the body"; "Psychoanalysis was in sharpest degree limited by its descent from the Jewish-materialistic mode of thought"; and the Hitler citation "The physical hardening should imprint the individual with his superiority and give him the confidence, which lies eternally in one's own power only." There is hardly any reference to Jewish authors in the book. But what is most remarkable is the clearly intended, absolutely *total* absence of references to non-German literature, whether English, French, Russian, or whatever. German psychology, or what remained of it, had turned inside and lost contact with the scientific world. It would take a quarter-century before the cocoon began opening again. Ach died in 1946.

[19] "Die schöpferische Seite der determinativen Veranlagung, die Objektionsfähigkeit und mit ihr die Entlastung des Ichs und die Möglichkeit der Übernahme von immer weiteren Aufgaben, sind, gepaart mit der Starrheit und ausdauernden Kraft des perseverativen Menschen, nach der typologischen Seite hin die Voraussetzungen des heroischen Führers." (Klemm 1934, p. 112).

[20] "insofern z.B. die überwiegende Zahl der Angehörigen der nordischen Rasse introvertiert verlagert ist." "für die Rassenseelenkunde von entscheidender Bedeutung." (Klemm 1937, p. 133).

[21] "daß die im wesentlichen introvertiert veranlagte nordische Rasse zum Erwerb der Verantwortungsbereitschaft günstigere Vorbedingungen aufweist." (Klemm 1939, p. 97).

[22] "Das zeigt uns in markanter Weise der heutige, sich seiner Verantwortung voll bewußte völkische Staat!" (ibid. p. 98).

Another Board member at the Leipzig congress was Erich Jaensch. As a young man, Jaensch had performed his very clever siren experiments on vowel perception (discussed in Chapter 12). Later he "discovered" eidetic imagery, the presumed ability of photographic memory in some children and adults. The evidence was, and still is controversial (if not totally lacking), but Jaensch got away with it. Jaensch and his team in Marburg then moved into typology, the study of biological-cognitive types. From around 1930 he became a staunch supporter of the National-Socialist movement and put his full scientific leadership into "a mental battle of unique toughness and difficulty." It was to develop his typology as a scientific tool to spot the *Gegentypus* ("counter type"), the type of person figuring in the cultural system that has to be eradicated. In contrast to Ach, Jaensch's science became deeply corrupted. His keynote lecture at the congress pre-shadows his 1938 book *Der Gegentypus*. "In psychological type research one encounters a basic form, which shows, in largest excessiveness, all the features against which the German movement turns itself."[23] It is the "lytic" type or S-type. This type is "liberalistic, egocentric, individualistic."[24] There is an "absence of all bonds,"[25] the lytic types "plays any role," being the "type of mimicry,"[26] there is "inner and outer instability,"[27] their intelligence is "free-floating."[28] The lytic type is "labile"; it is "unmanly" and not "heroic," and there is "weakness of volition." There is emotional "coldness and unsociability." This type comes about by "race mixture" and (both manifest and latent) "tuberculosis."[29] "The German movement is a *sanitary movement*, a movement of *cultural healing* and *culture enhancement*. By trying to end the predominance of the *Gegentypus*, it discharges an obligation, first in *Germany*, but with it in the whole *world*."[30] This salvation doctrine was worked out in exceeding detail in Jaensch's 1938 book (over 500 pages!). There is little psychology of language in the book, but it is a monument of the type of science promoted by the then chairman of the German Psychological Society.

Not surprisingly, the "lytic type" is widespread among Jews, because they are the product of race mixture. They are the main contributors to the "culture of decay" that has to be combated. It is especially Jewish psychologists (such as William Stern) who developed and applied intelligence tests that benefit the S-type children, eventually bringing them

[23] "Bei psychologischen Untersuchungen stößt man auf eine Grundform, die alle die Züge, gegen die sich die deutsche Bewegung wendet, in größter Übersteigerung zeigt." (Klemm 1934, p. 56).

[24] "Liberalistisch, egozentrisch, individualistisch" (ibid. p. 57).

[25] "Fehlen aller Bindungen." (ibid. p. 57).

[26] "Typos der Mimikry" (ibid. p. 57)

[27] "Dieser Typus würde bei seiner äußeren und inneren Haltlosigkeit ... überhaupt keine 'Welt' besitzen" (ibid. p. 57).

[28] "der freischwebende Intellekt" (ibid. p. 58).

[29] "ausgiebiger *Rassenmischung*." "bei *Tuberkulose* und anderen konsumierenden Krankheitsprozessen" (ibid. p. 58).

[30] "Die deutsche Bewegung ist eine *Virenzbewegung*, eine Bewegung der *Kulturgesundung* und *Kulturerhöhung*. Indem sie der Vorherrschaft des 'Gegentypus' in der Kultur ein Ende zu machen sucht, löst sie eine Aufgabe, zunächst für *Deutschland*, damit aber zugleich für die *Welt*." (ibid. p. 58).

into positions of leadership. And so it goes on. What to do about it? Jaensch writes: "When foreigners visit our institute, it happens occasionally that one moves to the race table, points to the Nordic type, and then, as recently an American did, says this about it: 'You would prefer to imprison and kill everyone who is not of this type . . .. With the *Gegentypus* you would do it?'"[31] But this is not at all the case. Rather, "we intend to heal the *Gegentypus* and to counteract its frequent appearance and its leading role in the cultural process, by preventive measures."[32] Jaensch did not live to observe the grand-scale healing process. He died, as rector of Marburg University,[33] in 1940, during an operation.

The 1933 "restoration" of the universities

On April 7, only weeks after Hitler's assumption of power, a law came into effect authorizing, and in effect compelling, universities to dismiss their Jewish members of staff, as well as political dissidents.[34] This affected some 20% of all German university staff. Among them were many scholars who had contributed to the psychology of language.

William and Clara Stern

Back in January 1933, William Stern had been lecturing in Holland. He had made earlier lecture tours in that country and was highly respected among his Dutch colleagues, such as physicist and educational psychologist Philip Kohnstamm.[35] In the course of the following summer, Stern was barred from lecturing and on 31 October he was prematurely pensioned off. In November he went to Holland to organize his and Clara's immediate future. Extremely tragic was the news they then received about William's staff member and loyal co-worker Martha Muchow. She had also been dismissed, not because she was Jewish, but because "she had identified herself with the work of a Jew."[36] She had then committed suicide.

Clara and William moved to The Hague in January of the next year. For a few months William did some lecturing in Holland, but mainly isolated himself to complete his *magnum opus*, *Allgemeine Psychologie* ("General psychology"), which was published by

[31] "Wenn Ausländer unser Institut besuchen, kommt es gelegentlich vor, dass einer von ihnen vor die Rassentafel tritt, auf den nordischen Typus zeigt und sich, wie kürzlich wieder ein Amerikaner, folgendermaßen ausläßt: 'Alle, die nicht von diesem Typus sind, möchten Sie am liebsten einsperren oder totschlagen.' Dieser Irrtum ist oben widerlegt. 'Aber mit dem 'Gegentypus' möchten Sie das so machen?'" (Jaensch 1938, p. 480).

[32] "daß wir vielmehr beabsichten, den Gegentypus zu heilen und durch vorbeugende Maßnahmen seinem gehäuften Auftreten und seiner Führerrolle im Kulturprozeß entgegenzuwirken." (ibid. p. 480).

[33] Jaensch had accepted the rectorship in Nazi uniform.

[34] "Gesetz zur Wiederherstellung des Berufsbeamtentums."

[35] Kohnstamm was also among the Dutch colleagues who were helpful to the Sterns during their stays in Holland.

[36] Eva Stern-Michaelis (1991, p. 137).

the Dutch publisher Nijhoff in 1935. Stern dedicated his book to Martha Muchow and to Otto Lipmann, a dear friend, who had suffered the same fate as Muchow. Meanwhile, the Sterns, still hoping to return to normal life, had moved their belongings to a small apartment in Hamburg. But also they accepted an invitation from William McDougall of Duke University to spend a sabbatical there. This they did, from October 1934 to May 1935, then returned to their Hamburg apartment. It was their Zionist daughter Eva who finally convinced them to emigrate to the United States. This happened while Eva saw her parents during their summer holidays in Luzern. Clara then packed their remaining belongings in Hamburg, while William went to Holland, from where the two of them departed, back to Duke University in North Carolina.

This was not the end of their trans-Atlantic travels, however. They returned to Holland in 1937 to assist their daughter Hilde in emigrating to the United States with her two children. Hilde had just completed two years of imprisonment in Berlin; she had sheltered "enemies of state," some leftish, probably communist, friends. Her two children had during that period been taken care of by a Quaker school in Holland. The Sterns returned with Hilde and her children. During that short stay in the Netherlands, William designed a new book on the psychology of space and time.[37] It was not to be written. During 1938, on 27 March, William suddenly died. Clara, now a United States citizen, then moved to New York. There she supported herself, according to daughter Eva, as letter censor (*Briefzensorin*). She died in December 1945.

What happened to their famous diaries? They had taken them to the Unites States, and William's former student Gordon Allport helped them to lodge them in Harvard's Widener library. However, nobody showed any interest in them. Ultimately, daughter Eva moved them as a permanent loan to the library of the Hebrew University. It was Werner Deutsch of the Max Planck Institute for Psycholinguistics in Nijmegen who, with Eva's unremitting support, had the full diaries transcribed. Since then they have been forever and for everyone electronically available.[38]

Ernst Cassirer

Ernst Cassirer (1874–1945), a close colleague of William Stern, had been a rector of Hamburg University, certainly exceptional for a Jew. He was a realistic man, understanding early what threatened to happen. Right after Hitler's access to power, Cassirer got an (already prepared) sabbatical in Switzerland, where he moved with his family. There he received various invitations from foreign universities. He first went to Oxford (1935–1937), then on to Göteborg in Sweden. From 1941 to 1944 he held a position at Yale. He then went to Columbia University as a visiting professor. There he enjoyed contacts with other emigrants, such as Kurt Goldstein, a cousin of his wife. He died there in 1945.[39]

[37] My sources for the Sterns in the Netherlands were Eva Stern-Michaelis (1991), Wilfred Schmidt (undated), and Lamiell (2010).

[38] http://www.mpi.nl/resources/data/stern-diaries

[39] The source of this information is Maas (2010, p. 149).

Heinz Werner

Another Hamburg colleague of William Stern was Heinz Werner, whose work on metaphor, speech physiognomy, and mental development we considered in Chapters 12 and 13. Like Stern, Werner was as a Jew dismissed during the summer of 1933. And like the Sterns, he first moved to Holland for a few months. He then accepted the invitation from Walter Pillsbury to come to the University of Michigan. Apart from one year as a visiting professor at Harvard, he remained at Michigan for ten years, dedicating himself to the study of learning disabilities. After a further four years at Brooklyn College, he moved to Clark University at Worcester in 1947. It was his first full professorship in the United States; he also became chair of the newly organized Psychology Department at Clark. Here Heinz Werner made his second and highly prolific career till his death in 1964. Many German emigrants, such as Stern, Cassirer, Goldstein, and Bühler, either did not live long enough or could not manage the cultural transition to become fully integrated in their new academic world. But Werner was not one of them. That happy fate he shared with Wolfgang Köhler, as we will see.

Kurt Goldstein and Adhémar Gelb

The fates of Kurt Goldstein and Adhémar Gelb, after their 1933 dismissal from Berlin and Halle Universities, respectively, were recounted in Chapter 12. Both of them first went into exile in Holland. There, Goldstein finished his *Der Aufbau des Organismus*, which was then, like Stern's book, published by Nijhoff. The title page gives as Goldstein's affiliation: "Former honorary professor at the University of Berlin." Whereas Goldstein then had 30 more years to go in his adopted country, the United States, Gelb was not given a new chance in his exile country Sweden, dying a lonely death there from tuberculosis in 1936.

A flashback should be added here. Twenty years before Goldstein was exiled because of his "race" he had published a little book himself, entitled *Über Rassenhygiene* ("On race hygiene" 1913). The monograph is largely about the "human race" in general. It discusses various factors that might contribute to "counter selection." One of them is war. The strongest men do the fighting and risk death. The weaker ones stay home and can procreate. Other factors affecting the quality of offspring include alcoholism, tuberculosis, mental disorders, and inbreeding. Race mixing (now meaning different human races) is probably another factor. "Mulattos, that is half-breeds of Whites and Negros, are likely to be mentally and bodily inferior to their parents."[40] Is there evidence for the deterioration of the human race? Yes, there is some evidence, or there is at least a noticeable increase in neurological disorders. What to do about it? Here Goldstein reviews some of the current eugenic programs in nine US states and Switzerland, but he has doubts about their effectiveness. We do much better to invest in education, Goldstein argues. By the end of the book, Goldstein specifically turns to "the white race." We can "consider it as

[40] "doch sollen die Mulatten, also die Mischlinge aus Weißen und Negern, geistig und körperlich minderwertiger als ihre Eltern sein." (Goldstein 1913, p. 22).

the most developed one and consider its increasing spread over the world as a sign of increasing development of the human race."[41] But success is not guaranteed. "Whether we will succeed in permanently keeping the white race as the leading one in the world, cannot be decided."[42] And then a discussion follows about "the yellow danger." This booklet shows how much Goldstein had been a child of his time and how little he foresaw the atrocities to which eugenics would lead in his own country.

Wolfgang Köhler

It has often been observed that the dismissal of Jewish university staff in 1933 encountered little opposition or protests from the remaining colleagues. The same would happen in Austria, after the 1938 *Anschluß*, as we will see. But there were remarkable exceptions, such as that of Gustav Kafka, who had cancelled the Dresden Congress. He resigned from Dresden University in protest against the dismissal of Jewish colleagues. Another one was Wolfgang Köhler. Köhler was the Director of the Psychology Institute at Berlin University, probably the most influential psychology institute in Germany. It was the center of Gestalt psychology, but ranged widely over the main areas of psychology. Right after the dismissal of his Jewish colleagues, Köhler, himself not a Jew, published an essay of protest in the *Deutsche Allgemeine Zeitung* of 28 April 1933 under the title *Conversations in Germany*. He first generally addressed the issue of competent, skilled, and above all patriotic Germans in all professions being put aside by the new regime. "Time and again, I hear the question: Why?" He then turns to the special case of the Jews. It may be true, Köhler admits, that the Jewish population has too large a representation in German leadership positions, but that cannot be a reason to depict them as "a lower, inferior form of mankind." My friends, Köhler writes, "don't want to agree with such a proposition." He then lists all the good things his friends are telling him about the Jews. They talk about the beauty of the psalms, the clarity and wisdom of Spinoza, the genius of Jewish physicist Heinrich Herz, or of contemporary Nobel Prize winning physicist James Frank, who had just been dismissed from Göttingen University. The deepest concern in these conversations is probably that one "feels the basis and foundation of one's moral world affected." The library of the American Philosophical Society, where I found this article in the Köhler archives, also contains a large number of spontaneous, approving letters and postcards from all over Germany, received by Köhler in response to his article.

Köhler also openly commented, in detail, on the new requirement to open each lecture with the Hitler salute. His audience of 200 responded in thunderous applause. Systematic harassment of Köhler and his institute began by the end of the year. An unannounced search took place in the institute by a Nazi student team, directed by a Mr. Henning, who sent a report of their "findings" to the rector of the university. Telephonic and written

[41] "Wir können diese deshalb als die höchstentwickelte betrachten und in ihrer zunehmenden Ausbreitung über die Erde das Zeichen einer aufsteigenden Entwicklung des Menschengeschlechtes sehen." (Goldstein 1913, p. 92).

[42] "Ob es uns gelingen wird, die weiße Rasse dauernd als die führende auf der Welt zu erhalten, ist nicht zu entscheiden." (ibid. p. 93).

protests by Köhler led to the rector's promise that this would not happen again. But it did, on 12 April 1934. This time Henning's report became quite concrete. It advised the rector to dismiss the "assistants Dr. Dunker and Dr. v. Lauenstein." The same should hold for "Elektromonteur Haag." In addition, the rector should consider moving the institute to localities that could be better surveyed. He should also consider imposing another organization on the institute, "which conforms more to our time and its spirit. Heil Hitler!" This time Köhler had enough. He resigned immediately as director of the institute. A detailed correspondence ensued between Köhler, the minister of science, and the rector of the university, which finally led to a temporary lull in the harassments of Köhler and his institute. At the end of the year Köhler went to Harvard to deliver the William James lectures. There he got a letter requiring him to pledge an oath of allegiance to the Führer. He also got a message that Kurt Lewin of his institute had been replaced by somebody else, without any consultation. Köhler replied that he refused the oath and that all of his dismissed staff should be reinstalled. Back in Berlin in the summer of 1935, he had his final dealings with Nazi officials. He resigned from the university. It had been "One man against the Nazis." as Mary Henle (1978) put it in the title of her account of this period in Köhler's life. His institute was basically dismantled by his Nazi successor and Köhler returned to Harvard and from there, in 1935, to Swarthmore College, where he stayed for 20 years. During the next decade he was at Dartmouth College. He was excellently integrated in his new country serving, for instance, as president of the American Psychological Association.[43]

David and Rosa Katz

Only a few years after David and Rosa Katz had published their *Conversations with children*, David was dismissed from Rostock University for being a Jew. He and his Jewish wife suffered harassment by the Nazi studentship and in the local newspaper. David first went to London and from there organized the future for his family. Rosa and the children did not leave Rostock till 1935, travelling via England to Stockholm. There the family settled. In 1937 David became the first professor of psychology at Stockholm University, a function he would hold till his death in 1953. Rosa became the leader of a child psychology center. In the four following decades she published on Montessori-based educational methods, on philological ability, and later on gerontology. In 1951 David Katz was the host for the 13th International Congress of Psychology in Stockholm, the first such congress after Paris 1937.

Not surprisingly, many of the Jews affected in 1933 could not believe what was happening. They thought it was probably just a temporary abberation of the new regime and things would redress themselves in the course of time. Even Wolfgang Köhler's article in the *Deutsche Allgemeine Zeitung* kept the hope that the regime would not completely ignore a rational argument, for instance the argument of how to replace all the competent professionals for "running the universities, the professional bodies, the individual

[43] I was in the audience when the tall, elegant Köhler gave a talk on memory at MIT in 1966.

businesses in the economy"—"that seems to be an impossibility to my friends." We saw how the Sterns decided to rent a new apartment in Hamburg after William's dismissal, not following their colleague Ernst Cassirer's example, who had organized his sabbatical already, during 1932. It was only under their daughter Eva's great pressure that the Sterns decided to emigrate in 1935. Two pioneers of psycholinguistics were particularly late in considering the definiteness of their fate in Nazi Germany, Max Isserlin and Otto Selz.

Max Isserlin

Max Isserlin, who had conceptually clarified the notions of paragrammatism and agrammatism, and who had discovered telegram style to be a (secondary) adaptation of the patient to speech need, was still directing the Heck hospital in Munich when the Nazis took power. The hospital contained the veterans' rehabilitation clinic that Isserlin had established in Munich following World War I. That clinic was immediately detached from the Bavarian state hospital, becoming subordinated to the Ministry of Internal Affairs. Its Jewish director was dismissed. But Isserlin remained in charge of the rest of the state hospital. In 1938 a new law came into force which ordained the dismissal of all Jewish doctors; Isserlin was discharged in July of that year. Still, Max and Ernestine Isserlin waited till 1939 before they finally joined their daughter Beata in England. The ultimate trigger was a visit from the Gestapo. The Isserlins settled in Sheffield, where Max died in 1941 at the age of 62.

Otto Selz

Whereas Isserlin survived the Nazi terror, Otto Selz did not.[44] Shortly after completing, still in Bonn, the second volume of his study of thinking, Selz was appointed professor at the Mannheim business school, which obtained the right to award doctorates in 1929. The same year, Selz was elected rector of the university for the academic year 1929/30. Although Selz was scientifically prolific during his Mannheim years, time turned out to be too short for him to actually take a student through to PhD.[45] In April 1933 Selz was suspended from his post and refused further access to his Institute of Psychology and Pedagogy, which ended his experimental work. Selz, a bachelor, still had some income, however, and stayed in his Mannheim apartment, working on theoretical issues. There is no indication that he received any support from Mannheim University officials or colleagues. In November 1935 Selz, of his own accord, resigned as a member of the German Society of Psychology.[46] He was still free to travel and in fact made some visits to Switzerland. In the wake of the terrifying *Kristallnacht* (10 November 1938), however, Selz was detained in Dachau. In mid December he was released again, presumably on the condition that he would emigrate (as would also be the case for Karl Bühler).

[44] My main sources on Selz have been Frijda and de Groot (1991) and Métraux and Herrmann (1991).

[45] With probably one exception, see Métraux and Herrmann (1991, p. 19).

[46] Reinert (1991, p. 16).

In mid May 1939 Selz went to Holland, ending up in Amsterdam. Philip Kohnstamm managed to organize a small stipend for him. Both Kohnstamm, professor of physics and director of the Amsterdam teacher college, and Geza Révèsz, professor of psychology at Amsterdam University, organized teaching and moderate research facilities for Selz. Selz stayed in Amsterdam when the Germans occupied the Netherlands on 10 May 1940. On 22 June, Selz sent a letter to Karl Bühler, then in Minnesota, asking him to mention him wherever there was action on behalf of emigrant scientists. It would be "frivolous if I would not focus my aspirations more intensively on America, under the new circumstances"[47] (i.e., six weeks after the German occupation of the Netherlands). Whether this letter reached the Bühlers in time is unclear, but after receiving a letter from Charlotte, Selz repeated his request in a second letter, dated 12 September. Nothing happened, however, in this respect: Selz stayed in Amsterdam. In 1942 he lost his German citizenship. Selz never accepted help from his Dutch colleagues to be smuggled out of the country. When, in 1943, the ultimate razzias of Amsterdam Jews took place, Otto Selz refused to go underground. He was rounded up and transported to Westerbork, the transit camp for Dutch Jews. On 24 August 1943, he was part of the human freight on train # DA 73 to Auschwitz. He died on that train, somewhere close to Auschwitz, on 27 August. His colleagues have often wondered why Otto Selz declined all help to evade his certain fate. There is no answer, but Selz had expressed his view that he had done nothing wrong, that he had been a good citizen; his logic was that, hence, nothing could happen to him. That, at least, is what Seebohm (1991) suggested.[48]

1933–1938: some further developments

Language and speech dwindled away as active topics in German psychology, at least I found nothing of this sort in the proceedings of the 1934, 1936, and 1938 congresses of the German Psychological Society. Psychologists became increasingly occupied, if not corrupted, by studies of race, military psychology, leadership (*Führertum*), education of character, and other such useful topics. Felix Krueger, the new chair of the Society, presented long, woolly opening addresses at the 1934 and 1936 congresses. But they were less, not more controversial than his 1933 address; we will presently return to Krueger. In 1938 the opening address of the congress in Bayreuth was by Jaensch, the new Society chair. It is, right after the Austrian *Anschluß*, a triumphant sketch of how the "old" psychology is going to be turned into a "real German psychology." Much had been wrong in psychology, for instance "the Jewish intelligence tests of William Stern." Many who had been tested that way "must have had the instinctive suspicion, that this form of people selection would, by necessity, always bring a heterologous (*artfremd*) intelligence on the higher educational path and with it into leading positions, hence facilitating a marked

[47] "Dennoch schiene es mir leichtsinnig, wenn ich unter den neuen Verhältnissen nicht meine Wunschziele wieder dringlicher auf Amerika richten würde." (Letter Selz to K. Bühler, 22–6–1940, Bühler archive).

[48] Seebohm (1991, p. 9).

counter selection . . . of an intelligence type strongly represented among Jews."[49] Some of us "came to truth finding in our discipline . . . from the instinctive drives of the blood and *at the same time* by way of psychological insight." Our science, which used to have a Cartesian, hence French design, is to be newly established on "the German movement" and in correspondence with the "German character" (p. 24). Jaensch castigated the journal *Acta Psychologica*, published in The Hague, which is in the hands of "Jews and emigrants" (p. 25). Psychology is "at the service of population-political eugenics" and will co-determine the "formation of our *Volk*" (p. 29). "We greet the man, who reestablished our state on an unshakable base. We greet the *Führer*! Unserem Führer Sieg Heil!" (p. 30). Party officials, including the chief of military psychology, a brigadier general, swarmed around. Nobody expected this to be the last congress under the Nazi regime.

Felix Krueger increasingly distanced himself somewhat from the regime. He also increasingly criticized Jaensch's theories. His 1935 inaugural address as rector of Leipzig University was still of orthodox National-Socialist character. But shortly after, the student association filed a complaint about Krueger who had, in a public lecture, called the altruistic and erudite German physicist, Heinrich Hertz, "a noble Jew." Krueger countered that he had never employed a Jewish assistant. He was, however, forced to resign as rector "for health reasons" in 1936. Shortly afterwards he resigned altogether from his professorship (Prinz 1985).[50] In 1934 some of his colleagues presented him with a Festschrift on the occasion of his 60th birthday. It contains one "psycholinguistic" paper by Sander. Friedrich Sander (1889–1971) of Jena University had been one of Wundt's last assistants. Sander was a Nazi, writing texts such as "The elimination of the parasitically proliferating Jewish race, as well as making infertile the carriers of inferior inheritance of one's own *Volk*, has its deeply ethical justification in this will to the pure gestalt of the German essence."[51] Sander's paper addressed the issue of bilingualism. Each child "is subject to the formative power of the community's language." The mother tongue is a main determinant of the child's *Weltanschauung*. A child growing up in a bilingual situation is in danger. It will not acquire a unitary conception of the world. The normal acquisition of the mother tongue is curbed and the child is hampered in becoming a normal member of the *Volk*. Bilinguals suffer from inner disunity. They show less normal right-handedness, they are susceptible to aphasia, they frequently make speech errors. The mission of this paper is political. In the German border regions, German children are forced to learn a second language. This is to affect the speech minority through the plasticity of its youth. But Adolf Hitler has rightly called this "a heavy fallacy in reasoning." Sander cites the 1924 study by Saer *et al.* in support of claims.

[49] "mittels der jüdischen Intelligenztests von William Stern geprüft worden . . . Mancher der so Geprüften mochte wohl instinktmäßig ahnen, daß diese Form der Menschenauswahl notwendig immer wieder dazu führen muß, einen artfremden Intelligenztypus auf den höheren Bildungsweg und damit in leitenden Posten zu bringen." (Jaensch in Klemm 1939, p. 10).

[50] See also the site of the history of Leipzig psychology, http://www.unileipzig.de/unigeschichte/professorenkatalog/leipzig/Krueger_89/

[51] Cited in Geuter (1994, p. 213).

In 1937 the Twelfth International Congress of Psychology was held in Paris, presided over by Henri Piéron.[52] It included a special symposium on psychology and linguistics, organized by Viggo Broendal from Stockholm. The symposium was conceived as a follow-up to the 1933 one organized by Delacroix, during the International Linguistics Congress in Paris. Broendal outlined the main problems in a grammar of oppositions. Louis Hjelmslev from Denmark worked out this theme in some formal, algebraic detail, whereas André Martinet applied it to phonology. Henk Pos from the Netherlands argued that linguistic oppositions work underground, that is beneath consciousness. There were presentations on interjections, and on sound change. Especially noticable was Karl Bühler's presence and his lecture (in German) on the Organon model of his *Sprachtheorie*, which had appeared three years before. The focus was on the functions of deixis and symbolization. Karl had come to the congress with Charlotte and with some 30 of their Viennese students. The weather was beautiful, the champagne flowed; it was a highlight in the Bühlers' life,[53] but their lives would drastically change within a few months.

This meeting of linguists and psychologists was the last one in Europe before the war. It was Révèsz from Amsterdam who in 1954 would organize the first such meeting after the war, on the topic of thinking and speaking, to which we will return in Chapter 15.

The Austrian *Anschluß*

On 12 March 1938, German troops marched into Austria without meeting resistance. In fact, the troops were heartily welcomed. Hectic events had preceded this first of Hitler's conquests. A desperate move on 9 March by the Austrian Chancellor to hold a plebiscite about a possible *Anschluß* on 13 March was prevented by a coup of the strongly organized Austrian Nazi Party on 11 March. Chancellor Schuschnigg stepped down and the referendum was cancelled. The Austrian president, however, refused to appoint Nazi Party leader Seyss-Inquart as chancellor. Hence, no telegram was sent by the Austrian government to offer *Anschluß* to the Germans. Hitler then faked such a telegram and marched in the next day. On 13 March the *Anschluß* was ratified, subject to a referendum. That was held on 10 April, supporting the *Anschluß* with a reported 99.7% of votes.

The fate of the Bühlers

Charlotte Bühler had travelled to London on 28 February. There she directed a "Parents Institute" which she visited twice a year for a few weeks. She got the news of the *Anschluß* on the very day and she called Karl, their last contact for several weeks. Karl was rounded up and imprisoned on 23 March on the grounds that he had supported the Austrian-Catholic government of chancellor Schussnigg and that he had been philosemitic in both his private life and in his institute administration (C. Bühler 1984, p. 27). Charlotte was, on 2 April, on her way to Oslo for a lecture tour, when she got this news. From there she moved heaven and earth to get Karl released. This finally worked with the help of an

[52] Proceedings edited by Piéron and Meyerson (1938).

[53] As reported by Charlotte Bühler (1972, p. 30).

unnamed Norwegian National-Socialist, who had been an honorary consul in Austria. Karl was, after almost 7 weeks, released on 7 May. An unsigned document of 21 May presents a "self-reported" curriculum vitae.[54] It is most probably the document that the officials composed, in interaction with Karl Bühler, to justify his release. It mentions, in particular, that Bühler had been politically uninvolved. He had, for instance, had many (named) employees who were active National Socialists, among them Konrad Lorenz. The document mentions the general scientific importance of his institute: it would host the 1940 International Congress of Psychology, with Bühler himself as congress president. It also mentions the relevance of the institute's research for military psychology. Karl had not agreed to divorce his Jewish wife, but he probably agreed to emigration as a condition for his release.

Already during Bühler's imprisonment, on 2 April, his Dean of Faculty had submitted a request to suspend Bühler and some of his colleagues "on political and world view accounts." Three weeks later, on 23 April, the enterprising Dean would send a letter to Karl Bühler (who was still in jail!) to announce his suspension. A longer list, including Charlotte, was submitted five days later. The Ministry accepted the Dean's proposal; Karl (and most likely also Charlotte) were pensioned from 31 May. A letter from the City Board, dated 28 April, stated that Karl's contract with the City of Vienna was to be terminated on 31 July.

Then the Bühlers' search started for new positions. In 1937 the two of them had received some kind of invitation from Fordham University in New York. After much hesitation on the part of Karl, they had accepted for the autumn of 1938. After the *Anschluß* and Karl's imprisonment, however, the Fordham offer did not come into effect, due to "certain personalities" (C. Bühler 1984, p. 28), by which Charlotte meant the Pope himself.[55] Whatever the truth of this story, Karl first settled their affairs in Vienna, in particular moving out of their house, and then went to a chalet in the German Schwarzwald in order to write. He moved to Oslo in October, together with daughter Ingeborg. Their son Rolf had already reached Oslo. While Charlotte took care of her children and her guest professorship in Oslo, Karl went to the United States in search of new positions. The situation turned out to be very difficult. The Bühlers had declined earlier offers not only to Harvard, but more recently to Berkeley. By now all these positions had been filled, often by Jewish immigrants. Egon Brunswik, for instance, Karl's former assistant whose wife Else Frenkel was Jewish, had been appointed to the Berkeley position. To make a long story short, the Bühlers ended up at St Thomas College and St Catherine's College in St Paul, Minnesota. This time, Charlotte had hesitated—she was so much better off in Norway. Karl, however, insisted; Norway was in danger. Charlotte left Oslo on 29 March 1940, two weeks before the German invasion there. Their Minnesota stay was interrupted by guest professorships

[54] Bühler archive. Prof. Achim Eschbach provided me with all relevant documents concerning this phase of the Bühlers' life.

[55] In her 1970 letter to Luria: "After which the Pope - nobody less - forbade Fordham to appoint us." (Bühler archive).

at Clark University in Worchester, Massachusetts. Eventually they decided to go to California, where their son had accepted a job at Cal Tech in Passadena. Karl accepted a job as clinical psychologist at the Cedars of Lebanon Hospital. Charlotte accepted a similar position at the LA County General Hospitals. They settled first in Pasadena, then from 1950 on in Hollywood. Karl died there in 1963. Charlotte went to Stuttgart in 1971, where she died in 1974.

Karl Bühler never managed to integrate in his new world, probably for several reasons. He was 60 years old when he moved to America—not exactly the age to begin a new career. The Bühlers had essentially refused all American offers until it was too late. When they finally arrived in the United States, there were no interesting vacancies left, let alone two in the same city. In addition, it transpires from his 1929 paper on American culture that Karl had had second thoughts about American culture. The new spirit in America is one of *homo technicus* Karl wrote. Man is perceived as a machine, shaped by its environment.[56] The future of human well-being is in the hands of technology.[57] Labor is the determinant of human dignity.[58] Behaviorism is the adequate theory for such portrayal of man (p. 646). Also, Karl Bühler's English did not match his gifted German speaking and writing. Charlotte, finally, mentions Karl's depression and one more difficulty: Karl felt he should acquaint himself with American semantics. But the predominant theory was Korzybski's, which did not inspire him (C. Bühler 1984, p. 35); I sympathize. Still, Karl Bühler was not idle, on the contrary. In Minnesota, for instance, he completed a monograph on traffic safety, for which, however, he could not find a publisher (C. Bühler 1965, p. 191). Achim Eschbach's detective work has unearthed a goldmine of manuscripts and papers produced both before and after his emigration, and Eschbach launched the project of publishing Karl's complete works.[59] Charlotte was more successful. In addition to her many lecture tours, she continued to be a prolific writer, both in German and in English. Her 1962 book *Die Psychologie im Leben unserer Zeit* became a bestseller. It was followed in the same year by *Values in psychotherapy.* She had become a leader in humanistic psychology. In 1960 the Bühlers travelled to Europe,[60] where Karl received the prestigious Wundt medal of the German Psychological Society during their congress in Bonn.

Let us now return to Vienna 1938. The speedy dismissal of Jewish and other university staff after the *Anschluß* did not lead to much expression of sympathy from their colleagues or students. Let us take Karl Bühler's case as an example. The university climate

[56] "*Der Mensch ist eine Maschine . . . Charakter und Persönlichkeit eines Menschen sind restlos das Produkt der Milieueinflüsse*" (K. Bühler 1929, p. 636).

[57] "*Die Zukunft der Menschheit liegt auf dem Gebiete der Technik.*" (ibid. p. 637).

[58] "*Ja, die Arbeit wird zum wertverleihenden, würdeverleihenden, zum adelnden Faktor des Menschen erhoben*" (ibid. p. 644).

[59] Several such papers by Karl Bühler appeared in Eschbach and Halawa (2005).

[60] It was the one time I met them, as a student at Leiden University. Charlotte presented a lecture there for the psychology department. Karl seated himself in the back row.

had changed since the Austro-fascists came to power in 1934. The research situation had not improved for the Bühlers, especially since their Rockefeller grants were cut back and eventually ended. But they kept the institute running, making use of their excellent network in the leading Viennese circles. That excellent network, however, was of little significance after Karl Bühler's imprisonment and suspension. There is no evidence that any member of the Psychology Institute's "honorary council," such as the archbishop, the major, or the school inspector, lifted a finger for Karl. Neither did Friedrich Kainz, a part-time extra-ordinary professor in the faculty, who had more than once solicited and received Karl's help. His letter of 10 May 1938 to the *Reichsstatthalter* did not concern the Bühlers, but his own salary. In October, Kainz was appointed interim director of the Psychological Institute, which had been without a director since Karl Bühler's suspension. In that role Kainz co-signed a letter on 19 November 1938 that estimated the value of Bühler's library, which was "offered for sale to the Psychological Institute." at RM 500,-. This was based on the expertise of the state librarian (and a colleague in the philosophy department), Victor Kraft, who co-signed the document. The amount of RM 500,- roughly corresponded to a month's academic salary. Bühler had left part of his library, which probably contained some 5,000 books, behind when he left for Oslo in October. Kainz's document speaks of "somewhat more than 900 volumes" offered for sale. It is not known whether Bühler had made his "offer" voluntarily, but Kainz's estimate was certainly not helpful. Bühler had already left for Norway and could not further influence the deal. And nobody knows what happened to the remaining books. We will return to Kainz in the final section of this chapter.

Another, much worse case was Konrad Lorenz's reaction to the *Anschluß* and Bühler's suspension. In Chapter 9 we mentioned Lorenz as one of Bühler's many brilliant students; others were Karl Popper and Paul Lazarsfeld. Lorenz, who had completed his MD in 1928, subsequently studied psychology under Bühler's direction from 1928 to 1933. This was completed by his promotion in animal psychology on 25 December 1933. Next, Bühler made considerable efforts to pave the way for Lorenz's habilitation. In fact, Bühler had to persuade Lorenz to go for a habilitation (and did so, personally, during a concert which they both attended[61]). Bühler started the procedure for Lorenz in May 1934, which involved getting the agreement and cooperation of relevant colleagues in psychology and biology. This Bühler did in spite of the fact that he was aware that Lorenz was an active National Socialist. Although the factual procedure once more changed for technical reasons,[62] Lorenz quickly moved to his habilitation in 1936, in close interaction with Bühler's assistant Egon Brunswik. Hofer (2001) convincingly shows Bühler's essential influence on Lorenz's ethology. Right after the *Anschluß* Lorenz became a member of the Nationalsozialistische Arbeiterpartei (NSDAP). Relevant in the present context is a letter by Lorenz, which he sent to his mentor, Prof. Stresemann, on 11 April 1938, the day after Hitler's referendum. The letter begins with expressing the "lasting festive mood"

[61] Hofer (2001, p. 147).

[62] Ibid. p. 147.

after the convincing plebiscite and then turns to Bühler. "Meanwhile, much happened here namely in terms of business, which should be known by the Mentor. The Professor of Psychology, Prof. Bühler, is not only gone, but incarcerated, it is not clear why, but it is certain that he has glossed over and lied about the descent of his fully Jewish wife, that he probably swindled currencies, he had Rockefeller funds. In addition, he was subsequently so intensively red and black, after the fashion of the moment, that that suffices as an explanation. At any rate, he won't return." He then relates a new plan of Dr. Auersperg, who had become interim director of "another Jewish institute." The plan is to acquire what is left of Bühler's institute. "The psychology of perception is precisely the great tradition of the former Bühler-institute . . . and A. hopes to get the equipment of the Bühler-institute, and to continue the tradition (be it on a much better biological base, an advantage of the medical people . . . over the human psychologists). That is all fine and good. But it requires, that a man will get into the Bühler-institute, who will not overlap with, but complement what A. planned, and that should be me, thinks A." And further in the letter goes on: "A professorship is not to be scorned, because I really feel the vocation and obligation, to gather a school and to intervene in the confused chatter of psychology." Together with zoology it will be possible "to squeeze human psychology and to put something new in its place, something *real*. Above all something really 'species-specific' German, because I must say (in strictest confidence), that for the experts, human psychology, also in its present German representatives, keeps being mixed with the intellectual legacy of the Jewish-chattering and word-orgiastic big Jews."[63]

There is no trace of sympathy in the letter for imprisoned Bühler. On the contrary, a coup is planned to appropriate his institute. "Opportunism" is an understatement. In another unpleasant letter of 16 April, Lorenz at least shows some sympathy. He owes Bühler much gratitude in many aspects, "though not at all in other respects." Bühler had

[63] "vor lauter dauernder Feststimmung."; "Inzwischen hat sich sachlich verschiedenes ereignet, was der Mentor wissen muss, nämlich. Es ist der Ordinarius für Psychologie, Prof. Bühler, nicht nur gegangen, sondern heftig eingesperrt, warum ist nicht klar, sicher ist, dass er bezüglich der Abstammung seiner volljüdischen Frau beschönigend gelogen hat, vielleicht hat er Devisen geschoben, er hatte Rockefellergelder. Ausserdem war er vorher hintereinander so intensiv rot und schwarz, je nach dem Zug der Zeit, dass das allein genügende Erklärung ist. Jedenfalls kommt er nicht wieder.."; "Kommissarischen Leiter eines anderen jüdischen Institutes"; "Nun ist aber gerade die Wahrnehmungspsychologie grosse Tradition des verflossenen Bühlerinstitutes, . . . und A. hofft, die Apparatur des Bühlerinstitutes zu kriegen und die Tradition fortzusetzen (wenn auch auf einer weit besser biologisch fundierten Basis, wie sie die Mediziner, auch Heinroth und Lorenz, so oft vor dem Humanpsychologen voraus hat). Das alles ist schön und gut. Dazu gehört aber, dass ins Bühlerinstitut ein Mann kommt, der sich mit dem von A. geplanten nicht überschneidet, sondern ergänzt, und das soll ich sein, meint A."; "Ein Ordinariat ist nämlich deshalb nicht zu verachten, weil ich wirklich die Berufung und die Verpflichtung in mir fühle, eine Schule zu machen und ordnend ins Durcheinandergerede der Psychologie einzugreifen."; "die 'Human' -Psychologie tot drücken und was Neues an ihre Stelle setzen, etwas *Wirkliches* wäre. Vor allem etwas wirklich 'arteigenes' Deutsches, denn ich muss (im strengstem Vertrauen) sagen, dass die Humanpsychologie auch in ihren heutigen deutschen Vertretern immer noch für den Kenner merklich von dem Gedankengut der jüdisch-daherredenden und wortschwelgenden Judengrössen duchsetzt ist." (Hofer 2001, pp. 154–155).

lied about his wife's descent and he had used "hostile statements" against National-Socialism. "Since Prof. Bühler has, however, repeatedly done me a favor and exerted himself for my habilitation, it would please me to be able to do something for him. . . . Prof. B. is after all a 'waverer' but not really a bad man, and the idea that he is really incarcerated, triggers my intense pity."[64] There is no indication that Lorenz did do anything for the confined Bühler. Bühler never knew of these letters. During his 1960 visit to Europe, he also visited his former student in his Max Planck Institute in Seewiesen. Lorenz writes about this visit: "Shortly before his 80th birthday . . . Karl Bühler visited me in my Max Planck Institute in Seewiesen and said to me during dinner quite casually: 'You know, Lorenz, what so much impresses me about you, is your invigorating epistemology.' This most humble of all people hadn't noticed, that I had learned from him precisely that which he praised."[65]

Sociologist and NSDAP member Gunther Ipsen was appointed to Bühler's vacant chair. Among the courses he would offer during the first semester were *Volkslehre* and *Rasse und Geschichte.* They did not last long with his teaching. Ipsen had to join the military in September 1939. Finally, Bühler's chair went to Hubert Rohracher, an EEG specialist, who had in 1938 lost his position at the University of Innsbruck because of "political untrustworthiness"; he never became an NSDAP member. He was drafted for military service, but then in 1943, to his own surprise appointed in Vienna. Shortly after his move there, the air raids on Vienna began and on 2 February 1945, Bühler's old institute was completely destroyed. Because Rohracher had never had any Nazi party connection, he kept his chair when the war was over. Within a year, Rohracher sent Bühler a most sympathetic letter, inviting him to return to his Vienna chair. If necessary, Rohracher would vacate his own position to make this possible. Karl, however, declined. Not only was the place devastated and without a chair for Charlotte, but, as Charlotte put it in her 1970 letter to Luria: "who wants to return to a place where one was kicked out?" In the same letter Charlotte complained about the total neglect they had experienced from their colleagues in Germany, "the only decent person was Rohracher."[66] There were others, relevant to psycholinguistics, who emigrated from Austria under the Nazi threat, among them Frieda Eisler.

[64] "Da mir Prof. Bühler aber immerhin zu wiederholten Malen Gefallen erwiesen hat und sich seinerzeit auch für meine Habilitation eingesetzt hat, so wäre es mir eine Freude, jetzt etwas für ihn tun zu können. . . . Prof. B. ist eben ein 'Rohr im Winde', aber doch kein eigentlich böser Mann, und die Vorstellung, daß er wirklich fest eingesperrt ist, erregt mein heftiges Mitleid." (cited from Taschwer and Föger 2003, pp. 82–83).

[65] "Kurz vor seinem 80. Geburtstag (. . .) besuchte mich Karl Bühler in meinem Max-Planck-Institut in Seewiesen und sagte mir ganz beiläufig beim Essen: 'Wissen Sie, Lorenz, was mich an Ihnen wirklich so beeindruckt, ist Ihre erfrischend richtige Erkenntnistheorie.' Dieser bescheidenste aller Menschen hat nicht gemerkt, daß ich, was er lobte, von ihm gelernt habe." (Letter to Eschbach 11–2-1983, Bühler archive).

[66] "aber wer wollte zurückkehren zu einem Ort, von dem man herausgeworfen war?" "der einzige anständige Mensch war Rohracher" (1970 letter to Luria, Bühler archive).

Frieda Eisler

In psycholinguistics Frieda Eisler (1907–1982, born as Frymet Leib in Tarnow, Galicia) became known as Frieda Goldman-Eisler. She obtained her Viennese PhD in German Studies in 1931, but had also studied psychology under Karl Bühler. She married in 1934 and the couple moved to London. There she began studying psychology at University College in 1938.[67] It became the place of her successful and pioneering further career in psycholinguistics, to which we will return in the next chapter.

Emil Fröschels

In 1938 Fröschels was dismissed from his Viennese chair for being a Jew. We discussed Fröschels in Chapter 10, as the founder of logopedics and director of the Newmann Klinik in Vienna. After his dismissal he moved to the United States and became one of the best examples of successful integration in the American academic world. After emigration he first spent two years at the Central Institute for the Deaf in St Louis. He then became founder and director of the speech and voice clinic of Mount Sinai Hospital in New York (1940–1949), later moving to the Beth David Hospital in the same town (1950–1955). Finally, he became the first director of the Alfred Adler Clinic for Individual Psychology, also in New York (in 1955). As early as 1948 he had published a carefully edited text on speech therapy, involving American authors only. Also new were his studies of half-waking states, transitions from full consciousness to sleep. His 1955 paper reviews them from the psycholinguistic perspective. Speech in these states is basically grammatical, he claims, in spite of Kraepelin (of whom he only considers quoted data). There are many neologisms, there is much nonsense, but paragrammatism is exceptional, in both his self-observations and in the speech cited in psychoanalytic publications. At least, that is his claim.[68] And why should that be? Because in these states, words are "somehow, the envoys which transfer messages from the id to the ego . . . these envoys are supposed to behave, forced by some kind of ceremony to which everybody must submit . . . These messages, sent into that kingdom from the so-called unconscious are permitted to enter only in the form of the grammar-modulation which is in use in the realm of the conscious mind." (p. 50). This is grammar as camouflage. Fröschels had eventually embraced psychoanalysis.

Roman Jakobson

In Chapter 10, we considered Roman Jakobson's "years of homeless wandering from one country to another," as he called it himself, after the Nazi invasion of Czechoslovakia in 1939. He went first to Denmark, where he met Hjelmslev, then to Norway and Sweden, meanwhile completing and publishing his *Kindersprache, Aphasie und allgemeine Lautgesetze.* In June 1941, Jakobson finally arrived in New York, where he first taught at the French-Belgian École des Hautes Études, which was hosted by the New School for

[67] See Maas (2010, Vol. I, pp. 173–174).

[68] Of the 28 self-observed speech examples, reported in Froeschels (1946), at least six are ungrammatical in my judgment.

Social Research. From 1945 to 1949 he held a professorial position at Columbia University, but then moved to Harvard University, where he stayed till the end of his academic career.[69] We must, finally, consider the fate of Jakobson's dear colleague, Nikolaj Trubetskoy.

Nikolaj Trubetskoy

Trubetskoy was Karl Bühler's close colleague in linguistics in Vienna. They inspired each other in their work, as we saw in Chapter 10. The Russian emigrants formed a special target for the Nazis. They had, on the one hand, fled the communist regime, but would they really be cooperative in the Nazi anti-Semitic policy? Some were, but others were not. Trubetskoy belonged to the latter faction. In 1935 he published a paper (in Russian) on anti-Semitism, in which he describes Russian anti-Semitism as incompatible with Russian tradition. This put him on the Nazis' black list, just like Bühler. But he was not immediately imprisoned after the *Anschluß*, since he was in hospital because of his chronic heart disease. After his release, the Gestapo entered his home for a thorough search; it caused Trubetskoy to die from a heart attack.[70]

German neurologists in war time

Our review would not be complete without looking at the neurologists who had earlier contributed to aphasiology and who stayed in Germany during the war. In 1924 Karl Bonhoeffer had published a paper on the sterilization of the "mentally inferior." His paper was triggered by a proposal of a Dr. Boeters to the Saxony state government to consider sterilization of all people born blind or deaf or with learning disabilities, and people suffering from epilepsy and mental illness. In addition, sterilization should be considered for criminals and persons who had two or more illegitimate children. Bonhoeffer carefully analyzed all arguments for such a program and concluded that it would serve no purpose. Eugenics would be of very little effect. The paper is entirely technical, however. There is no discussion of ethical arguments. Bonhoeffer did not change his convictions, however, during the war. Ernst (2001) mentions him as a leading figure in the opposition to the misuse of psychiatry within a euthanasia program.

Also mentioned by Ernst and others is Karl Kleist (Ernst 2001; Meyer-Lindenberg 1991). Just one week after Hitler's access to power Kleist (1933) presented a paper on eugenic sterilization to the Frankfurt Medical Society. It is a response to legal proposals that were already circulating. Kleist essentially did the same as Bonhoeffer in 1924: he discussed the arguments and the effectiveness of all measures proposed. His conclusion was also the same as Bonhoeffer's: he rejected the proposals and made his own instead: "A seriously diseased person can be surgically sterilized if he/she consents and when, according to scientific medical and genetic insight, his/her offspring can be foreseen, with

[69] And where I met him in 1966, when he participated in the seminars of the Center for Cognitive Studies.

[70] See Maas (2010, Vol. I, pp. 826–836) for much more detail.

great probability, to have serious genetic mental or bodily malfunctions, or when children with serious genetic defects have already been produced by that person."[71] However, on 14 July of the same year the Law for the Prevention of Genetically Defective Progeny came in force. It decreed involuntary sterilization of persons carrying a hereditary disease. This included "subnormals, the congenital deaf, the congenital blind, schizophrenics, the deformed." Such patients had to be reported by the hospital to special courts, who would take decisions. Within four years some 300,000 patients were sterilized. What should a director of a neurological clinic, like Kleist do? This terrifying problem multiplied when Hitler's decree of October 1939 required euthanasia of "unworthy" individuals. All state institutions would have to report patients who had been ill for more than 5 years and were not able to work (and "Jew" was one of the items in the questionnaire). A committee of 48 doctors then took decisions and the selected patients were sent to special centers where they were gassed or otherwise killed. The committee also handled a similar program concerning young children. By 24 August 1941 some 100,000 patients had been killed, four-fifths of hospitalized psychiatric patients. Public opposition induced Hitler to stop the program at that date. However, it was continued as "wild euthanasia" and the killing now took place in the hospitals themselves, often through starvation. By the end of the war an estimated 200,000 adults and 6,000 children had been killed. The German medical world had become totally corrupted. Again, how could Kleist manage such a situation without precedents? Neumärker and Bartsch (2003) reviewed Kleist's dealings during this period. Kleist was criticized in 1933 for keeping his Jewish doctors and for continuing to treat Jewish patients. His was "the Jewish clinic." As an inspector of mental institutions in Hessen he complained about the "starvation treatment" of psychiatric patients, after which he was dismissed as inspector. Kleist told his staff not to diagnose in terms of "genetic neurological disease" or to diagnose "schizophrenia" or "manic-depressiveness." He created his own diagnostic guidelines and scales, and saw to it that they were applied. It was a balancing act to keep his clinic functioning. As did most medical officials, he became a member of the NSDAP, which provided some protection.

Somehow, Kleist continued focusing on his science and on his role as a neuroscientist. Under pressure from the regime, a compulsory Association of German Neurologists and Psychiatrists had been established. In 1936 its annual convention took place in Frankfurt, Kleist's place of work. The opening address of its rabid Nazi chairman, Ernst Rüdin, is an appeal, if not a summons, to support the preservation of the race, among other measures through sterilization of patients suffering genetic nervous diseases. In his closing statement Rüdin hails and greets "the innovator of our perspectives on medicine in the service of genetic and racial care"[72]: "Unser Führer Adolf Hitler Sieg Heil!" But then, the further

[71] "Eine schwer erkrankte Person kann operativ sterilisiert werden, wenn sie einwilligt und wenn nach den Lehren der ärtzlichen und erbbiologischen Wissenschaft bei ihrer Nachkommenschaft mit großer Wahrscheinlichkeit schwere geistige oder körperliche Erbschäden vorauszusehen sind, oder wenn von ihr bereits schwer erbkranke Kinder erzeugt worden sind." (Kleist 1933, p. 116).

[72] "den Erneurer unserer Anschauungen über Medizin im Dienste der Erb- und Rassenpflege." (Rüdin 1937, p. 5).

presentations and discussions were mostly scientific-technical. That was also true for Kleist's own lecture and extensive discussion in response to questions. In particular, Kleist responded to accusations of being a localizationist. "Since my first brain pathological studies, now more than thirty years ago, I have never been a supporter of rigid localization."[73] Kleist then mentioned two non-localizationist "laws" that he did support. First, a law of "division of labor" in the nervous system, the assignment of different operations to different organic substructures. Second, a law of "plasticity," the adaptability of the nervous system to varying conditions of life and work. But at the same time Kleist criticized, even ridiculed, the then dominant holistic (*ganzheits-*) theories of brain function.

When his clinic was bombed in 1945 and his patients were assigned to the Eichberg asylum, a notorious euthanasia clinic, Kleist managed to interfere and transfer them to a safe place. In 1946 the Nürnberg Medical Crimes Trial judged Kleist as having opposed the inhuman medical practice of euthanasia and he was rehabilitated. After the war Kleist received many prizes and awards, among them an American one. There is no doubt Kleist did everything in his power to protect and treat his patients, but his power was limited under the extreme circumstances. He did not fight on the barricades; that would certainly not have served his patients.

Kleist's final contribution to aphasiology was his 1959 edition of three lectures on aphasia and amusia. They provide detailed neuro-architectonic analyses of a number of clear cases of people with aphasia. Really new was his re-interpretation of "conduction aphasia," in quotes because Wernicke's old theory of a disruption of the conduction from sensory to motor speech areas could no longer be maintained. Conduction aphasia had become the term for the patient's inability to repeat stretches of speech. Kleist's analyses of four such cases put the finger on another type of conduction involved in the syndrome, namely conduction from the right to the left hemisphere. In at least three of the four cases, the right hemisphere was, or had become, involved with speech perception, whereas speech production from the left hemisphere remained intact. This arrangement induced limited "conduction" from sensory (now right hemisphere) to motor (left hemisphere) speech areas, just as Wernicke had hypothesized.

One question I have not seen answered in the literature: What happened to aphasic patients during the Third Reich, i.e, patients at home, patients in clinics, in nursing homes? There is no reason to be optimistic about the answer.

Friedrich Kainz

When we finally turn to Friedrich Kainz (1897–1977, Fig. 14.1) in this chapter, we are at the same time substantially exceeding its temporal boundary. Kainz was only 48 years old by the end of World War II and still had over three productive decades to go; in that sense

[73] "*Ein Anhänger einer starren Lokalisation,* wie die Herren *Lange* und *Bumke* meinen, bin ich übrigens seit meinen ersten, jetzt über dreißig Jahre zurückliegenden hirnpathologischen Arbeiten nie gewesen." (Kleist 1936, p. 337).

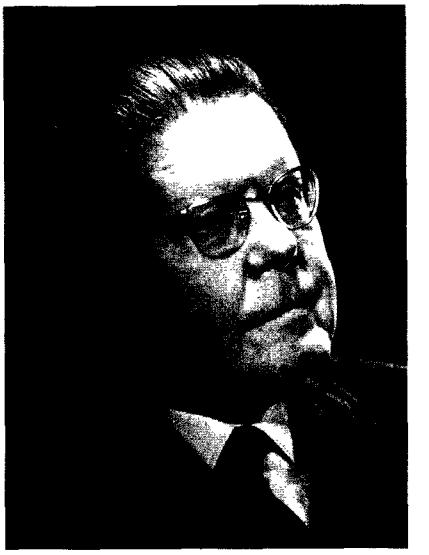


Fig. 14.1 Friedrich Kainz. © Austrian Academy of Sciences, 2011.

this final section is transitional to the next and last chapter of this book. But Kainz also fits the present context. He had studied with Karl Bühler, among many others; he became interim director of Bühler's institute after Bühler's emigration; he contributed to a war volume of Germanistic supporters of the regime and during the war he published the first two volumes of what would become his life's work, his six-volume *Psychologie der Sprache.*

Friedrich (Fritz) Kainz was born in Vienna. Immediately after completing his grammar school education in 1915, he volunteered for military service and fought on fronts in Russia, Italy, France, and Belgium, being wounded twice. After his second injury he was granted study leave and began a study of philosophy and philology at the University of Vienna. He was very studious and opted for breadth, following courses not only in philosophy, German linguistics, and various languages, but also in history, art history, and musicology (he was a gifted bassoonist). After receiving his PhD in 1921 he studied for his habilitation. It was attained in 1925 and was a study in aesthetics. He also followed courses in anatomy, physiology, psychiatry, and again psychology, now with Bühler. If it was your ambition to become a full professor at Vienna University, you had to be very patient. It took Kainz a quarter of a century, till 1950. Meanwhile he went through all intermediary stages of *Privat-dozent* to honorary and salaried extra-ordinary ranks. Before the war, his income was largely obtained by teaching courses and doing library work in Vienna's Pedagogical institute, teaching in adult education centers, and lecturing at the university.

When the Nazis came to power in 1938, Kainz almost immediately lost his teaching assignments at the Pedagogical Institute; these would go to more outspoken National Socialists. On 10 May he applied for a salaried professorship. In his letter he argued that all his earlier applications had been declined because he had been participating in a team

of intellectuals in support of an *Anschluß* of Austria to Germany. The Versailles peace treaty of 1918 had prohibited a (re-)union of the two countries, but in both there were active, public committees for the promotion of such unification. These committees counted many prominent intellectuals and politicians as members. Kainz contributed a paper to a 1930 volume on cultural, political, and economic aspects of unification.[74] This book opens with two forewords, one by the chair of the German parliament (*Reichstag*) and another one by the chair of the Austrian parliament (*Nationalrat*). Kainz's contribution to the book reviews Austrian literary history in its relation to the wider German context. He concludes his analysis by claiming the total unity of the German *Volk* and of its cultural region. As early as 1930 Kainz attributed the deep cause of this unity to race and blood. The essential identity of Austrian and "pan-German" literature is due to the "elementary fact, that, considered from the racial-anthropological perspective, the people living in Austria belong to the German racial type. . . . It is only due to the common blood, that a joint orientation on homogeneous cultural ideals remains effective."[75]

The new officials were, however, not impressed by Kainz's letter; nothing happened. But things were better when he became interim director. On 11 December Kainz was given tenure as extra-ordinary professor, with a teaching assignment in aesthetics and psychology of language. Kainz applied for NSDAP membership, which was, in a first round, declined.[76] He re-applied, however, this time successfully.

Kainz was an armchair scholar in the first place, never a serious political activist. But with many of his university colleagues he shared a good deal of opportunism. In 1941 he joined in publishing a scholarly volume on the German language in its relation to the German soul. Among the contributors were Nazi linguists such as editor Maurer, Weisgerber, and Koch. Franz Koch's preface begins with "Total war, as we experience it, is not only a military but also a spiritual-cultural polemic of the largest degree." What it is to be a German? The present effort is for "Germanistic science to contribute to this self-pursuance of our *Volk*." May the young people "and with them our *Volk* . . . gain certainty, that also the German spirit doesn't run idle, but goes for decisions, pushes out frontiers and also wrestles for a new organic order of his *Reich*."[77] And Weisgerber adds insights such as: "then our mother tongue will also meet the new grand challenge that is

[74] Kainz cites the book in his letter (Kleinwaechter and Paller 1930). I am grateful to the Archive of Vienna University for giving me access to this and other Kainz documents.

[75] "ist der einfache Auswirkung der elementare Tatsache, daß das in Östereich lebende Volk, rassenmäßig-anthropologisch betrachtet, in den Bereich der deutschen Rassentypik hineingehört." . . .. "Nur zufolge des gemeinsamen Blutes ist es möglich, daß eine gemeinsame Zielrichtung auf homogene Kulturideale wirksam bleibt." (Kainz 1930, p. 260).

[76] Letter from Ortsgruppenleiter, April 1941.

[77] "Der totale Krieg, wie wir ihn erleben, ist nicht nur eine militärische, sondern zugleich auch eine geistlich-kulturelle Auseinandersetzung größten Maßes."; "von Seiten der germanistischen Wissenschaft einen Beitrag zu jenem Selbstfindungsstreben unseres Volkes zu leisten"; "Mögen sie und mit ihnen unser Volk aus dem gewollten die Gewißheit schöpfen, dass auch der deutsche Geist nicht müßig geht, sondern Entscheidungen sucht, Grenzen verschiebt und um eine neue organische Ordnung auch seines Reiches ringt." (Franz Koch's preface to Maurer (Ed.) 1941, pp. v, vi, ix).

hers in our day: to ensure the German victory and to accomplish the universal force of the German spirit."[78] Kainz contributed in his own, characteristic way. His paper is a long-winded sentimental exposition of what German, mostly Romantic, authors from Herder and Humboldt on to Grimm, the Schlegel brothers, and Fichte had been saying about the wonderful relations existing between *Volk* and the German language, with occasional outpourings such as: "In that way he [A.W. Schlegel] hits upon lines of thought, that are telling for a modern blood-, race- and soil theory of language."[79]

Kainz's first publication on the psychology of language appeared in 1936. The drawn-out 60-page journal paper is on the psychology of language functions. Starting with Plato, it ends up with Bühler's Organon Model, describing the functions of expression, evocation, and representation in exceeding detail. In passing, it takes issue with the Wundt–Dittrich discussion of the dialogical character of speech, which Dittrich had argued to be essential. Wundt's reaction had been that language fully functions in "silent thought" where there is no interlocutor. Dittrich's response had been that in that situation speaker and listener, one explaining something to the other, coincide in the same person. Here Kainz takes sides with Wundt. This "hearer" already knows what the "speaker" means. Dittrich argues as if "speaker" and "hearer" do not have the same brain. Also in passing, Kainz refers to Jaensch's type psychology, which can provide "rich gains" for the psychology of language.

Then, five years later, Kainz initiated his *magnum opus* in five "parts" filling six volumes. The first volume appeared in 1941, the last one in 1969. The total size of this work amounts to no less than 3621 pages. Kainz mentions that these volumes resulted from the psycholinguistic courses he presented in Vienna. And indeed, in a way they are texts. But they also surpass that character in that Kainz clearly aimed for full comprehensiveness. Not only did he treat every thinkable topic under the heading of "psychology of language," but he took pride in dealing with every single publication he could lay his hands on. There are references to thousands of papers and books. The volumes indeed approach comprehensiveness in as far the German language literature is concerned since about a century's worth of material is involved, but there is also occasional reference to French and English language sources.[80] The work, therefore, is as much a handbook as a text, although Kainz in his preface to Volume I explicitly rejects that idea: "The encyclopedic-multifaceted point of departure aspired to is not a handbook-encyclopedia."[81] Much had to remain untreated, Kainz sighs in his humble pedantic way. It is, however, indisputable that Kainz's six-volume work has never been replaced as the Guinness world record for

[78] "dann wird unsere Mutterspache auch der neuen großen Aufgabe gerecht werden, die ihr in unseren Tagen gestellt ist: den deutschen Sieg zu sichern und zu vollenden in der Weltgeltung deutschen Geistes." (Weisgerber 1941, p. 41).

[79] "Dabei kommt er zu Gedankengängen, die für eine moderne erbbiologische Blut-, Rassen- und Bodentheorie der Sprache bedeutsam werden." (Kainz 1941a, p. 122).

[80] And occasional references to Dutch, Russian, and Italian publications.

[81] "die erstrebte enzyklopädisch-vielseitige Grundhaltung ist keine Handbuch-Enzyklopädie. Ich mußte manche Fragestellung, manches Forschungsergebnis unerwähnt lassen." (Kainz 1941b, p. viii).

a psycholinguistic handbook, let alone for a single-authored one. In the preface of Volume III (1954) Kainz eventually conceded that the work could be conceived of as a handbook.

Kainz published his first two volumes during World War II, in 1941 and 1943. Volume I treats the "basics" of the psychology of language. Here Kainz largely follows Bühler, though suggesting that he does not. Bühler did not carefully distinguish between essence and functions of language, according to Kainz. Bühler's *Darstellung* is the essence of language; it is a system of "representational sound signs" (*darstellende Lautzeichen*). Bühler's "expression" (*Kundgabe*) and "evocation" (*Auslösung*) are only subsumed functions of language, because they presuppose its essential representational character. Kainz then provides an elementary introduction to linguistic structure, going from phonology (where he cites Trubetzkoy, Sapir, and Hjelmslev) to words, inflexions, and phrases. However, sentences should not be treated as linguistic structures. They are, rather, aspects of *parole*, i.e., psychological events. They will often follow Wundt's schema: there is a total thought, which is subsequently elaborated into its parts. But they can also follow Paul's schema, especially in lively conversation: there are first bits and pieces, which combine into phrases and sentences. A third chapter deals with functions of language. It recapitulates the literature on the expressive and evocative functions of language, then treats "secondary" functions. Here Kainz lists aesthetic, ethical, magical-mythical, and logical functions of language. Ethical functions, for instance, include euphemisms, courteous language, lying, sophisms, and hyperbole. The final chapter reviews the literature on the origins of language. Kainz then pleads for the development of a "paleo-psychology" that should deal with the "mental ascent of mankind" (p. 310). Both biologists and anthropologists have theorized about the use of tools, the evolution of the hand, the mastery of fire, the evolution of the brain and so on. Psychologists should add to this by developing a comparative, genetic theory of mind and in particular of language. That, in fact, becomes the topic of Kainz's second volume.

Volume I was sold out. A second, unchanged edition appeared in 1954. A surprising detail is that Kainz did not even adapt the original preface: it announced the whole planned series of volumes. At the time, four were planned. The fourth volume, Kainz announced, would be about "special" psychology of language, in particular the "relation between, on the one hand, psychology of language and, on the other hand, folk and race psychology."[82] It will develop a "psychology of the German language." "Theme is the mental basic structure of the German *Volk* and its creative affirmation in the linguistic domain."[83] The preface also mentions names of leading authors, not including Bühler.

Volume II is a comparative treatment of language varieties that are not "fully developed." The book consists of chapters on child language, the language of "primitives,"

[82] "Das Verhältniss zwischen Sprachpsychologie einerseits, Volks- und Rassenpsychologie anderseits." (Kainz 1941b, p. vi).

[83] "Thema ist die seelische Grundstruktur des deutschen Volkes und deren schöpferische Bekundung im Bereich des Sprachlichen." (ibid. p. vii).

animal "language," the language of people with aphasia, mental illness and learning disabilities, language in semi-consciousness, sign languages and other signing systems, and, finally, "reduced" languages (meaning pidgins and creoles). The stated aim is: "What can be gained as insights in the essence, function and origin of language from the structure and genesis of the phenomena discussed?"[84] This second volume was the only one that was thoroughly revised in a new edition. The 1960 version adds another 100 pages, in spite of the fact that the chapter on animal "language" was entirely removed. It was published as a separate monograph (Kainz 1961). The new preface stresses the inverse relation existing between the development of language (whether onto- or phylogenetically) and language diminution (as in pathology and creolistic reduction). The approach is highly similar to Heinz Werner's of 1940 (discussed in Chapter 13), but Werner's book is not cited.

The chapter on child language reviews the literature on stages of acquisition and explicitly discusses the nature–nurture issue. Kainz's position is that Saussure's "langage" is innate. It is the child's "preformed language ability"[85], which is species-specific and matures in a genetically fixed order.[86] Saussure's "parole (langue)," however, the material content and process of the mother tongue is taken from the environment. A major mechanism here, which is not limited to language, is the imitation drive of the child.

The next chapter on "language of the primitives" starts out by dismissing as untenable traditional claims. It is not the case that the languages of "primitives" are themselves primitive or close to primordial language. They are "full languages" (*Vollsprachen*, p. 173), often much more developed than the culture in which they function. The purpose of studying them is in the first place to obtain a genetic-comparative window on the psycholinguistic universality of language, which will shed some light on the origins of language. After this hopeful start, however, we are showered with prejudiced statements about primitive language and thought. Primitive thought is "hypologic" (p. 184), sometimes denoted by "pre-conceptual Negro-metaphysics." There are primitive languages that are so deficient that they cannot be spoken without additional gesturing (p. 174). Because "primitives" have concrete but no abstract thought, they often have languages that are rich in concrete vocabulary and in rampant morphology and "polyonymy" (i.e., synonymy; pp. 187, 220). This richness emerges because "primitives" "strive for imitating everything perceived and to describe it with their own sounds."[87] These are "vocalic gestures." Ewe-speaking Negros have forty different ways for saying "go" (p. 202). "Primitives" speak "in a drawing and painting manner" (p. 189). Patriarchic societies tend to have prefixed languages, matriarchic ones suffixed languages. This is because the "aristocratic-spacious" *Weltanschauung* of the patriarchic societies prefer subordinal divisions as

[84] "Was kann man aus Struktur und Genesis der erörtenten Phänomene an Einblicken in Wesen, Leistung und Entstehen der Sprache gewinnen?" (Kainz 1941b, p. iv).

[85] "eine anlagemäßig präformierte Sprachbegabung." (Kainz 1943/1960, Vol. II, p. 138).

[86] "ein reifungsmäßig in den Wesenszügen bestimmter, in der Abfolge der Entwicklungsphasen erbmäßig festgelegter Vorgang." (ibid. p. 136).

[87] "Dieser Reichtum rührt dem Streben her, alles Wahrgenommene nachzuahmen und vermittels eigener Laute zu beschreiben." (ibid. p. 202).

opposed to the more democratic, personal value system of matriarchic peoples (pp. 192–193). "Primitives" have an "enumerating" way of speaking, like children and people born deaf who cannot speak (as van Ginneken said about the speech of Dutch Jews). Speech sounds that children have trouble with (like *r*) are often also absent in primitive languages. But then, they still use clicks, which are also apparent in infant speech. More generally, the more developed a people, the more frontal their articulation becomes. In short, Sapir's school passed Kainz by completely.

Kainz then reviews the classical localizationist and the more recent holist, gestaltist theories of aphasia (which he further works out in Volume IV, pp. 53–55). Eventually, he opts for a position close to the traditional view, which was certainly not standard at the time of writing. But then, dealing with the language of the people with mental illnesses we find sweeping statements like the following: "Among the mentally ill, as well as among children and primitives one finds a chattering imitation of speech sounds, where the sense of these constructions – if at all there – recedes behind the speech-motor production delight and the pure functional pleasure in complacent motor-articulation."[88]

The chapter on sign language does not go beyond the dominant standard view. Deaf sign language has to make do with telegram style, which marks its poverty (p. 592). There are no signs for abstract concepts and the concrete signs are highly ambiguous. The best, more conventional versions were developed in analogy to spoken language, and are therefore "artificial" in nature. In even the most developed sign language systems, the lexicon strongly dominates the syntax (p. 604). There is a lack of grammatical means. Sign language is uneconomic and sluggish, like primitive languages.

The final chapter on "reduction languages" argues that, as with aphasia, a language can loose its complexity. That happened, Kainz argues, with Afrikaans. Under the influence of creolization, the seventeenth century Dutch spoken in South Africa and Namibia lost most of its inflection. Kainz takes that (and other features of creolization) as a sign of reduction. It is a primitive trait of language, whether children's, "primitive" people's, or reduced, to lack auxiliaries and inflection (p. 688). The grammar is "simplified." Here Kainz essentially expresses Wundt's view. Inflectional languages are at the top of the hierarchy. A language, like Afrikaans, loosing its inflections is like a ship loosing its sails. The notion that the complexities and affordances of a grammar can reside anywhere in the system had not dawned in Kainz's writing. Again, the modern work on American-Indian languages was completely lost on Kainz.

Kainz had produced and published the almost 1000 pages of his first two volumes within three years, war years, in relative isolation. As a veteran, rather seriously wounded during World War I, he was not conscripted like many of his professorial colleagues. But then times changed. His institute was bombed in February 1945. Whether there and then

[88] "Bei Geisteskranken wie bei Kindern und Primitiven findet man ein plapperndes Wiederholen und Nachahmen von Sprachlauten, wobei der Sinn dieser Gebilde - wenn überhaupt vorhanden - zurücktritt hinter der sprechmotorischen Produktionslust und der baren Funktionsfreude an selbstzweckhafter Artikulomotorik." (Kainz 1943/1960, p. 419).

is unknown, but Kainz lost his almost complete manuscripts for Volumes III and IV.[89] After Austria's surrender, Kainz was put on leave to await the outcome of a purging operation. On 10 June Kainz submitted a statement declaring that he had only become a party member "to save his existence," but that he had never visited party meetings and that he "had never used the Hitler salute, except at official occasions." A few months later, on 22 November, the committee provided Kainz with a testimonial of no objection; Kainz could take up office again. In 1950 he was appointed full professor of philosophy at Vienna University, a position he held till the end of his career in 1969. He became a full member of the Austrian Academy of Sciences in 1955 and received various awards, among them, in 1977, the Austrian Honorary Medal for Science and Art. Let us now follow Kainz's work during these post-war years.

Volume III of Kainz's *Psychology of language*, published in 1954, forms its psycholinguistic core, in that it discusses the processes of speaking, speech understanding, and conversation. The volume opens with a chapter on brain localization. As mentioned, Kainz stays away from the "holist" position. There exist dedicated cortical speech regions, in particular the classical sensory and motor speech areas. They should not be viewed as sites where sensory or motor images are stored. With Pick, Kainz considers them as "switch and coordination loci."[90] They are functional loci, storing complex operations. The operations involved in speaking and speech understanding rarely become conscious. That occasionally happens in case of "speech need" while making speech errors or searching for words or formulations. Here Kainz feels pressed to take a position regarding the notion of "sub-" or "unconsciousness." He basically follows Wundt on this issue. Psychology is about consciousness. There exist non-conscious operations, but they are physiological in nature. It adds nothing to call them "unconscious." A comprehensive treatment of speaking and speech understanding must therefore involve a consideration of their neurophysiological basis. That is what Kainz does in this volume. For this he employs a somewhat updated Wernicke–Lichtheim scheme (although without graphically displaying Lichtheim's house, i.e., Fig. 3.8). In the chapter on speaking Kainz proposes a two-stage theory of speech production based on a flood of references to the literature. He calls these stages *diction* and *articulation*, essentially following Kussmaul (1877). *Diction* is the internal, speech preparatory process. It involves two usually overlapping and interacting operations. There is *conceptual preparation,* which has an *Urteil* as output, i.e., a proposition or predication, entirely conceptual in nature. There is, second, *linguistic formulation*, which involves processes of word finding and syntactic ordering and collocation.[91] Its output is the "inner speech plan."[92] The stage of *articulation* involves not only the motor execution of the inner speech plan, but also the kinesthetic

[89] See his preface to Kainz 1956, Vol. IV.

[90] "daß die Sprachzentren Schalt- und Koordinationsstellen seien." (Kainz 1954, Vol. III, p. 84).

[91] These operations correspond reasonably well to Levelt's (1989) *conceptual preparation* and *grammatical encoding.*

[92] "innersprachliches Konzept" (ibid. p. 122).

and auditory feedback of this execution. They provide control options, which can lead to last-minute modifications or to full interruptions of the ongoing speech. Regrettably, Kainz does not work this out. There is no discussion whatever of spontaneous self-repair in speech. Kainz uses his scheme in the obvious way to review aphasic and other disorders of speech.

The chapter on speech understanding follows a similar scheme. Kainz first proposes a stage theory and then considers some of the neurophysiology involved. Speech understanding involves four phases. There is, first, the *acoustical-perceptive* phase. This is not just "general" auditory processing, but already involves the speech-specific aspect of recognizing sounds as phonemic signs. The latter ability is precisely what is lost in sensory aphasia. The second phase is the *assimilative-reproductive* one. It is a memory-based "apperceptive" completion or "filling-in" of perceptual gaps. It involves access to auditory word memory in the sensory speech area. The third phase, denoted by *mnestic-gnostic* (which literally means "involving memory and knowledge"), establishes word recognition, including word *sense* recognition. The fourth and final phase, the *logic-intellectual* one, uses perceived morphological and syntactic features to derive the meaning of sentences and paragraphs. Their further use in the speaker's thinking "is not a theme of the psychology of language, but of thought psychology and epistemology."[93] There is some discussion of understanding difficult or ambiguous sentences, but it does not surpass the anecdotal.

The final chapter of this volume concerns conversation. In itself, this is a relative novelty in the psychology of language. Kainz does refer here (p. 499) to both Kantor and Gardiner, who had, in different ways, argued for the establishment of cooperation as a core purpose of language. He also uses the occasion to add a fourth function to the three that Bühler formulated in his Organon Model. Next to "expression," "evocation," and "representation" one should distinguish "questioning" as a separate function of language. This idea was inspired by Géza Révèsz, to whom we will return in the next chapter. Kainz is still hesitant about this extension of Bühler's model, but later, especially in his (1965) over-extended over-flattering preface to the new edition of Bühler's *Sprachtheorie*, he calls this "basic function" "an important completion of the Organon model."[94] One would have hoped that Kainz would have discussed in this final chapter the available literature on speech acts (such as Reinach's, Lipps's, or Bühler's), especially in his section on speech acts (*Sprachhandlungen*), but one would have hoped in vain.

To summarize, this core volume provides a thorough review of the literature on speaking and speech understanding, at least of the German literature. Theoretically, Kainz follows the stage-theorizing introduced by Kussmaul and Pick, but he does not go much beyond them. The theoretical distinctions are largely argued from speech disorder, in

[93] "Der hierbei ins Spiel tretende logische Prozeß bildet kein Thema der Sprachpsychologie, sondern der Denkpsychologie und Erkenntnislehre." (Kainz 1956, p. 361).

[94] "Damit ist zugleich eine wichtige Vervollständigung des Organonmodells erbracht, indem als vierte Elementar- und Fundamentalfunktion, die irreduzibel neben den auch bei Bühler genannten steht, die Frage angeführt wird." (Kainz 1965b, p. xv).

particular aphasiological evidence. Two things are noticeably missing. There is no hint of a procedural approach to the operations of production and perception. Otto Selz's (1913, 1922) highly innovative contributions are entirely disregarded.[95] The other observation is that a systematic experimental approach to the study of speaking and understanding is simply not conceived of. There is, for sure, the occasional reference to an experimental study, such as Bagley's (1900) experiment on the perception of mutilated spoken words, but the testing of stage theories never becomes an experimental challenge. It is especially remarkable that the sophisticated reaction time methodology, developed by Donders, Cattell, and others, had entirely evaporated. Kainz's treatment does rather faithfully reflect the current state of the art in the (German) literature, but there is no noticeable effort to go beyond it, neither theoretically nor empirically. Kainz was not an original thinker.

Volume IV, published in 1956, deals with writing, reading, linguistic intuitions, and errors. The chapter on writing begins with a review of writing systems, from ideographic to syllabic and alphabetic. It then turns to the issue of a "writing center" in the brain. The classical theories of Exner, Dejerine, and Wernicke are reviewed, resulting in a depicted diagram, the only one in Kainz's six volumes. It is Thiele's (1928) "writing" version of Lichtheim's house (p. 63). Kainz also reviews literature on eye–hand coordination, writer's cramp, and the automaticity and potential graphological relevance of a person's writing movements. The chapter on reading also reviews the relevant brain machinery, based on the alexia literature (Dejerine and Wernicke—see Chapter 11) and the evidence from congenital dyslexia. Kainz does not evade the classical letter and word recognition literature and its Gestaltist extensions, but again there is no urge to come up with outlines of a processing theory.

The next chapter is on *Sprachgefühl*, best translated as "linguistic intuition." Kainz had published a small monograph *Sprachgefühl* in 1944, which was probably the very first review of this topic. In his chapter, Kainz managed to collect and review the highly scattered and disjointed literature, including whatever had emerged since his 1944 monograph. There is a serious effort to understand the underlying mechanisms of linguistic intuition. Kainz argues that it involves a combination of memory and analogy. Our linguistic memory involves phonological, morphological, and syntactic schemata. When we hear or read something new, we apply these schemata by analogy. These operations are usually highly automatic, which explains the immediacy of most linguistic intuitions. Kainz suggests that the same mechanism is at work in the child's characteristic "regularization" of language (as in *eat—eated*).

The final chapter of this volume reviews the literature on speech errors, comprehension, and reading and writing errors. The same year (1956), Kainz published a somewhat overlapping monograph on this topic. Again, Kainz is a "catholic" reviewer, equally attentive to all contributors, whether Meringer, Freud, or Bawden. He is also comprehensive in providing typological tables of error types, from exchanges to anticipations,

[95] Although Kainz does refer to Selz in other places.

contaminations, etc. to deletions, dissimilations, and stammers. Missing again is any real effort to come up with an explanatory processing theory. This is especially noticeable because he characterizes Meringer's contribution as "mainly descriptive," providing an "ordering and classification of the phenomena; psychological causes and explanations are only secondary to him."[96] He simply failed to see that Meringer provided the first entirely *mechanistic activation account* of error generation, an approach that Cutler and Fay (1978)[97] appropriately call "modern." Kainz dismissed that processing model as "elementary-psychological atomism" (p. 459). Kainz also misses the other "modern" feature of Meringer and Mayer's work, the inclusion of *all* systematically observed errors. That is precisely what Freud failed to do; he just picked out whatever he liked. Kainz has no eye for such essential features.

Kainz published his last two volumes in 1965 and 1969, respectively. They both transcend the time window of this book. Still, a few remarks are appropriate; after all, these volumes complete a major and unique work, which was conceived a quarter of a century before. They also complete what once was the leading German tradition in the psychological study of language. The preface of Volume I (1941) had announced that the final volume would be on the relations between people, race, and language, with a focus on the psychology of the German language. The final volumes are indeed about the relations between *Volk*, language, world view, and ways of thinking. Race is still mentioned, but declared rather irrelevant, because even monolingual nations are hardly ever "pure" in terms of race. Volume V discusses general issues, such as what it is to be a "people" and what happens if a people "wanders" or moves into a different-language region. Here Kainz discusses changes in both the mentality and language of the English emigrants to North America and the Spanish and the Portuguese moving to South America. A preliminary consideration of "national styles," such as a French, British, or Chinese style in language use, leads to distinguishing some psychologically relevant features in which peoples/languages can vary. Among them he focuses on the more concrete or the more abstract, static versus dynamic, intellectualistic versus volitional/utilitarian. Kainz stresses the fundamental interdependencies of a people's language, thought, and labor cultures, approvingly citing, among many others, Géza Révèsz, who wrote: "Peoples with primitive languages are limited in their labor methods. They do not possess the imaginative power and inventiveness required for complicated labor, because the conscious setting of objectives and the conception of their execution is bound to their language."[98] Kainz's idol and guide throughout both volumes is Weisgerber, whose views on the

[96] "Seine Hauptleistung liegt auf dem Gebiet der Beschreibung, Ordnung und Klassification dieser Phänomene; psychologische Ursachen und Erklärungen kommen für ihn erst in zweiter Linie." (Kainz 1956, Vol. IV, p. 459).

[97] In the introduction to their (1978) re-edition of Meringer and Mayer (1895).

[98] "Völker mit primitiver Sprache sind in ihren Arbeitsmethoden beschränkt. Sie besitzen die für kompliziertere Arbeit erforderliche Einbildungskraft und Erfindungsgabe nicht, weil die bewußte Zielsetzung und die Gedankenform ihrer Ausführung an die Sprache gebunden ist." (Kainz 1965a, Vol. V, p. 224).

determining role of the mother tongue on a person's *Weltanschauung* and ways of thinking we discussed in the previous chapter. In the final chapter of Volume V Kainz discusses "applied" consequences of these well-ascertained facts. One is the potential harmfulness of early bilingualism. Here Kainz discusses Sander (1934), who, we saw, claimed that bilingual education undermines the child's "mental certainty"; that self-certainty can only be rooted in the mother tongue. This time Kainz is critical. Citing Ronjat, Leopold, and Arsenian (see Chapter 10), he argues that the evidence is controversial and certainly not in full support of Sander's position.

Volume VI then sets out to present "psychograms" of three different languages, German, French, and English. The term "psychogram" is used to denote, metaphorically of course, a language's "personality profile." Kainz discusses the notion of linguistic relativity, making reference to Weisgerber, Sapir, Whorf, and others. He carefully contrasts relativity with universality, arguing that neither can be missed in a comprehensive treatment of language. Then, by way of general introduction, the first half of the volume systematically reviews the "fundamentals" of language. Together they constitute the features on which languages can vary and hence differ. A language's "personality" can appear from its sound structure, its prosody, its lexicon, its system of word classes, its grammar (morphology and syntax), its stylistic means, such as hyperboles, euphemisms, and polite forms, and more. Next Kainz moves to the core of his enterprise, the psychological characterization of German, French, and English. Here we are flooded with pointed citations from the linguistic and philological literature, cumulating in "psychograms" of the three languages. Kainz implies that linguistic relativism is at work here. Users of a language tend to "think" the way their language is psychologically endowed. Not surprisingly, the psychograms express the usual clichés about German, French, and English. German is explicit and precise in its pronunciation, morphology and syntax. Germans are dynamic and sound symbolic. They love to build complex morphological and syntactic Gestalts, always trying to keep the whole in mind.[99] They happily take the consequence of elaborate thinking-for-speaking in the planning of their utterances. The French are "rationalistic" and somewhat rash. The English, what else could they be, are utilitarian, economical, and concrete in their language use. They speak indistinctly (which is economical). They are also individualistic, "never mind others, speak for yourself" (p. 498). After all this, one is happy to come across a citation from Thomas Mann: "Neither Germans nor French should be so foolish as to have themselves talked into whatever striking character part by popular *Völkerpsychologie* and to believe that the one should eternally play-act rationality, the other continuously dynamics and chaos."[100] It did not convince Kainz.

[99] With the *Rahmungsgesetz* as unique feature. Such seductions of language are the theme of Kainz (1972).

[100] "Weder Deutsche noch Franzosen aber sollten so töricht sein, sich von der populären Völkerpsychologie irgendeine simpel profilierte Charakterrolle aufschwätzen zu lassen und zu glauben, der eine müsse ewig Vernunft, der andere unausgesetzt Dynamik und Chaos mimen." Thomas Mann, cited by Kainz (1969, Vol. VI, p. 473).

It is almost symbolic for the disappearance of German psycholinguistics that their last all-round representative never ran an empirical research program of his own and never had PhD students in this field. His six volumes of *Psychologie der Sprache* did not leave a ripple on the field as a whole.

Retrospect

During the Nazi period the (already diminished) German leadership in psycholinguistics ground to a halt. This largely happened between 1933 and 1939. The dismissal of all Jewish university staff and of political dissidents in both Germany and Austria caused a wave of emigration. Many leaders in the field settled eventually in the United States. In Germany and Austria the scientific scene became increasingly corrupted. Remaining psychologists and linguists often adapted to the new regime, with different degrees of opportunism. In many cases the adaptation was just personal, putting up a front to keep one's job. In other cases, it affected the content of research. Topics such as race, leadership, and mother tongue enjoyed increasing popularity. In the worst cases, the science itself became corrupted, 'proving' foregone racist conclusions. During the same period the era of leadership in the study of language in the brain came to an end. Here the situation was much more dramatic. Not only was the Jewish medical leadership dismissed, first from their academic positions, then from their clinics, but the patients themselves became the target of the criminal regime and then of an increasingly corrupted medical profession. Only a fraction of them survived the 12-year ordeal. The issue of adaptation to the regime was severely complicated by one's responsibility for the well-being of patients.

Even the small world of language scientists, whether psychologists, linguists, or neurologists, showed the full range of human response. There were heroes, responsible scientists, opportunists of various degrees, collaborators, and Nazi leaders. One cannot avoid the question: "How would I have behaved under such horrendous conditions?" The only honest answer, it seems to me, is "I cannot say."

Part 4

Psycholinguistics re-established

Psycholinguistics entered a new prosperous era after World War II and especially during the 1950s. This was to some extent triggered by the same war, in two initially unrelated ways.

American and British defense research in electronic communication systems had greatly advanced both the mathematics and the human psychophysics of signal transmission. A new interdisciplinary community crystallized around the study of human communication. A major impetus here came from Shannon and Weaver's mathematical theory of communication, Alan Turing's theory of computation, and Norbert Wiener's cybernetics. George Miller took the lead in applying Shannon and Weaver's information theory to the psychology and linguistics of communication. His 1951 text set entirely new standards for the field. An optimistic "yes, we can" mood characterized the pioneering, interdisciplinary communication scientists in Boston and Cambridge, Massachusetts. Their influence quickly spread over the Anglo-Saxon world, in particular to the UK. At least equally influential, and very much in the same domain, were the 1951 and 1953 seminars in psychology and linguistics initiated by John Carroll and sponsored by the United States Social Science Research Council. The proceedings of the latter seminar, entitled *Psycholinguistics* and largely edited by Charles Osgood, became something of a "charter" of the re-established science.

The other trigger came from the world-wide need to take care of brain-injured war veterans. In various countries, on both sides of the Atlantic, research teams were set up to assist often large-scale therapeutic endeavors. Over the 1950s this began to establish an international community in aphasiology. Wilder Penfield's pioneering work in Canada on electronic brain stimulation added substantially to the returning interest in localizing language functions in the brain.

The final chapter of this book is not a review of post-war psycholinguistics. Much of that work is discussed in earlier chapters. It is rather a sketch of the unifying new efforts, whether successful or not, to create a viable science of the language user. These efforts contained the seeds of modern psycholinguistics, as it was to develop since the "cognitive revolution" of the late 1950s. In fact, one cannot really draw the line here. As George Miller put it, it all contributed to "reviving a better past."

Chapter 15

Psycholinguistics post-war, pre-Chomsky

The ravages of World War II had deeply affected the global map of psycholinguistics. Not only had this once blossoming field been effectively eradicated in Germany, but it had, like most branches of science, also suffered severe blows in all European countries that had undergone Nazi occupation. Recovery in this continental part of Europe, from Russia to France, was slow and small-scale. The one sad exception arose from the urgent need to take care of brain-injured war veterans. Many of them suffered from speech and language disorders. Veteran rehabilitation programs were set up, in some cases backed up with research facilities, very much like what happened in Europe after World War I. Luria's work, reviewed in Chapter 11, is the most outstanding example.

The point of departure in the Anglo-Saxon world was quite different. British and American war efforts had involved major theoretical and technological advances in the development of electronic communication systems. The online transmission and interpretation of radar signals had been a top priority. But the efficient coding of speech for telephone transmission had been hardly less relevant. Large-scale defense research projects involving speech and language had been running at MIT, Harvard, and Bell Laboratories. This went hand in hand with the development of cryptography. The great minds of Alan Turing (1912–1954) of Cambridge University and the British Code and Cypher School, where the Enigma code was cracked, and Claude Shannon (1916–2001) of Bell Laboratories (Fig. 15.1) had met in 1943, when Turing spent two months in Washington on a cryptographic mission. Shortly after the end of the war, Shannon's classified work on information transmission crystallized in his two-issue paper *A Mathematical theory of communication* (1948) and on his almost same-titled 1949 book together with Weaver. Quite similar notions on the theory of information were independently developed by Norbert Wiener (1894–1964) in his *Cybernetics: Or control and communication in the animal and the machine* (1948). The theory became the dominant mathematical framework for psycholinguistics during the next decade and remains an important theoretical tool today. Alan Turing's (1939) theory of computation laid the foundation for what became artificial intelligence, a term presumably coined by John McCarthy in 1956. But applications to natural language processing and machine translation had emerged earlier, with the media taking up the "Georgetown experiment" of 1954 as a publicity stunt. The IBM computing device translated 60 Russian sentences into English.[1] Linguistics was in for serious funding.

[1] See Sheridan (1955).

(a) (b)

Fig. 15.1 Claude Shannon (a) © Alfred Eisenstaedt/ Time & Life Pictures/Getty Images and Alan Turing (b) © National Portrait Gallery, with permission.

The human interface in linguistic signal transmission had also become a serious topic of defense research. The process of speech perception, signal transmission by the ear, the identification of individual speech sounds, and the recognition/interpretation of spoken words in context were all actively pursued, in particular in the MIT and the Harvard Psycho-Acoustic Laboratories. The masking of speech by noise in the transmission channel had been one of George Miller's war-time defense projects (see Miller 1947 for a review). This not only involved steady noise in different frequency bands, by tones of different pitch, and by "warbling" tones, but also the masking arising from other speech in the same channel.

This revolutionary new approach to speech communication was by nature multidisciplinary. Electrical engineers, psycho-acousticians, mathematicians, statisticians, biophysicists, phoneticians, linguists, psychologists, and neurologists all had their say in the new enterprise. This is especially apparent from a 1950 landmark event, the conference at MIT on speech communication.

The 1950 Conference on Speech Communication

This meeting was held at MIT from 31 May to 3 June 1950, under the auspices of the Acoustical Society of America. Chair of the meeting, psychologist Smith Stevens of the Harvard Psych-Acoustics laboratory ventured to open the meeting with his own definition of communication: *Communication is the discriminatory response of an organism to a stimulus*,[2] which sounds exactly like we are back to square one behaviorism. What emerges, however, is that the collective of mathematicians, engineers, psycho-acousticians, biophysicists, phoneticians, etc. could not care less about behaviorism. The first paper, by mathematician Robert Fano, provided an informal introduction to communication or

[2] The proceedings of the conference were edited by Stevens and published the same year by *JASA* (Volume 22). References are to the pages in this special issue.

information theory, as developed by Shannon and Weaver and by Wiener, the founder of cybernetics. The paper elaborates the two basic premises of communication theory, the first of which is that any information communicated is a choice from a number of alternatives. A choice from two alternatives is less informative than one from a hundred alternatives. The second premise is that communication is probabilistic. If one alternative has a high probability and does happen, it is a less informative event than when a low-probability alternative appears. In other words, the information of a signal or message transmitted is the amount of uncertainty it reduces in the receiver.

The next paper, by Norbert Wiener, discussed the generality of these notions. They apply as much to electronic transmission systems as to human interpretation of linguistic messages. Semantic interpretation involves choosing from the many alternatives that are somehow stored in human memory. In Wiener's own words: "I do not think then, that there is any fundamental opposition between the problems of our engineers in measuring communication and the problems of our philologists. Our philologists and engineers both have the problem of studying the properties of a little black box with terminals where they don't know what is inside. This is a much less important problem for the engineer than for the philologist. The engineer after all, can rip his box open. The philologist can't." (p. 697).

Joos, the linguist, argued that his discipline singles out the "discrete" part of the communication process. There is, on the one hand, the continuous speech signal received and transmitted by the ear of the listener. This is the realm of physics/phonetics. On the other hand is the listener's semantic interpretation, which has to do with conditional probabilities of that message in the speech community. That is the realm of sociology. In between is discrete linguistics, with its discrete code of phonemes, words, etc. Language is like the Morse code, a discrete, symbolic communication system.

At this point, Oliver Straus of MIT argued against such provincialism: "One proposed solution is to declare that linguists will stop at the level of discrete phonemes, and the engineer and phoneticist will go no further than the phone, leaving them to glare at one another across a wide strip of logical no-man's-land." (p. 709). It is precisely information theory which can bridge the gap. When the speaker encodes in his speech a particular discrete phoneme (the speaker's "s-phoneme"), the listener will decode that bit of speech as the same or another particular phoneme (the hearer's "h-phoneme"). The overt acoustic event will vary from occasion to occasion and from speaker to speaker. There is noise in the system. Here, context will do the work. If the speaker's *s*-phoneme is part of an English word or sentence, "the probabilities for all but one antecedent phoneme are likely rapidly to converge toward zero, leaving a unique *h*-phoneme as the only possibility." That is a general characteristic of (higher-order) Markov processes. Any linguistic string of phonemes is highly redundant. And what do we do if, occasionally, the convergence does not take place? We will say: "We did not understand" (p. 711).

There were several papers on the statistical, physical properties of speech signals, and on signal transmission in the ear, among them three European contributions.[3]

[3] From Meyer-Eppler's phonetics laboratory in Bonn and from an ENT clinic in Basel.

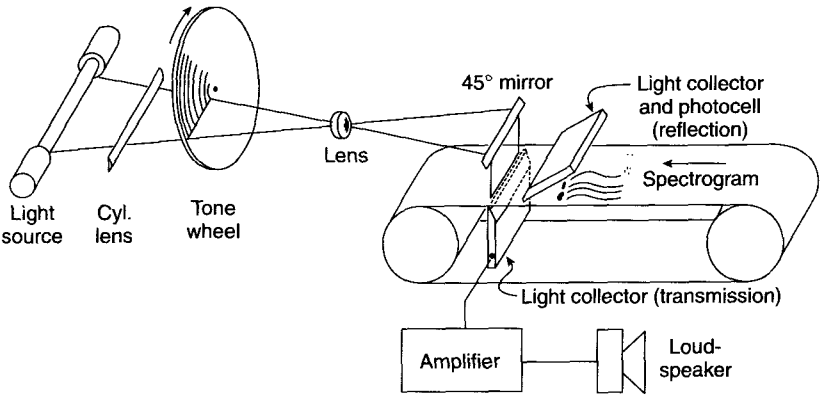


Fig. 15.2 Pattern playback equipment. The experimental speech spectrogram is painted in white on a transparent acetate band. When light is projected on the running band, it is reflected by the white, reflecting parts of the spectrogram, detected by a "light collector" and photocell, whose output is fed to an amplifier and loudspeaker. Reproduced from Cooper, F.S., Liberman, A.M. & Borst, J., The interconversion of audible and visible patterns as a basis for research in the perception of speech. *Proceedings of the National Academy of Sciences, 37,* 318–325. © 1951, the authors.

Especially noticeable were the early efforts to invent a way of "playing back" spectrograms. Frank Cooper of Haskins Laboratories[4] presented their patented solution to the problem. The technique was soon developed to great precision and usefulness. You could paint your own experimental "formants" on a long acetate band, run it through the playback equipment and evaluate the perceptual, auditory result (Fig. 15.2). It became the standard tool for speech generation, till the computer began taking over during the 1970s. Much of the pioneering work by Cooper, Potter, Liberman, Delattre, Borst, and others on the role of formants and formant transitions in the perception of speech sounds exploited this elegant tool in one version or another. We have already mentioned, in Chapter 12, Delattre *et al.*'s (1952) work on vowels. Liberman (1957) reviews the early work on the role of formant transitions in the perception of consonants. What Kempelen began in 1791 came to a partial completion 160 years later. Partial, because the playback device was not yet a working model of the vocal tract, as Kempelen had envisaged.

One further paper at the conference deserves special mention in the present context, George Miller's on "language engineering," a self-invented term for a new discipline in the offing. What would it be about? It would "gather together the various strands of interest from philosophy, philology, logic, linguistics, acoustics, electrical engineering, psychology, education, sociology, mathematics, anthropology, literature." (p. 720). It would

[4] The Haskins laboratories, in existence since 1935 and then still situated in New York City, turned to psycho-acoustic work during the war. The National Academy of Sciences made it the research center for its Committee on Sensory Devices. One major project concerned auditory communication in the blind.

produce such things as a phonetic typewriter (which will write down whatever you say to it), an auxiliary international language, a translation machine, and so on. Miller then illustrates the endeavor by discussing what would be needed to devise an "international language for the air," a standardized language for air traffic, to be used by pilots and air traffic control. The construction of such a language would require answers to the following six questions: (1) What is being said? It will be necessary to record actual air traffic communication and analyze it physically, phonetically, linguistically, statistically, and psychologically in term of its contents. (2) What should be said? One should decide on a vocabulary and a grammar that are both efficient, but also statistically redundant, in order to overcome noise in the communication channels. (3) Can it be spoken? An international language should not include speech sounds such as *th*; it should not rely heavily on word order (as is the case for English), etc. (4) Can it be heard? The intelligibility and confusability of speech sounds and words should be thoroughly tested out. (5) Does it work? Real-life tests should be run on the use of language, errors should be analyzed, and corresponding revisions should be made. (6) Can it be learned? This should be explored by the psychologists, linguists, and teachers involved.

Miller's prophetic proposal is suffused with the new, multidisciplinary attitude in communications research. His paper then turns to what the psychologist can specifically contribute. Here Miller presents his own work on the perception/reception of speech as illustration. It is, in particular, the work that we reviewed in Chapter 12. How is speech perception disrupted by different degrees of noise? And how does it fare for different approximations of English? Miller signals that listeners are just as good on approximations in the order of four to five as on real text. Also, their memory abilities are no different for these alternatives. This should be a relief for the engineers, who are afraid of working with high-order Markov chains, which require large storage facilities.

The British scene

Similar developments, although on a smaller scale, took place in the UK. Among the young scientists working on radar during the war were Donald MacKay and Colin Cherry. MacKay (1922–1987) became a leader in artificial intelligence and cybernetics, first betting on analogue computers. His interdisciplinary team at Keele worked, among other things, on sensory aids for blind and deaf people. Colin Cherry (1914–1979), who spent most of his academic career at Imperial College, London, was by training an electronic engineer, but soon became a cognitive scientist (before the term existed). His wide reading in psychology, philosophy, linguistics, anthropology, and history of science made him the embodiment of the new post-war interdisciplinarity in the study of mind.

Cherry's main contributions to psycholinguistics were threefold. In a cross-Atlantic cooperation project with Morris Halle and Roman Jakobson (at MIT and Harvard, respectively), Cherry applied Shannon's information theory to phonological feature analysis. Jakobson, Fant, and Halle had, in *Preliminaries to speech analysis* (1952), introduced their influential distinctive-feature analysis of phonemes. Cherry then involved them in a statistical, information-theoretic analysis, which was based on a corpus of

Russian text available at Jakobson's Slavic Department at Harvard. Their joint publication (Cherry, Halle, and Jakobson 1953) was based on an analysis of all vowel–consonant-vowel sequences in the text, which involved 42 different phonemes. How informative is a single phoneme? You will minimally need 5.38 binary choices (bits) to distinguish these 42 phonemes. Jakobson and Halle, however, had proposed 11 distinctive, binary features for the Russian phonemes, such as "vocalic," "nasal," and "voiced." That is about twice as much as would be strictly needed. It shows that Russian phonology is quite redundant. An additional issue is that phonemes are often simply not specified for a feature. The feature of nasality, for instance, is not applicable to consonants like *k* or *t*. Cherry *et al.* then proposed a procedure of *ordered* checking of features. First, phonemes such as *k* or *t* are distinguished on the basis of a *subset* of features (i.e., not including nasality or vocalicness). Then further features (such as nasality) are added to distinguish the other phonemes. This reduces the information load from 11 to 6.5 bits, a major gain. Next, Cherry *et al.* considered transitional probabilities among phonemes. Phoneme *k*, for instance, is quite improbable after *t*. It is highly likely that *t* will be followed by some vowel or liquid. There are preferred pairs and triples of phonemes. A phoneme in such digrams or trigrams is much less informative than when it appears in isolation. The speech signal is quite redundant and therefore error-resistant. In a last step, the authors discuss such transitional probabilities in terms of sub-phonemic distinctive features. Distinctive features tend to stretch over larger units, such as digrams, trigrams, or whole syllables. There are also preferred alternations among features, such as among vocalic and consonantal. The paper's suggestion is that the sub-phonemic level is most appropriate for the information analysis of the sound patterns of a language.

Cherry was also the first to study the "cocktail party effect" (his term) experimentally. His first paper, in 1953, was based on experiments he had performed during a six-month visit to MIT's electronics laboratory. We saw that the perceptual distortion of speech by noise received substantial attention during and shortly after the war. Miller had, in particular, considered the interference caused by other speech in the channel. That is what we have to deal with at cocktail parties. There are various ways of keeping your attention focused on your target speaker, such as lip-reading, ignoring voices from other directions or from a different sex, etc. Cherry was most interested in transitional probabilities. The redundancy of a speaker's speech should facilitate keeping track. Cherry then mixed two different messages spoken by the same speaker and instructed his subjects to repeat one of them. The subject was allowed to listen to the tape as often as needed. This was a hard task, but subjects usually succeeded. This changed drastically when the spoken texts were concatenations of clichés, such as *I am happy to be here today to talk to the man in the street*. Here the two messages got mixed up; the listener would reproduce clichés from both messages. This, Cherry argued, resulted from the very high (word) transitional probabilities within clichés and the very low ones between them. But then Cherry set up the clever condition of playing the two messages over different ears (now called "dichotic listening"). The instruction was to repeat the message in one ear, as it was unfolding. This introduced the famous "shadowing task" into psycholinguistics. The main finding was

that listeners do not have the slightest problem with this task. And quite surprisingly, listeners could not reproduce anything from what they had received in the other ear. They did, however, notice when the voice in the unattended ear switched from male to female or the other way round. However, people hardly noticed a switch to German and back, or a short period of reversed speech. In a follow-up study, Cherry and Taylor (1954) had a single message switch back and forth between the two ears. If the switching was very slow, such as one second per ear, the message was fully received. The same was true for very high-frequency switches, such as a hundred per second; that sounded like continuous, binaural speech. But three to five switches per second was detrimental. This, Cherry, concluded, is the critical latency for making an attentional switch or the time needed for "picking up" the identity of the speaker.

Three years later, Cherry published his text *On human communication* (1957), dedicated to his dog Pym. It is not a psychologist's text, nor a philosopher's text, nor an engineer's text. It is, rather, a heroic effort to sketch a unified, integrated approach to human communication. It adopts the mood of talking across disciplines, which Cherry had encountered at MIT in 1952.

Cherry acknowledged in his 1953 paper that it had been Broadbent's idea to mix two messages and have the subject attend to one of them. Donald Broadbent (1926–1993), who was to become the greatest post-war cognitive psychologist in Britain, had in 1952 published three seminal papers on selective attention. The work was performed at the MRC Applied Psychology Unit in Cambridge, which Frederic Bartlett had established during the war. In a first paper Broadbent (1952a) had subjects answer short questions with short, standardized answers. The pacing of the questions could be increased in such a way that the subject was still answering the previous question, i.e., talking, while listening to the new question. This condition was highly interfering. It is apparently hard to attend to both listening and talking at the same time. In a second paper (1952b) the subjects listened to two questions simultaneously, but had to answer only one of them. Which one was indicated in various ways, for instance by a "call signal," such as "S-one" directly preceding the question; subjects knew that they had to answer to all and only the "S-one" questions. This was a pretty hard task. But it became a lot harder when the call signal *followed* the relevant question. Acoustically, the two stimulus situations were identical: two fully overlapping questions. But the call signal, when presented before the messages, made it possible to selectively attend to the one and ignore the other. This, clearly, ruled out the simple explanation that the reduced performance was (entirely) due to the mutual masking of the two speech events, i.e., to a sensory effect. That had been the (mostly tacit) assumption in Miller's (1947) paper on speech recognition in noisy conditions. The listener could, somewhere "later in the system" set a switch which would pass the one message and inhibit or block the other one. This would become Broadbent's famous "filter model" of selective attention (Broadbent 1957a). The third paper (1952c) introduced some further conditions. The simplest one was that the listener would first hear the question of speaker A and then the question of speaker B—he then had to answer both. This was very easy. But it got harder when the words of the two questions alternated

one by one. It remained difficult if the instruction was to answer only speaker A's question and to ignore the interspersed words from the other speaker. In 1957 Broadbent published his classic *Perception and communication* in which these early experiments are discussed in the much larger framework of his theory of human attention and performance. In the book he criticizes S-R psychology, because it neglects all factors operating between stimulus and response. The objectivity of the notions "stimulus" and "response" is illusory. A stimulus with two alternatives is psychologically totally different from the same stimulus with a hundred alternatives, and the same holds at the response side. "The main reasons for departing from stimulus-response language are the need for considering the coding of input into output, the need to consider the whole ensemble of possible stimuli rather than simply the presence or absence of each one, and the need to distinguish between the arrival of the stimulus at the sense organ and use of the information it conveys. These points must be considered in view of recent experimental results, and it is difficult to do so in S-R terms." (p. 59). Nobody remembered that this important scientific fact had been discovered by Donders, in 1868.

Phonetician Dennis Fry (1907–1983) spent the war years in charge of the acoustics laboratory of the Royal Air Force Medical Establishment. It was an intermission in his career at the Phonetics Department and Laboratory of University College London, which began in 1934 and continued till his retirement in 1975. Fry's early work was on the phonetics of stress. Not only amplitude, but also pitch and duration contribute to perceived word stress (Fry 1958). A major theme in Fry's work was the remarkable robustness of speech communication under everyday distortion. Much more than the speech signal is at stake here. Fry promoted an interdisciplinary perspective, including linguistics and psychology, in the analysis of speech communication. Fry had a keen interest in the speaking process. He was the first to develop a theory of incremental production. Different processing levels in producing speech, from semantic planning to morphological and phonological encoding to motor execution, are layered like tiles on a roof, the higher level processes temporally preceding the lower level ones (Fry 1969).

One psychologist Fry appointed in his Phonetics Laboratory (in 1955) was Frieda Goldman-Eisler (1907–1982), who emigrated from Vienna to London in 1934 as mentioned in Chapter 14. There she completed her interrupted study of psychology and began her research career in the clinical setting of Maudsley Hospital. Interviews, both clinical and "normal," became her topic there. How frequently does a speaker breathe or inhale during speech? What is his or her speaking rate in syllables per minute? How many syllables are produced in a single exhalation? Goldman-Eisler carefully measured such features of interview speech and related them to characteristics of the speaker, to the speaking style of the interviewer, and also to the natural course of an interview. The number of syllables per exhalation, for instance, normally goes down during an interview. It signals growing relaxation as the interview proceeds (Goldman-Eisler 1952, 1954a,b, 1956).

After her move to the phonetics department, where she would further her career, Goldman-Eisler began exploring the determinants of hesitations in otherwise fluent speech. Her *Nature* paper of 1957 demonstrated that hesitations tend to occur in positions

of low transitional probability.[5] She took from recorded conversations a set of sentences containing hesitation pauses exceeding a quarter second. She then had subjects, for each sentence, "predict" the second word, given the first word, then the third word, given the first two words and so on till the end of the sentence. In this way she could estimate the transitional probability to each next word in the sentence. It turned out that the average word predictability of about 50% dropped to as little as 5% for word transitions around a pause. She also determined the "backwards" predictability, going from the last to the first word of the sentence. Also here transitional probabilities were relatively low when a pause separated both words. Another observation was that words following a pause were much longer than words preceding it. This correlated with the word types involved. For instance, hesitations tend to occur between a function word and a content word (*I saw a... hippopotamus*). Goldman-Eisler interpreted these results in terms of a steady alternation between automatic processing in speech and voluntary, organized planning, in much the same way as proposed by Hughlings-Jackson.

Goldman-Eisler became a leader in the study of the speaking process. Her influential monograph *Psycholinguistics* (1968) reviews her elegant combination of statistical and experimental studies of the speaker in action.

The analysis of interviews was also the starting point for Richard Oldfield (1909–1972). Oldfield was a student of Frederick Bartlett in Cambridge when, in 1937, he became a research assistant on a project of Charles Myers, an industrial psychologist in London. The resulting booklet (Oldfield 1941) is a rather discursive account of interview tactics, the display of attitudes, and practical uses of interview techniques. During the same period, however, Oldfield and Zangwill also published the paper discussed in Chapter 12 on the learning of normal, ungrammatical, and anomalous texts. This was Oldfield's real start in psycholinguistics. He later became director of the Medical Research Council (MRC) Speech and Communication Research Unit in Edinburgh.

Some further developments in the study of the brain and language

As mentioned before, much of the post-war research on language in the brain resulted from the care of brain-injured war veterans, but research on epilepsy patients was another major development.

Soviet Union

Alexander Luria probably conducted the most extensive study of language loss in war victims. The project was extensively reviewed in Chapter 12.

Germany

A similar but smaller-scale study was performed in Germany by Klaus Conrad (1905–1961). Conrad, an Austrian, received his MD in Vienna and spent a semester as

[5] The details of this study appeared in Goldman-Eisler (1958).

a neurosurgical volunteer in London before moving first to Magdeburg and then to Paris in 1933 to become acquainted with French neurology. Next, in 1934, he accepted a position in Munich in Kraepelin's former institute, which was then directed by Rüdin, whom we met in the previous chapter as the Nazi chairman of the German Neurologist Association. Conrad left the politicized place in 1938 and got a post in Ernst Kretschmer's hospital in Marburg. Two years later he was drafted as a military physician in Russia, but then received orders to create a hospital for the brain injured in Marburg. There he took care of over 800 war victims in the two years from 1943 to 1945. Among them were 216 cases of aphasia, about half of them diagnosed as motor aphasics. The patients were carefully tested by Conrad and his team. Almost all of them had well-localized brain damage caused by trephine apertures during their brain operations. That made it possible to create statistical brain maps for people with motor aphasia and sensory aphasia, and left-hemisphere non-aphasic patients (see Conrad 1954 for an overview of his findings). The loci for the motor aphasics were widely spread along the Sylvian fissure. There was no particular concentration in the vicinity of Broca's area. The distribution for the non-aphasics was complementary, "evading" the Sylvian fissure. The localizations for sensory aphasics concentrated around the base of the first temporal gyrus, following the classical picture. These findings made Conrad align with the "holists," in particular with Jackson and Head. He proposed his own version of a Gestalt theory of aphasia, not too different from Goldstein's. Aphasia is caused by trouble in Gestalt formation. Gestalts are created on a micro-time scale, a micro-genetic process.[6] For the speaker this involves a process of internal formulation, followed by one of motor planning. The outcome is a speech Gestalt, a spoken utterance. In aphasics this "verbal Gestalting" is arrested in some transitional stage, with a defective Gestalt as outcome. The implications of this rather global account for a theory of localization remain unspecified. It is, moreover, not obvious that the damage, caused by trephine insertions during the operation, were always the real or sole cause of the observed aphasias.

With his connections in London and Paris and his engaging personality, Conrad played a constructive role in the post-war re-establishment of the European neurological scene.[7]

United Kingdom

One of the British veteran clinics for brain injury was the Oxford Military Hospital for Brain Injuries. It had been temporarily established in St Hugh's College for the duration of the war years. During that period it handled no fewer than 13,000 patients, mostly soldiers from the allied forces. Oxford neurosurgeon Hugh Cairns pioneered the quick treatment of head wounds in the battlefield and the subsequent handling in Hugh's College. Right after the war, neurologist Ritchie Russell, together with Cairns, developed the long-term research program of the hospital. Russell himself, for instance, in 1946, had

[6] Here Conrad follows Sander's theory of "Aktualgenese."

[7] Ploog (2002) provides a detailed account of Conrad's scientific career.

published on traumatic amnesia.[8] Schiller (1947) tested 46 missile-wound aphasic patients in Russell's clinic. He related the topography of their wounds to their symptom complexes. He concluded, on the one hand, that there are no "pure" cases among his patients; all had multiple symptoms, with difficulty in word finding as their most outstanding feature. On the other hand, he rejected Head's holistic views and global classifications. Missile wounds in frontal, posterior temporal, and parietal lesions correlated with characteristic symptom complexes, by and large the "classical" ones. Russell's 1959 book *Brain, memory, learning: a neurologist's view* provides an overview of his research. In 1960 Russell appointed neuropsychologist Freda Newcombe (1925–2001) in his clinic, who quickly moved its neuropsychology, including aphasia research, to the forefront of the field. The first decade of her work is reflected in her book *Missile wounds of the brain: a study of psychological deficits* (1969).

Oliver Zangwill (1913–1987) spent the war years as a psychologist at the Brain Injuries Unit in Edinburgh. He then moved to the Oxford Experimental Psychology Institute and in 1955 to Cambridge. His neuropsychological studies were usually performed at the National Hospital in London or at the Radcliffe Infirmary in Oxford. In the present context, it is his work on cerebral dominance and language which is relevant.[9] His 1960 monograph on the topic demonstrates that left-handers, and those with a tendency to left-handedness or with left-handers in the family, have less outspoken and more varying hemispheric dominance than right-handers. This may be advantageous for recovery from traumatic aphasia, but also carries the risk of handicaps, such as trouble in learning to read.

United States

Right after the war, three National Veterans Centers were established for the study of aphasia, at Long Beach, California, Minneapolis, Minnesota, and Framingham, Massachusetts. In Chapter 11, we mentioned Margaret Schuell's aphasia test. It was released in 1955 by the Minnesota center. In 1952 the Framingham Center was moved to the new VA Hospital in Boston. One of their neurologists was Fred Quadfasel, who had been an assistant to both Bonhoeffer and Goldstein in Germany. He possessed both the knowledge and the literature on the classical tradition in aphasiology and became a main informant of Norman Geschwindt, who joined the Center in 1958. The first psychologist appointed at the Center (in 1951) was Harald Goodglass (1920–2002), who, like Zangwill in the UK, started his research on the relation between language laterality and handedness.[10] He had been an Air Force officer during the war and subsequently took up the study of clinical psychology, receiving his PhD in Cincinnati in 1951. Goodglass and his growing team, among them David Howes and Norman Geschwind, were to lay the grounds for modern American aphasiology. The Boston Center became a major

[8] Russell and Nathan (1946).

[9] For his early work, see Humphrey and Zangwill (1952) and Zangwill (1954).

[10] Goodglass and Quadfasel (1954).

promotor of generative psycholinguistics in neuropsychology, with Goodglass and Hunt's (1958) paper as a firstling.

France and Belgium

In France, aphasia research was continued by Théophile Alajouanine and his coworkers, as discussed in Chapter 11. In 1948 Alajouanine published an interesting paper on aphasic artists. Maurice Ravel was one of his three case studies. Art is "overcome difficulty," Alajouanine writes, difficulty which is especially localized in creative, expressive means based on language. In 1955 Alajouanine edited a volume on the functions of the temporal lobe. It contains three of his own papers on aphasia. One reviews his own work on phonetic disintegration, relating it to the speech arrest phenomena during brain stimulation, described by Penfield (see the section on Canada). A second paper raises the issue of whether sensory aphasia is, at least in part, caused by some degree of hearing loss. No convincing evidence was found. The third paper reports on electro-encephalographic measurements in aphasics. There is evidence for disordered alpha and theta-rhythm, but no correlations could be established with different types of aphasia. At the age of 78 Alajouanine published his introductory textbook *L'Aphasie et le langage pathologique.*

André Ombredane (1898–1958) got his MD in Paris in 1929. He then gradually moved into psychiatry, aphasiology (we have already considered his 1933 chapter on language in the Dumas handbook of psychology), and psychology: first child psychology, then applied psychology and ergonomics. In 1947 he received his PhD with a psychological thesis on aphasia. It was published as a book in 1951. Ombredane moved to Brussels in 1948, where he occupied a chair in psychology until his death in 1958. His book on aphasia is largely a critical review of the history of aphasiology, from Gall to Broca, Wernicke, Jackson, Pick, Marie, Head, and Goldstein. It also provides Ombredane's own synthetic view on aphasic psychology.[11] Here Ombredane relies heavily on Hughling Jackson's views, in particular his distinction between more automatic and more volitional uses of language. On the automatic side are affective usages, such as emotional voice modulation and interjections, and the playful uses such as the babbling of infants, rhythmic repetitions and so on. On the automatic side are often the "practical" uses of language, such as calling "push" or "stop." Such automatic modes of language use are basically "agrammatic." In that sense, agrammatic language is a normal language modality. On the volitional side are the representational and dialectical uses of language, Jackson's propositional language. It is this propositional side of language which is affected in aphasia. Aphasia, according to Ombredane, "is *a syndrome of disintegration in the elaboration of verbal propositions.*"[12] It is based on a deficit of constructive thought. Here, Ombredane places himself in the tradition of Pierre Marie. Aphasic intelligence is "fundamentally mutilated" (p. 369). It lacks abstraction, propositional integration, invention, somewhat like the concrete thought of

[11] The neural side of aphasia is not a major theme in Ombredane's book.

[12] "l'aphasie est un *syndrome de désintégration de l'élaboration des propositions verbales*" (p. 292).

the child. It is at the mercy of affective features of the situation. Ombredane fits the holistic tradition, approvingly citing Head, Van Woerkom, Gelb, and Goldstein.

Henry Hécaen (1912–1983) was a student of Jean L'Hermite, clinical director of the Salpetière in Paris. His work in neurology and psychiatry ranged widely, but it took a long time before his interests moved to language in the brain. There is a comprehensive chapter on aphasia in Ajuriagguerra and Hécaen's (1949) textbook on the cerebral cortex, which was somewhat updated in the second edition (1960). In 1952 Hécaen spent several months with Wilder Penfield in Montréal, with whom he published a paper on the excision of the right-hemisphere homologue of Wernicke's area; the excision caused no aphasic symptoms, but a loss of body scheme.[13] In 1965 Hécaen and Angelergues published a textbook on aphasia, after which Hécaen still played a role in relating the study of aphasia to new developments in linguistics.

Italy

Ennio De Renzi (born 1924) pioneered neuropsychology in Italy. After his MD and neurology/psychology internship (Pavia 1950, 1953), he first moved to Modena and then in 1958 to Milan, where he spent the next decade. There he attracted young psychologists, such as Luigi Vignolo, who graduated in 1959 with a thesis on aphasia. De Renzi's team changed perspective from case studies to group studies, which was strongly promoted by their guest scientist Arthur Benton of the University of Iowa. Together with Vignolo, De Renzi published the token test (1962), now probably the most universal testing tool in aphasiology. It was inspired by George Miller's early publications on the redundancy of language. The mark of the token test is non-redundancy. In order to follow the instruction "Take the white large circle and the small green triangle," each and every content word has to be understood by the patient. In 1969 De Renzi moved to Trieste and from there, in 1973, to Modena, where he became emeritus in 2002. It was from De Renzi's scientific patronage that Italian cognitive neuroscientists such as Carlo Umiltà and Anna Basso emerged.

Canada: Wilder Penfield and electrical brain stimulation

Probably the most exciting development in the study of brain and language during the period under consideration was the work by Penfield and colleagues at the Montreal Neurological Institute at McGill University, which Penfield founded in 1934. Wilder Penfield (1891–1976) graduated from Princeton University in 1913, spent three years with Sherrington in Oxford, and then got his MD at Johns Hopkins in 1918. After his internship with Cushing in Boston, he accepted a position at the New York Presbyterian Hospital, where he began his work with epileptic patients. This he interrupted in 1924 to spend five months with Ramon y Cajal in Madrid, to become acquainted with new staining methods for non-neuronal brain cells. In 1928 Penfield moved to McGill University in Montreal. He directed the Montreal Neurological Institute from its foundation in 1934

[13] Hécaen *et al.* (1956).

till his retirement in 1960. Also in 1928, just after his move to Montreal, Penfield spent six months with Otfrid Foerster in Breslau, who had been a student of Carl Wernicke. It was Foerster who pioneered the electrical stimulation of the brain in the surgery of epileptic patients, a technique which Penfield would develop to a high level of sophistication. Their joint (Foerster and Penfield 1930a,b) papers describe how the electrical exploration of the patient's cortex under local anesthesia is used to detect epileptic loci. Convulsions or auras can be induced if a suspected area is stimulated. Such mappings of the brain were called *electroencephalograms.*

Penfield and Roberts' *Speech and brain mechanisms* (1959) provides a lucid account of their work on language in the brain. The procedure Penfield's team developed was to cut a segment of the skull under local anesthesia and turn it back to expose the brain surface underneath it. Next electrical stimulation is used to map the functional areas of the brain. This localizes both the seizure-prone regions and functions in the neighboring critical areas. Stimulation takes place with a unipolar platinum electrode with a glass handle. When excision is indicated in potential speech regions, the awake patient participates in a range of tests. Among them are picture naming (a card with a picture on it is shown to the patient and the patient names it or in a writing version writes down the name), counting and reading aloud. The critical phenomenon is speech arrest. If a speech relevant region is stimulated, the patient is suddenly unable to speak or write, or to produce the required verbal response, a state of induced aphasia. For instance, the patient is shown a picture of a foot to be named and is stimulated somewhere in the left inferior frontal region. The patient then says: "Oh I know what it is. That is what you put in your shoes." After withdrawal of the electrode he says "foot." Stimulated spots receive a tiny numbered label, which refers to the record written down during stimulation of that spot. Another phenomenon is vocalization, a vowel cry that is sustained or interrupted. This only happens when spots in the left or right Rolandic region are stimulated, both pre- and postcentral, where representations of the articulatory musculature are localized.

The book reports on cortical mapping data obtained in 273 operations of the dominant hemisphere and an equal number from operations in the other hemisphere. Figure 15.3 shows Penfield and Roberts' summary diagram, all points on the left hemisphere where electrical stimulation produced interference with speech.

A large number of further diagrams display these interference results in more detail. There are maps for hesitation and slurring of speech, number confusions in counting, failures of naming with preserved speech (as in the "foot" example), incorrect naming, and dysphasic speech. Not shown in Figure 15.3 as reproduced here is a region in the mid-sagittal fissure, just anterior to the Rolandic foot area, whose electrical stimulation also causes interference with speech. Detailed analyses of these findings, as well as the temporary or lasting effects of excision on speech, bring the authors to the conclusion that there are three cortical regions involved with the ideational aspects of speech: a large area in the posterior temporal and posterior-inferior parietal regions, a small area in the posterior part of the third frontal convolution, anterior to the motor voice control area, and part of the supplementary motor area within the mid-sagittal fissure. They call these, for brevity, Wernicke's area, Broca's area, and the supplementary speech area.

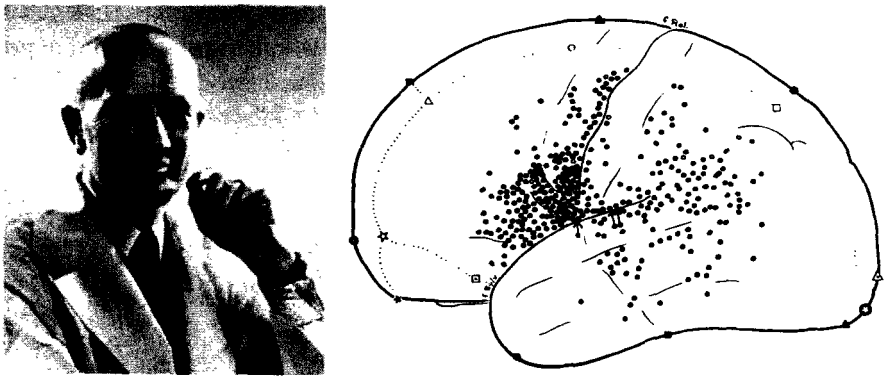


Fig. 15.3 Wilder Penfield and the summary diagram of points of electrical stimulation producing interference with speech. Penfield and Roberts, *Speech and Brain Mechanisms.* © 1959 Princeton University Press, 1987 renewed PUP. Reprinted by permission of Princeton University Press.

How dispensable are these regions? Ablations in the supplementary speech area cause aphasias that disappear within a few weeks. Broca's area is probably more essential, according to the authors. Although in some cases ablations in that area did not cause a permanent aphasia, the authors' advice is to spare it as much as possible in all cases. Any large destruction within Wernicke's area, however, is certain to cause severe aphasia, in particular if the underlying projection area in the thalamus is also involved. A further conclusion is that the ideational mechanisms of speech function in one hemisphere only. Voice control, which, among mammals, is only cortical in humans, involves the Rolandic areas in both hemispheres. Ablation in either hemisphere alone does not lead to the loss of speech.

Speech and brain mechanisms is written in a clear and matter-of-fact style, which is reminiscent of Wernicke's 1874 monograph. The first four chapters provide well-written introductory accounts of the neural and functional architectures of the brain and the history of aphasiology. In the concluding chapter the authors return to the early history of localization in the brain. They mention Gall's challenging of the dominant view of equipotentialism, but then, most regrettably, they accuse Gall of creating "an absurd pseudoscience called *phrenology*" and that the medical profession looked at it "with even greater suspicion when Gall made it both fashionable and lucrative!" (p. 192). The dedicated reader of this book knows better. In an epilogue Penfield discusses educational matters of language learning, relating them to issues of brain maturation. He strongly pleads for language teaching by the "mother's method" or "direct method" and in quite early stages of schooling: "a child's brain has a special capacity for learning language – a capacity that decreases with the passage of years." (p. 240).

Wilder Penfield had Donald Hebb as a student, and both of them were teachers of Brenda Milner (born 1918),[14] who would dominate Canadian and world-wide cognitive

[14] Hebb (1949) became a neuropsychological classic. Milner was born and grew up in England. She studied experimental psychology at Cambridge. During the war, she was involved in research on radar displays. Shortly after the war, the Milners went to Montreal, where Brenda entered the PhD program at McGill.

neuroscience for decades to come. It was Milner who initiated the study of H.M., whose medial temporal lobes had been bilaterally removed in an epilepsy operation (not performed at McGill) with deep anterograde amnesia as a result. Even after decades of working with him, he would not remember having met her even the next day. In the area of language and speech Milner's focus was on hemispheric lateralization.

Géza Révèsz and the Amsterdam symposium on thinking and speaking

The Hungarian psychologist Géza Révèsz (1878–1955) studied in Göttingen with G.E. Müller, Husserl, and Brentano; David Katz and Gustav Kafka were his fellow students and friends there. After the 1920 political revolution in Hungary, Révèsz emigrated to the Netherlands, where he became a full professor at the University of Amsterdam in 1933. There he established a laboratory for experimental psychology, which became one of the largest in Europe. Révèsz became emeritus in 1950, but continued his wide-ranging scientific activities. By the end of the 1930s, Révèsz became interested in the origins of language, which he first worked out in papers presented to the Royal Netherlands Academy. His 1950 book *Origine et préhistoire du langage* presents the final version of his theory. After reviewing the major theories on the origin of language, and rejecting all of them as inadequate, Révèsz presents his own "contact theory." The essence of language is not to be sought in its means, but in its purpose, the functions it should serve. They concern the establishment of mutual understanding. Three functions are basic to human communication and cooperation. First, there is the imperative function, to move someone to act. This function is to some extent shared with other social animals and often is quite developed in domestic animals. It also appears quite early in human ontogeny. Second, there is the indicative function, to inform someone about something. These two functions are still "unidirectional" in character. The source of human language, however, is the intention to establish/maintain mutual understanding; not only to be understood, but also to understand. Here, the third, interrogative, function comes into play. It is basic to the dialogical function of language and the last one to develop in the child, around the age of 2;0. There is a modern touch to Révèsz's "mental contact" theory, although it had been formulated more succinctly by Degérando over a century before. Révèsz continues to consider the means that our species uses to establish and support such mental contact. Here many of the means which figured in the traditional theories of language origin, such as sound imitation, exclamation, and gesturing, come into play. They all contribute to the creation and cultural transmission of a language, but none of them explains its *origin*. Révèsz is especially outspoken on gestural origin theories. It is rather absurd to suppose that primordial man, equipped with exquisite vocal and auditory means, would go around for generations like somone born deaf who cannot speak, limited to gestural communication.

Révèsz organized, in 1954, a symposium on thinking and speaking. Among the participants were Kainz, Piaget, and Goldstein.[15] The contributions range from closely

[15] The complete list is Buyssens (philosopher from Brussels), Cohen (psychologist from Manchester), Elisaberg (psychologist from New York), Goldstein, Gruhle (psychiatrist from Bonn), Jørgenson

identifying language and thought to defining them as essentially different. Révèsz is in the former camp. His paper elaborates two claims: "speech is not possible without thought" and "thought is not possible without speech." Both are obviously false, but Révèsz builds his defense on restricting the notion of language. The speech of a person with mental illness or learning disabilities is not "real" speech. The "wordless" thought in abstract and creative thinking is still linguistic in that it uses grammatical thought schemata (here Révèsz makes reference to Selz's theory of thinking). Piaget is in the latter camp. His paper forcefully marginalizes the role of language in the child's three stages of development, the sensori-motor stage, the concrete logical-operational stage, and even the stage of propositional logic. Language is important and influential in attaining the second and especially the third level of logical operations, but is not the essence. Mastering the language is not a sufficient condition for attaining these levels of thought. Mathematician Van der Waerden is of the same opinion, arguing that mathematical thought can be free of language and even free of formulas. Kainz takes a middle position and Kurt Goldstein warmly embeds both language and thought in the whole organism's fulfillment of self-realization.

There is no reference anywhere in the carefully edited proceedings to Watson's "thought is implicit language activity" and the 1920 British symposium on that theme (discussed in Chapter 8). Behaviorism is as non-existent in this symposium as it always was on the west European continent.

Old and new in developmental psycholinguistics

John Carroll's (1960) review of language acquisition covers the period from the end of the 1930s, mostly the post-war period. Most noticeable is the continuity of method and theory we observed in Chapter 10. There are, on the one hand, new single case studies, such as Sprague De Camp (1946), Cross (1950), and Cohen (1952) on the acquisition of French, and Chao (1951) on the acquisition of Mandarin Chinese.[16] There are, on the other hand, the normative group studies on the acquisition of phonology, vocabulary, and syntax in the tradition of the Institutes for Child Welfare, such as Templin and Irwin, discussed in Chapter 10. But then, Carroll makes a strong plea for an experimental approach. "Theories of language learning cannot ultimately be tested unless an experimental approach is adopted" (p. 335). However, "oddly enough" this is hardly done. Carroll mentions some promising recent work, to which we will turn in a moment. Theory in Carroll's review is still behaviorist theory. "The most ambitious effort to work out the details of such a theory as applied to language learning is Skinner's. . . . The process of language learning can be described in general terms in this way: throughout his language development, the child learns what verbal or gestural responses will get what he wants or fend off what he dislikes, and what responses of others are the cues for

(philosopher from Copenhagen), Kainz, Piaget, Révèsz, and Van den Waerden (mathematician from Amsterdam, working in Zürich).

[16] Chao's (1951) study is, in fact, a snapshot of a Chinese girl's speech at the age of 2:4. It contains a detailed account of her phonology, of her phrasal words, quite innovative compounds, and a complete listing of her 334 word active vocabulary.

what he wants or does not want. In effect, he is learning the 'semantics' of the language." (p. 333).

The 1950s showed two new developments worth mentioning. The first was a new "non-educational" approach to second-language learning and bilingualism. The second was the emergence of experimental approaches to language learning and acquisition.

Second-language learning and bilingualism

For many decades the term "bilingualism" raised alarm in educational circles, as we saw in Chapter 10. Bilingual education was considered to be potentially harmful to the child, negatively affecting intelligence and school performance. Whereas linguists provided wonderful case studies of bilingual acquisition, educational psychologists ran major group studies to measure the effects of bilingualism on the child's intellectual development. It was rare for psychologists to ask the obvious psychological question: How do the two languages coexist and function in the bilingual's mind and how is this acquired? That was to change drastically from 1954 onwards.

There were two reasons that this happened. The first was the appearance of Weinreich's (1954) book *Languages in contact.* Weinreich considers the patterns of interference emerging when two or more languages are alternatively used by the same individuals. These patterns can in part be described, even explained, in structural linguistic terms. Different phonologies and different grammars tend to interfere in specific ways. But the linguist Weinreich took a psychological stance. The locus of interference is in the bilingual's mind and there is a host of psychological factors contributing to the pattern of interference, such as verbal proficiency, attitudes to the languages involved, presence of bilingual "model persons," manner of learning the other language, "switching skill," and age of acquiring the second language.

The second reason was Ervin and Osgood's (1954) theory of second-language learning and bilingualism. This theory was presented in Osgood and Sebeok's *Psycholinguistics* (1954), which gave it significant readership. Susan Ervin also presented the theory at the (unpublished) 1954 Work Conference on Research Problems in Bilingualism, held at Columbia University. The basic idea is that bilingualism varies between two poles: *compound* and *coordinate.* The compound bilingual essentially has *one* system of meanings, the coordinate bilingual has *two.* This is depicted in Figure 15.4.

Take as an example English–German bilingualism. The compound bilingual has a single mediating meaning representation for "horse/Pferd." Both the stimulus *horse* (S_A) and the stimulus *Pferd* (S_B) activate the same semantic representation ($r_m \rightarrow s_m$). The bilingual can express that one meaning by either *horse* (R_A) or *Pferd* (R_B). The coordinate bilingual has two different meaning representations, one for "horse" and another one for "Pferd." They are not entirely identical, differing in connotation ("horse" may be more playful than "Pferd" and "Pferd" may be stronger than "horse"). The one meaning is activated and expressed by *horse*, the other by *Pferd.* There is a difference in *Weltanschauung* between the two languages. You can, in principle, test how compound or coordinate a bilingual person is. The compound bilingual will rate *horse* and *Pferd* (and all other

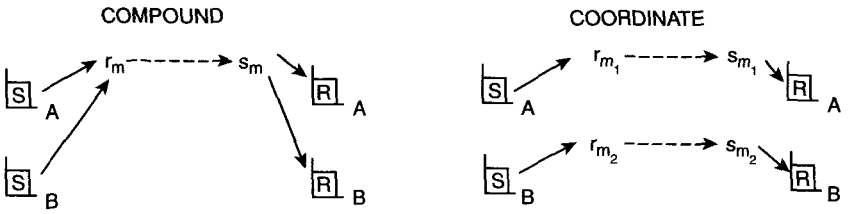


Fig. 15.4 Compound and coordinate bilingualism in terms of Osgood's mediation theory. Reproduced from *Psycholinguistics. A survey of theory and research problems*, Osgood, C.E. and Sebeok, T.A., © 1954, Indiana University Press. Reprinted with permission of Indiana University Press.

translation equivalents) the same on a semantic differential, the coordinate bilingual will show differences in rating. When you grow up with an English father and a German mother, you will most likely become a coordinate bilingual, with your father's and your mother's meaning systems separately internalized. But when you acquire your second language at school, by learning that German for *horse* is *Pferd*, and similarly for all other translation equivalents, you will become a compound bilingual.

It was Wallace Lambert (1922–2009) and coworkers who, at McGill University, put some of these ideas to experimental test. Lambert (1956) studied what happens to patterns of word association with increasing proficiency in a second language. He compared three levels of proficiency in French: undergraduates majoring in French, graduates majoring in French, and native speakers of French living in the United States. He presented his subjects with French or English stimulus words (such as *maison, libre* or *garden, happy*) and the subject was asked to provide as many word associations to the stimulus as possible during the next 45 seconds. There were 16 English and 16 French stimuli. Subjects were asked to associate either in the language of the stimulus or freely in either of the two languages. The not entirely surprising outcome of this experiment was that, with increasing proficiency (undergraduates versus graduates), the pattern of associations became more like those of the natives. The sheer number of word associations increased with proficiency. Also, some stimuli provoked more associations than others; with increasing proficiency this pattern of "provocativeness" approached the native pattern. Here Lambert was applying Noble's notion of "meaningfulness," discussed in Chapter 12. Interestingly, associations increasingly followed the habitual word order of the stimulus language, French nouns provoking more adjective responses than English nouns.

Lambert *et al.* (1958) compared coordinate and compound bilinguals, or more precisely bilinguals who had acquired their two languages (English and French) in a *separated* context (from different parents, inside versus outside the home or in different countries/cultures) or in *fused* contexts. They first tested whether Ervin and Osgood's prediction is valid, namely that translation equivalents (like *horse* and *cheval*) are rated more similarly on the semantic differential by "fused" than by "separated" bilinguals.

That indeed turned out to be the case, but only for bilinguals who had acquired their two languages in different countries/cultures. It is not enough for your two parents to speak a different language; they participate in the same, unicultural world of reference and meaning. This also showed up in a second test, on the interlingual facilitation of memory. The subject would learn a list of English words (but not to full proficiency), followed by three presentations of their exact French translations or by a list of nonsense words. They were then tested on their memory for the original English words. The "fused," but also the unicultural "separated," bilinguals profited more from the intervening list of French translations than did the "separated" bicultural bilinguals. A final test measured the speed of word translation in the two groups, but here no differences were found.

It has always surprised researchers how much learners differ in their ability to learn a second language. Carroll (1958) set out to test whether this "foreign language aptitude" can be discovered by means of specific verbal tests. He constructed a battery of no less than 30 different tests, measuring abilities such as memorizing words, picture naming, anagram construction, phrase completion, and phonetic discrimination. It was applied to 168 male students who had applied for participation in a Mandarin Chinese course. A factor analysis revealed some seven different "language aptitude" factors, six of which were predictive of success in the course. Clearly, the proficient second-language learner is not characterized by a single, privileged verbal capacity.

Experimental studies of language acquisition

What is it that the child (or adult) acquires when learning a new word? That question was rarely raised in the flood of educational studies of vocabulary growth. But as we saw in Chapter 4, the question is as old as the study of child language acquisition. The 1950s saw a cautious reopening of the debate, with a variety of experimental approaches. Feifel and Lorge (1950) still kept close to the testing approach, by asking children of different age levels to define words, as is done in the Stanford–Binet vocabulary test. However, their scoring was not primarily in terms of correct/incorrect, but in terms of the *kind* of response the child produced. Four kinds could be distinguished. There was, first, a *synonym* category, for instance when a "gown" was called a *long dress*. There was, second, a *use/description* category, for instance "orange" → *you eat it* or "straw" → *it's yellow*. Third, there is the *explanation* category, for instance "skill"→ *being able to do something well*. And finally, there is a mixed bag labeled *demonstration/repetition/illustration*, with examples such as "tap" → *tapping with finger*, "puddle"→ *a puddle of water*, or "priceless"→ *a gem*. The authors established developmental trends for these different kinds of response, over an age range from 6 to 14 years and tested 900 children. The main finding was that children increasingly opt for the synonymous type of response. The category is almost absent among 6-year-olds and primary among 14-year-olds, with explanations as a secondary category. The authors attribute these developments to an evolution in the child's mode of thinking. The child's initial word uses are concrete; they do not involve generalization. Older children recognize class features denoted by the word.

Werner and Kaplan (1950) addressed the important issue that many word meanings are induced from verbal contexts, not from referential, perceptual situations.

Their approach was to replace an existing English word by an artificial word and use it in a set of six example sentences. So, for instance, they would replace the word *stick* by the artificial term *corplum*. They would then, one by one, present the child with sentences, such as *A corplum may be used for support*, *You can make a corplum smooth with sand paper*, and *The painter used a corplum to mix his paints*. After each sentence, the child had to guess the meaning of *corplum*. The 125 children ranged from 4½ to 13½ years of age. The youngest children hardly differentiated between word and sentence meaning. The authors present this example: the artificial word *bordick* means *fault*. The child gets the sentence *People talk about the bordicks of others and don't like to talk about their own*. The child's response was *Well, bordick means people talk about others and don't talk about themselves*. Such "concepts" remain fluid and change from sentence to sentence. It was only by the age of 10 or 11 that sentence context no longer "intruded" into the child's signification of the artificial word. The paper is strikingly weak in experimental design and data analysis.

Lenneberg (1957) objected to these kinds of experiment. The child has already acquired the relevant concept; it is only given a different name in the experiment. But that is not the canonical situation in the child's learning of new words. The task for the child is to derive a hitherto unfamiliar categorization to which the new word applies, *as distinct from other words*. Lenneberg set out to compare these two conditions of learning new words. Here he returned to the color naming paradigm he had used in his tests of linguistic relativity (cf. Chapter 13). The essence of the experiment was as follows. Students were given a color name learning task, involving four color names. Zuni color names were used to make the task somewhat "realistic": how well would they be "able to learn the Zuni color system"? For the control group, the Zuni names corresponded exactly to English *brown*, *green*, *blue*, and *pink*. (i.e., equivalent to Werner and Kaplan's experiment). Hence, these subjects did not have to learn new color concepts, but only new color names. The subject was repeatedly presented with Munsell color chips, ranging in 100 steps over the full color circle. For each chip the experimenter mentioned the appropriate name. Next, the subject was given each chip and asked to rate how appropriate each of the four Zuni names was for that chip. Not surprisingly, these subjects learned to rate the Zuni names very much like (other) students had rated the chips for *brown*, *green*, *blue*, and *pink*. This went on for four sessions of continued learning. But then, for an experimental group of subjects, Lenneberg shifted or transposed the Zuni color names over the color circle. The shift was such that (for instance) one of the Zuni names was given to the transition from green to blue, i.e., to a bluish green, and similarly for the other three names. Here the subjects had to learn new color concepts, different from their native ones. In the default version of this condition, the new color concepts were quickly picked up and consistently applied.

But Lenneberg also varied another feature of the new color system, its *determinacy*. When subjects rate the appropriateness of the four English color names for all the chips on the color circle, the borders between colors are not sharp. There are regions of transition where both names are, to some extent, appropriate. Lenneberg took care to introduce the same degree of (in)determinacy when naming the chips by way of the new Zuni terms. He would name chips in the border regions with either term, using a

probability distribution that corresponded to the Zuni native ratings. In this "default condition" it was quite easy for the subjects to acquire the shifted color categories. But "bedlam" arose when the shift combined with *increased* indeterminacy. That was, of course, predictable. Fuzzy color regions are harder to learn than determinate ones. However, most interestingly, "bedlam" also arose with *decreased* indeterminacy. Here the color names became *less* ambiguous, but learning still got harder. Lenneberg concluded that what you learn is the *entire* color system with its characteristic indeterminacy. If the latter is different from what you are accustomed to, learning will be hard. In other words, Lenneberg returned full circle to the structuralists. What you acquire is not one sign after another, but a system of signs. Their meanings or "values" are determined by their mutual relations.

The last experimental study to be mentioned here is on the child's acquisition of morphology. It is Jean Berko's now classic 1958 dissertation work, which was supervised by Roger Brown (she was Brown's first doctoral student). Berko raised the following question: Do children possess morphological rules? Children produce morphologically complex words such as *girls*, *played*, *John's*, *bigger*, or *birthday*. But does that involve more than retrieval of whole words from rote memory? Or are children able to productively apply morphological rules? Here Berko was ahead of the new "syntactic" psycholinguists, who were soon to ask whether syntactic rules, such as rewrite rules and transformations, are "psychologically real." This she combined with an elegant experimental design. One example should suffice here. Figure 15.5 presents one of the test cards used in the experiment.

The experimenter would show the card to the child and read the text. It induced the child to supply the missing word. If the child said *wugs* to denote the plural of *wug*, there

Fig. 15.5 Example test card for testing the child's ability to produce the plural allomorph /-z/. Reproduced from Berko, J., The child's learning of English morphology, *Word, 14*, 150–177 © The International Linguistic Association, 1958, with permission.

is evidence for the child's awareness of a morphological rule, because *wugs* is not in the child's rote memory. In similar ways, Berko tested the child's possession of rules for past tense, possessive and adjectival inflection, diminutive formation, and compounding. Her subjects were 86 boys and girls in the age range of 4 to 7 years, plus 12 adult controls. The general finding was that, at the age of 7, children were in most cases able to produce the correct morphological response. The pre-schoolers were correct in 52% of the cases, the first-graders in 69%. Some pluralizations were harder than others: *wugs* was easy, but *nizzes* was hard, even for the first-graders. Berko concluded that, around this age, children become quite orderly and consistent in applying morphological rules. Berko also tested children's awareness of compound morphology by asking them to explain why a blackboard is called *blackboard*, an airplane *airplane*, and so on for a total of 14 compounds. Here, children's intuitions were usually incorrect. "A blackboard is called a *blackboard* because you write on it," "*Friday* is a day when you have fried fish," "An airplane is called an *airplane* because it is a plain thing that goes in the air," etc. Berko concludes: "So long as we use a compound word correctly, we can assign any meaning we like to its constituent elements." Here Berko underestimated the child's ability to produce new, transparent compounds—in fact she did not test that ability.[17] Jane Berko-Gleason would spend most of her further, productive professional career at Boston University.

Roger Brown (1958b) reconsidered the development of the child's naming skills. Is the child first using short, high-frequent terms to name things (as Zipf would have it)? That cannot be generally true, Brown argues. Children do not say *thing* to everything, although it is short and high-frequent. The child will refer to a pineapple as *pineapple* rather than *fruit*. Another rather general claim in the literature is that children acquire concrete names before abstract ones, because they "think concretely." They use *water* before *liquid* and *daddy* before *parent*. But then, counterexamples abound: *fish* comes before *perch*, *house* before *bungalow*, etc.; "children easily form large abstract categories," Brown argues (p. 18). It is not an increased faculty of abstraction that drives lexical acquisition, but finer differentiation at any level, and the adults set the pace. The child who calls all elderly ladies *aunt*, is not taught to call them *elderly ladies*. Rather, the child is taught "to cut back *aunt* to accord with standard usage" (p. 19), i.e., to differentiate. The standard word usage is one that "anticipates the equivalences and differences that will need to be observed" in most of the child's dealings with the object. Spoons are primarily to be distinguished from knives and forks, the other eating utensils, not from other metal objects. Hence, *spoon* is a more useful term than *silverware*. Brown spills no ink on behavioristic learning theory in this paper; it has simply become irrelevant.

The state of general psycholinguistics since 1951

Let us now return to Chapter 1. The 1951 Summer Seminar at Cornell University and its 1953 follow up at Indiana University established psycholinguistics on the American scene

[17] As demonstrated, among others, by Werner Deutsch (*Annual Report 1980*, Max Planck Institute for Psycholinguistics, p. 28).

as a coherent, interdisciplinary enterprise. *Psycholinguistics. A survey of theory and research problems* (1954), diligently and thoroughly edited by Charles Osgood and Thomas Sebeok, became its "charter." Equally influential, and largely covering the same ground, was George Miller's elegant text book *Language and communication* (1951), which established coherence in a scattered, multidisciplinary field.

These two landmarks in many respects characterized the zest of psycholinguistics during the 1950s. There was widely felt agreement that the study of human communication, which had been pushed by wartime engineering, was in for a major scientific effort. There was substantial common ground on method and theory for such a science, there was broadly shared zeal to cross disciplinary boundaries, there was scientific leadership, there was also substantial concentration of research talent in the Boston/Cambridge area of Massachusetts, and there was funding. The 1950s formed, on the American scene, a boom period for psycholinguistics.

Shannon's communication theory formed a widely shared theoretical base for the new enterprise. The amount of information carried by any sign transmitted from sender to receiver is not an intrinsic property of the sign, but of the set of alternatives to the sign. In addition, it is not just the number of alternatives that is at issue, but the probability distribution over the set. If an unlikely sign appears, it carries more information than if a recurrent one appears. The always evanescent notion of "context" could now be further specified in terms of (higher order) Markov chains. A new "sequential psycholinguistics" was conceived. A message is a sequence of signs (phonemes, morphemes, words, etc). Any sign's probability of appearance in a message is to some extent dependent on the previous sign. That is the sign's transitional probability. It is its well-defined context. It will also depend on the pair, the triple, etc. of preceding signs. These are "higher order" contexts, with their own higher-order transitional probabilities. There was the general expectation that a fairly limited size of context (some four or five units) would suffice to characterize a sign's probability of occurrence.

Why was that important? Substantially larger contexts would not only make the statistics of (higher order) transitional probabilities intractable as a formal characterization of a language, but also undermine a highly attractive and, again widely shared, psychological theory of verbal encoding. The transitional probability of one sign (phoneme, word) to another expresses the strength of a verbal habit. These verbal habits are stimulus–response associations, built up over years of verbal experience. The language user "runs" these associations in encoding a message. One word (or short string of words) will probabilistically "call" the next one and so on. The statistical evidence seemed to support this. Although the untenability of this concept was explicitly argued by Lashley (1951) and at least "footnotes" were placed by many others, it continued to be the default theory for most of the 1950s.

This psychological notion of verbal context in turn opened up a new perspective on "meaning," or rather on avoiding the notion of meaning in new ways. Miller and Selfridge (1950) replaced it by "contextual determination." A word's "meaning" is best characterized, or operationalized, by the set of verbal contexts that affect its probability and the set of contexts that it activates. This is, clearly, still an echo of Bloomfield's definition of

meaning: "We have defined the *meaning* of a linguistic form as the situation in which the speaker utters it and the response which it calls forth in the hearer." It is only a small step to extend this associative characterization of meaning to whole phrases and sentences. It also formed the basis for the measurement of "meaningfulness," introduced by Noble (1952) and discussed in Chapter 12. Noble's "*m*" became a widely used experimental measure.

Really innovative, and widely acknowledged during the 1950s, was Charlie Osgood's approach to meaning. There was, on the one hand, his mediation theory of meaning. Here the meaning of any stimulus is not its set of associations, but a "mediating response," which is, as we saw in Chapter 8, an internal, "lightweight" fraction of the original "total" overt response to that stimulus. The meaning of a word is some set of such fractional, internal responses, which became associated with the word during language acquisition. Here mind came in through the back door, an organization of "lightweight" internal responses and stimuli. On the other hand, Osgood provided a way of measuring such internal meanings, his semantic differential. The 1950s saw it capturing the imagination of the new psycholinguists. It was only by the end of the decade that Osgood had to accept Weinreich's (1958) critique: the semantic differential measured affective (or "connotative") meaning, not meaning in general.

The associative conception of meaning, finally, also formed the basis of the much-used practice of "content analysis" by social scientists during the 1950s. In all these respects there existed substantial consensus among psycholinguists.

Psycholinguistics also boomed in the domain of speech and word recognition. Jakobson, Fant, and Halle's (1952) theory of distinctive phonological features made it possible to connect formants and formant transitions to the discriminatory perceptual responses of listeners. Combined with the new pattern playback equipment of the Haskins Laboratories, a whole new research paradigm on phonemic perception developed during the 1950s.

The study of word recognition had received new impetus from Howes' and Solomon's discovery of the relation between recognition threshold and word frequency. It continues to be surprising that this obvious relation had not been studied in the German tradition, in spite of the fact that both the tachistoscopic expertise and sophisticated word frequency tables (Kaeding 1898) had been around for many decades. During the 1950s the discovery led to follow-up studies on whether the relation is due to purely perceptual factors or rather to response bias, an issue that would in many different ways become incorporated in modern theories of word recognition.

In addition, the 1950s saw a return of the notion of "attention" in the study of speech perception, a crucial factor mediating between stimulus and response. Here, British researchers, in particular Donald Broadbent, took the lead. From a different angle, George Miller's 1956 paper *The magical number seven plus or minus two* raised the same issue. The number of items (digits, words) you can memorize is not dependent on the amount of information (in "bits") per stimulus item, but on what Miller called the number of "chunks." A trainee Morse code operator can memorize a string of some seven auditory dits and dahs. But after some training on Morse codes for letters (d: . - .; m: - - -; c: - . - ., etc.), the operator will "recode" any string into letters and memorize the letters, not the

dits and dahs. She will now be able to memorize some seven letters, and after some more training some seven words composed from these letters. In other words, it is neither the stimulus itself, nor its set of alternatives, but its recoding in larger and larger "chunks" that determines memory span. Recoding is an attentional process. "In particular, the kind of linguistic recoding that people do seems to me to be the very lifeblood of the thought processes." (Miller 1956, p. 97).

Here we are on the cusp of the cognitive revolution. The widely shared common ground among psycholinguists during the 1950s, sketched in this section, began to crack by the end of the decade, not least because of George Miller's own initiative. In retrospect, Miller's (1954b) *Annual Review* paper on "Communication" was his final statement on the core notions of American post-war psycholinguistics. The paper has an unusual, "idiosyncratic" format. Rather than describing the notions in some coherent fashion,[18] Miller puts the authors of these notions on the stage. His selection was based entirely on "personal appeal." Here are some of the scientists that Miller selected.

Roman Jakobson, "probably the outstanding linguistic theorist alive today" (p. 401) was chosen by Miller for his theory of distinctive features. These features have psychological force, they are what psychologists have named "discriminative stimuli," those aspects of a stimulus that make the difference, that must be recognized to discriminate it from all other stimuli. Most features of any stimulus are irrelevant, but some become distinctive, because "reinforcement is contingent on just a few of these differences." (p. 401). In speech perception it is this small set of sub-phonemic features that do the discriminative work. These features, moreover, are dichotomous, putting them firmly in the framework of binary logic, which has become basic to communication engineering and neural science alike.

Zellig Harris, who stayed away from "psychologizing," is selected by Miller for his complementary distributional analysis, which "makes no reference to meaning or to any nonlinguistic evidence." It is an entirely corpus-based discovery procedure for linguistic units at any level of description. If Jakobson, unlike Harris, is not inclined to avoid the notion of meaning, "the kinds of meanings that the linguist needs can be defined with considerable objectivity in terms of behavioral discriminations. No appeals need be made to subjective intangibles in order to use meaning in a linguistic analysis." (p. 404). Here, still behaviorist Miller prefers to side with Harris: "Context can, in sufficient quantity, substitute for meaning." (p. 405).

Claude Shannon is highlighted for his insight that there is covariance between any linguistic sign and its context. A letter or word is to some extent redundant in its verbal context. Shannon made it possible to measure the span of that contextual dependency and the repeated finding is that the span is quite limited. In modern terms, there are no long-distance dependencies.

Charles Osgood is selected for his contributions to theory and measurement of meaning. "It seems that everyone who studies communication becomes involved in problems of meaning and the more involved he becomes, the slower he gets ahead. The psychologist

[18] As he did the same year in his review *Psycholinguistics* (1954a).

who unravels the meaning of meaning will break the worst bottleneck in communication research. It is conceivable that Osgood may do just that." (p. 412). Then Miller mentions both Osgood's mediation theory of meaning and his new measurement tool, the semantic differential.

Benoit Mandelbrot is chosen for his work on Zipf's law. It comes down to this conclusion: "if one wants to communicate efficiently word-by-word, then one must obey Zipf's law."

Miller concludes that the persistent theme in all of this work "has to do with the question of context. . . . After years of neglect, linguistic context has now become a very popular problem." The other recurrent theme is the "treatment of meaning" (p. 418).

Miller's highlights characterize the American general (i.e., of adults without disabilities) psycholinguistics in the mid 1950s.[19] It should be added that there had been enormous advances in experimental sophistication, which was an entirely Anglo-Saxon contribution to the field. Two features of American psycholinguistics, however, were about to be abandoned. The first was the simplistic syntax on which it was based. Although it had been argued by Carroll, Lashley, and others that planning in linguistic encoding is hierarchical in nature, a formal generative approach to such organization had not yet appeared. The only available, and totally dominant, formal tool was Markov theory. It needed Chomsky's "proof" to dismiss it (there was no proof though, as Levelt 1974/2008, Vol. II, pp. 23–26 showed).

The second, one could say "default," feature of American psycholinguistics was S-R theory. Behaviorism was, as we observed, rather irrelevant to the engineers, mathematicians, and acousticians who had jointly developed communication theory since World War II. That also held largely for students of brain and language. Only the experimental psychologists were still married to behaviorism. It was a quite persistent regional belief system in American academia, which had never affected psycholinguistics in other parts of the world.[20] It was soon to give way to mentalism in various guises. This was in part due to an intentional, organized revolution, led by Chomsky, Miller, Bruner, and others, with Chomsky's (1959) review of Skinner's *Verbal behavior* as the declaration of war. It was also due to what Thomas Kuhn would later call a paradigm shift, in this case resulting from the diminishing returns the theoretical framework of behaviorism could provide. This is what George Miller had to say about it in 1986:[21]

"Did we lead a revolution? Noam Chomsky did, and we were caught in marginal skirmishes. But I never thought of myself as a revolutionary. Quite the contrary. I was enormously conservative, looking back to a better past that needed to be revived. How could it be revolutionary to say that psychologists must explain mental processes?"

[19] The next Annual Review (1960) was by Rubenstein and Aborn.

[20] The Soviet Union was a marginal exception. There was always the obligatory curtsy to Pavlov and the notion of conditioned reflex, even in the work of Vygotsky and his associates. But Pavlov himself had been quite liberal here. His "secondary signaling system" set human mental mechanisms far apart from animal conditioning.

[21] In a personal letter to me, October 30, 1986.

Epilogue

Psycholinguistics is not a young, recent science. This book has hopefully dispelled that widespread belief among psycholinguists and non-psycholinguists alike. Its empirical roots go back to the end of the eighteenth century; the psychology of language was an established discipline by the end of the nineteenth century; and its empirical and theoretical output during the first half of the twentieth century was substantial.

There is, of course, a further book to be written: on "modern" psycholinguistics since the cognitive revolution. I won't be its author. Or rather, I have already covered an initial part of that history in my book *Formal grammars in linguistics and psycholinguistics*, which I wrote as a member of the Institute for Advanced Study in Princeton, during the academic year 1971/72. I had been invited there by Duncan Luce and also shared the distinguished company of George Miller, Aravind Joshi, Philip Johnson-Laird, and Thomas Kuhn. In addition, we regularly had joint seminars in which Jack Carroll participated. At the Institute I set out to describe what had happened to linguistics and psycholinguistics as a consequence of Chomsky's generative-transformational grammar. I believe the resulting book still is the most comprehensive treatment of that initial, 15-year history. I am grateful to the John Benjamins Publishing Company for republishing the book in 2008.

But I should leave it at that and the reason is obvious. I became a player myself in the psycholinguistic ball game, particularly when the German Max Planck Society offered me the fabulous opportunity of establishing a Max Planck Institute for Psycholinguistics in Nijmegen, the Netherlands. A history of the field should not, even in part, be autobiographical. Or, at least, I have not yet attained the required level of schizophrenia.

Still, there is, admittedly, some hidden touch of autobiography to the present book. It was George Miller who invited me to spend my first postdoctoral year at Harvard's Center for Cognitive Studies, which he directed together with Jerry Bruner. (Bruner later generously agreed to become the first chair of my Max Planck Institute's Scientific Council.) My Leiden dissertation had been in psychophysics (*On binocular rivalry*, 1965) and I went to Miller's Center as a total *tabula rasa* in psycholinguistics. I audited Chomsky's and Halle's courses at MIT and at the Center interacted with bright young psycholinguists, most of them intending to become world famous. Indeed, many of them later became leaders in the field. In the windowless office next door to me was Art Blumenthal, already working on the historical roots of psycholinguistics.

I still remember the general excitement in the Center about Dutch Heineken beer, which I had always thought to be just beer. I similarly remember the general excitement about mentalism, which I had always thought to be just normal psychology. I honestly didn't know the difference between classical and operant conditioning, but I did meet Professor Skinner (be it only in the elevator of William James Hall). I even ran some acoustic experiments in Professor Stevens' laboratory. But primarily I made an effort to

participate in the excitement of the cognitive revolution. The Center and its outstanding intellectual ambience turned me into a psycholinguist and provided me with a lifelong network of first-rate peers and friends.

I then moved on to the University of Illinois, Urbana/Champaign, where Charlie Osgood had invited me for a semester as a visiting professor. Osgood used me, as he did any visitor, as a sounding board for his developing ideas on the psycholinguistics of meaning. We spent hours and hours arguing about verbal habits, mediation theory, semantic features, fractional responses, and what have you. Osgood saw no reason whatsoever to join the bandwagon of the cognitive revolution.

I have been privileged in my personal contacts with both George Miller and Charlie Osgood, the new leaders of psycholinguistics since 1951. Some of that inevitably shines in the pages of this book.

Bibliography

Abercrombie, D. (1991). *Fifty Years in Phonetics.* Edinburgh: Edinburgh University Press.

Ach, N. (1905). *Über die Willenstätigkeit und das Denken.* Göttingen: Vandenhoeck & Ruprecht.

Ach, N. (1935). *Analyse des Willens.* Berlin: Urban & Schwarzenberg.

Ach, N. (Ed.) (1944). *Lehrbuch der Psychologie.* Band 3. *Praktische Psychologie.* Bamberg: Buchners Verlag.

Adams, S. & Powers, F.F. (1929). The psychology of language. *Psychological Bulletin, 25,* 241–260.

Ajuriaguerra, J. de & Hécaen, H. (1949, 1960) *Le cortex cérébral. Étude neuro-psycho-pathologique.* Paris: Masson.

Alajouanine, Th. (1948). Aphasia and artistic realization. *Brain, 71,* 229–241.

Alajouanine, Th. (1952). Pierre Marie et l'aphasie. *Revue Neurologique, 86,* 753–764.

Alajouanine, Th. (Ed.) (1955). *Les grandes activités du lobe temporal.* Paris: Masson.

Alajouanine, Th. (1968). *L'Aphasie et le langage pathologique.* Paris: Baillière & Fils.

Alajouanine, Th., Ombredane, A. & Durand, M. (1939). *Le syndrome de désintegration phonétique dans l'aphasie.* Paris: Masson.

Allport, F.H. (1924). *Social Psychology.* Boston: Houghton Mifflin Company.

Allport, G.W. (1935). *Phonetic symbolism in Hungarian words.* Unpublished doctoral dissertation, Harvard University.

Allport, G.W. & Cantril, H. (1934). Judging personality from voice. *Journal of Social Psychology; Political, Racial and Differential Psychology, 5,* 37–55.

Allport, G.W. & Vernon, P.E. (1931). *A study of values.* Boston: Houghton-Mifflin.

Alspach, E.M. (1917). On the psychological response to unknown proper names. *American Journal of Psychology, 28,* 436–443.

Ament, W. (1899). *Die Entwicklung von Sprechen und Denken beim Kinde.* Leipzig: Ernst Wunderlich.

Aphasia debate (1908). *Revue Neurologique, 16,* 611–635, 974–1023, 1025–1047.

Arsenian, S. (1937). *Bilingualism and mental development.* New York City: Bureau of Publications Teachers College, Columbia University.

Aschaffenburg, G. (1896). Experimentelle Studien über Associationen. In: E. Kraepelin (Ed.), *Psychologische Arbeiten.* Vol. 1. Leipzig: Engelmann, pp. 209–296.

Aschaffenburg, G. (1899). Experimentelle Studien über Associationen. II. Theil. Die Associationen in der Erschöpfung. In E. Kraepelin (Ed.). *Psychologische Arbeiten.* Vol. 2. Leipzig: Engelmann, pp. 1–83.

Ash, M.G. (1988). Die Entwicklung des Wiener Psychologischen Instituts 1922–1938. In: A. Eschbach (Ed.), *Karl Bühler's theory of language.* Amsterdam: John Benjamins, pp. 303–325.

Ash, M.G. (1995). *Gestalt psychology in German culture 1890–1967. Holism and the quest for objectivity.* Cambridge: Cambridge University Press.

Atherton, M.V. & Washburn, M.F. (1912). Mediate associations studied by the method of inhibiting associations. *American Journal of Psychology, 23,* 101–109.

Auburtin, E. (1863). *Considérations sur les localisations cérébrales et en particulier sur le siége de la faculté du langage articulé.* Paris: Victor Masson & Fils.

Auerbach, F. (1913). Das Gesetz der Bevölkerungskonzentration. *Geographische Mitteilungen, 59,* 74–76.

Austin, J. (1962). *How to do things with words.* Oxford: Clarendon Press.

Ayres, L.P. (1915). *A measuring scale for ability in spelling.* New York: Russell Sage Foundation.

Baayen, R.H., Piepenbrock, R., & Gulikers, L. (1996). *The CELEX Lexical Database (CD-ROM).* Linguistic Data Consortium. Philadelphia, PA: University of Pennsylvania.

Bachman-Mann, M. (1944). The quantitative differentiation of samples of spoken language. In: W. Johnson (Ed.) Studies in language behavior. *Psychological Monographs, 56,* Whole No. 255, pp. 41–74.

Baginsky (1871). Aphasie in Folge schwerer Nierenerkrankungen – Uraemie. *Berliner klinische Wochenschrift, 8,* 428–431.

Bagley, W.C. (1900). The apperception of the spoken sentence: A study in the psychology of language. *American Journal of Psychology, 12,* 80–130.

Balassa, J. (1893). A gyermek nyelvének fejlödése. *Nyelvtudományi Közlemények, 23,* 60–73, 129–144.

Baldwin, B.T. (Ed.) (1920). In memory of Wilhelm Wundt. Reproduced in W.G. Bringmann, & R.D. Tweney (Eds.). (1980). *Wundt studies. A centennial collection.* Toronto: Hogrefe, 280–308.

Baldwin, B.T. & Smith, M. (1925). Physical growth of two generations of one family. *Journal of Heredity, 16,* 243–258.

Ballet, G. (1888). *Le langage intérieur et les diverses formes de l'aphasie.* Paris: Félix Alcan.

Barat, L. (1917). Le langage. *Revue Philosophique, 83,* 105–131.

Barlow, M.C. (1928). The rôle of articulation in memorizing. *Journal of Experimental Psychology, 11,* 306–312.

Barrows, S. (1916). Experimental phonetics as an aid to the study of language. *Pedagogical Seminary, 23,* 63–75.

Bartlett, F.C. (1932). *Remembering. A study in experimental and social psychology.* Cambridge: Cambridge University Press.

Bartlett, F.C. & Smith, E.M. (1920). Is thinking merely the action of language mechanisms? *British Journal of Psychology, 11,* 55–62.

Bastian, H.C. (1865). The human brain. *Macmillan's Magazine,* 13, 63–71.

Bastian, H.C. (1869). On the various forms of loss of speech in cerebral disease. *British and Foreign Medical and Chirurgical Review, 43,* 209–236; 470–492.

Bastian, H.C. (1880). *The brain as an organ of mind.* London: Kegan Paul.

Bastian, H.C. (1897). The Lumleian Lectures. Some problems in connection with aphasia and other speech defects. *Lancet, 1,* 933–942, 1005–1017, 1131–1137, 1187–1194.

Bateman, W.G. (1916). The language status of three children at the same age. *Pedagogical Seminary, 23,* 211–240.

Bateman, W.G. (1917). Papers on language development. I. The first word. *Pedagogical Seminary, 24,* 391–398.

Baudouin de Courtenay, J. (1869). Einige Beobachtungen an Kindern. *Beiträge zur vergleichenden Sprachforschung, 6,* 215–220.

Bawden, H.H. (1900). A study of lapses. *Psychological Review. Monograph Supplement #3,* 14.

Bean, C.H. (1932). An unusual opportunity to investigate the psychology of language. *Journal of Genetic Psychology, 40,* 181–202.

Becher, E. (1904). Experimentelle und kritische Beiträge zur Psychologie des Lesens bei kurzen Expositionszeiten. *Zeitschrift für Psychologie, 36,* 19–73.

Benjamin Jr., L.T., Whitaker, J.L., Ramsey, R.M., & Zeve, D.R. (2007). John B. Watson's Alleged Sex Research. An Appraisal of the Evidence. *American Psychologist, 62,* 131–139.

Bentley, M. & Varon, E.J. (1933). An accessory study of "phonetic symbolism". *American Journal of Psychology, 45,* 76–86.

Berelson, B. (1952). *Content analysis in communications research*. Glencoe, IL: Free Press.

Berger, H. (1929). Ueber das Elektroenkephalogramm des Menschen. *Archiv für Psychiatrie und Nervenkrankheiten, 87*, 527–570.

Berko, J. (1958). The child's learning of English morphology. *Word, 14*, 150–177.

Binet, A. (1903). *L'étude expérimentale de l'intelligence*. Paris: Librairie Reinwald.

Binet, A. & Henri, V. (1894). La mémoire des phrases. *L' Année Psychologique, 1*, 24–59.

Bingham, W.E. (1943). A study of the relations which the galvanic skin response and sensory reference bear to judgments of the meaningfulness, significance, and importance of 72 words. *Journal of Psychology, 16*, 21–34.

Binswanger, L. (1910). Über das Verhalten des psychogalvanischen Phänomens beim Assoziationsexperiment. In: C.G. Jung (Ed.), *Diagnostische Assoziationsstudien. Beiträge zur experimentellen Psychopathologie*. Leipzig: Johann Ambrosius Barth, pp. 113–195.

Blagoveščenskij, V. (1886). Detskaja reč' [Child speech]. *Russkij filologičeskij vestnik, 16*.

Blankenship, A.B. (1938). Memory span: A review of the literature. *Psychological Bulletin, 35*, 1–25.

Bloch, O. (1921). Les premiers stades du langage de l'enfant. *Journal de Psychologie, 18*, 693–712.

Bloomfield, L. (1914). *An introduction to the study of language*. New York: Henry Holt.

Bloomfield, L. (1926). A set of postulates for the science of language. *Language, 2*, 153–164.

Bloomfield, L. (1931). Albert Weiss. *Language, 7*, 219–221.

Bloomfield, L. (1933). *Language*. New York: Henry Holt. (1976 edition London: Allen & Unwin).

Bloomfield, L. (1936). Language or ideas? *Language, 12*, 89–95.

Bloomfield, L. (1943). Meaning. *Monatshefte für Deutschen Unterricht, 35*, 101–106.

Blumenthal, A.L. (1970). *Language and psychology: Historical aspects of psycholinguistics*. New York: John Wiley.

Blumenthal, A. L. (1980). Wilhelm Wundt – Problems of interpretation. In: W.G. Bringmann & R.D. Tweney, R.D. (Eds.). *Wundt studies. A centennial collection*. Toronto: Hogrefe. pp. 435–445.

Boas, F. (1889). On alternating sounds. *American Anthropologist, 2*, 47–54.

Boas, F. (1911a). *Handbook of American Indian languages. Part I*. Washington: Government Print Office (Smithsonian Institution, Bureau of American Ethnology).

Boas, F. (1911b). *The mind of primitive man*. New York: Macmillan.

Boas, F. (1942). Language and culture. In: W.G. Leland (Ed.). *Studies in the history of culture: the disciplines of the humanities*. Menasha, WI: Banta, pp. 178–184.

Bock, J.K. (1995). Sentence production: from mind to mouth. J.L. Miller and P.D. Eimas (Eds.), *Handbook of perception and cognition*. Vol. 11: *Speech, language and communication*. San Diego: Academic Press, pp. 181–216.

Bock, J.K., & Miller, C. A. (*1991*). *Broken agreement. Cognitive Psychology, 23*, 45–93.

Boder, D.P. (1927). The adjective-verb-quotient: a contribution to the psychology of language. *Psychological Record, 3*, 310–343.

Bolin, I., & Bolin, M. (1916). De två första årens språkutvekling hos en svensk flicka. *Svenkst arkiv för pedagogik, 4*, 159–223.

Bolin, I., & Bolin, M. (1920). Psykologiska och språkliga iakttagelse rörande en svensk flicka. *Svenkst arkiv för pedagogik, 8*, 1–55.

Bonhoeffer, K. (1902). Zur Kenntniss der Rückbildung motorischer Aphasien. *Mitteilungen aus den Grenzgebieten der Medizin und Chirurgie, 10*, 203–224.

Bonhoeffer, K. (1924). Die Unfruchtbarmachung der geistig Minderwertigen. *Klinische Wochenzeitschrift, 3*, 798–801.

Bopp, F. (1833–1852). *Vergleichende Grammatik des Sanskrit, Zend, Griechischen, Lateinischen, Litthauischen, Gothischen und Deutschen*. 6 Vols. Berlin: Dümmler.

Bouillaud, J. (1825). Recherches cliniques propres à démontrer que la perte de la parole correspond à la lésion des lobules antérieures du cerveau, et à confirmer l'opinion de M. Gall sur le siège de l'organe du language articulé. *Archives Générale de Médecine, 3*, 25–45.

Bourdon, B. (1892). *L'Expression des émotions & des tendances dans le langage*. Paris: Félix Alcan.

Brandenburg, G.C. (1918). Psychological aspects of language. *Journal of Educational Psychology, 9*, 313–332.

Bredsdorff, D.J.H. (1821, 1886) *Om Aarsagerne til Sprogenes Forandringer*. Copenhagen: Gyldendalske.

Brenner, R. (1940). An experimental investigation of memory span. *Journal of Experimental Psychology, 26*, 467–482.

Brewer, W.F. (1994). Charles Egerton Osgood: 1916–1991. *American Journal of Psychology, 107*, 583–596.

Bringmann, W.G. & Tweney, R.D. (Eds.). (1980). *Wundt studies. A centennial collection*. Toronto: Hogrefe.

Bringmann, W.G., Bringmann, N.J., & Ungerer, G.A. (1980). The establishment of Wundt's laboratory: An archival and documentary study. In: W.G. Bringmann & R.D. Tweney (Eds.). *Wundt studies. A centennial collection*. Toronto: Hogrefe, pp.123–157.

Brinton, D.G. (1890). *Essays of an Americanist*. Philadelphia: Porter & Coates.

Briones, I.T. (1937). An experimental comparison of two forms of linguistic learning. *Psychological Record, 1*, 205–214.

Broadbent, D.E. (1952a). Speaking and listening simultaneously. *Journal of Experimental Psychology, 43*, 267–273.

Broadbent, D.E. (1952b). Listening to one of two synchronous messages. *Journal of Experimental Psychology, 44*, 51–55.

Broadbent, D.E. (1952c). Failures of attention in selective listening. *Journal of Experimental Psychology, 44*, 295–303.

Broadbent, D.E. (1957a). A mechanical model of human attention and immediate memory. *Psychological Review, 64*, 205–215.

Broadbent, D.E. (1957b). *Perception and communication*. London: Pergamon Press.

Broadbent, W.H. (1872). On the cerebral mechanism of speech and thought. Abstract. *Transactions of the Royal Medical & Chirurgical Society London*, February 18, 24–29. 2nd ser. 55, 145–194.

Broca, P. (1861a). Perte de la parole, ramouissement chronique et destruction partielle du lobe antérieur gauche du cerveau. *Bulletins de la Société Anthropologique de Paris, 2*, 235–238.

Broca, P. (1861b). Remarques sur le siège de la faculté du langage articulé, suivies d'une observation d'aphémie (perte de la parole). *Bulletin et mémoires de la Société Anatomique de Paris, 6*, 330–357.

Broca, P. (1861c). Nouvelle observation d'aphémie produite par une lésion de la moitié postérieure des deuxième et troisième circonvolutions frontales. *Bulletin et mémoires de la Société Anatomique de Paris, 6*, 398–407.

Broca, P. (1863). Localisation des fonctions cérébrales: Siège de langage articulé. *Bulletin de la Société d'Anthropologie, 4*, 398–407.

Broca, P. (1865). Sur le siège de la faculté du langage articulé (15 juin). *Bulletins de la Société Anthropologque de Paris, 6*, 377–393.

Brodbeck, A.J. & Irwin, O.C. (1946). The speech behaviour of infants without families. *Child Development, 17*, 145–156.

Brown, R. (1957). Linguistic determinism and the parts of speech. *Journal of Abnormal and Social Psychology, 55*, 1–5.

Brown, R. (1958a). *Words and things*. Glencoe, IL: The Free Press.

Brown, R. (1958b). How shall a thing be called? *Psychological Review, 65*, 14–21.

Brown, R. (1973). *A first language. The early stages*. Cambridge, MA: Harvard University Press.

Brown, R. (1976). Reference. A memorial tribute to Eric Lenneberg. *Cognition, 4*, 125–153.

Brown, R., Black, A.H., & Horrowitz, A.E. (1955). Phonetic symbolism in natural languages. *Journal of Abnormal and Social Psychology, 50*, 388–393.

Brown, R. & Lenneberg, E.H. (1954). A study in language and cognition. *Journal of Abnormal and Social Psychology, 49*, 454–462.

Brown, W. (1915). Practice in associating color-names with colors. *Psychological Review, 22*, 45–55.

Brugmann, K. (1904). *Die Demonstrativpronomina der indogermanischen Sprachen. Eine bedeutungsgeschichtliche Untersuchung.* Leipzig: Teubner.

Bruner, J.S. (1983). *In search of mind. Essays in autobiography.* New York: Harper.

Bruner, J.S., Miller, G.A., & Zimmerman, C. (1955). Discriminative skill and discriminative matching in perceptual recognition. *Journal of Experimental Psychology, 49*, 187–192.

Bühler, C. (1918). Über Gedankenentstehung. *Zeitschrift für Psychologie und Physiologie der Sinnesorgane. Abteilung 1: Zeitschrift für Psychologie, 80*, 129–200.

Bühler, C. (1930). *The first year of life.* New York: John Day.

Bühler, C. (1933). *Der menschlichen Lebenslauf als psychologisches Problem.* Leipzig: Hirzel.

Bühler, C. (1965). Die Wiener Psychologische Schule in der Emigration. *Psychologische Rundschau, 16*, 187–196.

Bühler, C. (1972). Autobiographie. In: L.J. Pongratz, W. Traxel & E.G. Wehner (Eds.). *Psychologie in Selbstdarstellungen.* Vienna: Hans Huber. pp. 9–42.

Bühler, C. (1984). Karl Bühler. Eine biographische Skizze. In: A. Eschbach (Ed.) *Bühler Studien.* Vol. I. Frankfurt aM: Sührkamp.

Bühler, C. & Hetzer, H. (1928). Das erste Verständniss für Ausdruck im ersten Lebensjahr. *Zeitschrift für Psychologie, 107*, 50–61.

Bühler, K. (1907). Tatsachen und Probleme zu einer Psychologie der Denkvorgänge. I Über Gedanken. *Archiv für die gesamte Psychologie, 9*, 297–365.

Bühler, K. (1908a). Tatsachen und Probleme zu einer Psychologie der Denkvorgänge. II Über Gedankenzusammenhänge. *Archiv für die gesamte Psychologie, 12*, 1–23.

Bühler, K. (1908b). Tatsachen und Probleme zu einer Psychologie der Denkvorgänge. III Über Gedankenerinnerungen. *Archiv für die gesamte Psychologie, 12*, 24–92.

Bühler, K. (1908c). Nachtrag. Antwort auf die von W. Wundt erhobenen Einwände gegen die Methode der Selbstbeobachtung an experimentell erzeugen Erlebnissen. *Archiv für die gesamte Psychologie, 12*, 93–122.

Bühler K. (1909a). Über das Sprachverständniss vom Standpunkt der Normalpsychologie aus. *Bericht über den III. Kongreß für experimentelle Psychologie* (Frankfurt 1908). Leipzig: Johann Ambrosius Barth, pp. 94–130.

Bühler, K. (1909b). Anton Marty, Untersuchungen zur Grundlegung der allgemeinen Grammatik und Sprachphilosophie. *Göttingische gelehrte Anzeigen, 171*, 947–979.

Bühler, K. (1909c). Jac. van Ginneken, Principes de linguistique psychologique. *Zeitschrift für Psychologie, 51*, 274–294.

Bühler, K. (1913). *Die Gestaltwahrnehmungen. Experimentelle Untersuchungen zur psychologischen und aesthetischen Analyse der Raum- und Zeitwahrnehmung.* Stuttgart: Spemann.

Bühler, K. (1918a). *Die geistige Entwicklung des Kindes.* Jena: Fischer.

Bühler, K. (1918b). Kritische Musterung der neuen Theorien des Satzes. *Indogermanisches Jahrbuch, 6*, 1–20.

Bühler, K. (1924). Das Forschungsprogramm des psychologischen Instituts Wien. *Pädagogisches Jahrbuch, 43*, 72–73.

Bühler, K. (1927). *Die Krise der Psychologie.* Jena: Fischer.

Bühler, K. (1929). Europa und der Amerikanische Kulturwille. *Europäische Revue, 5*, 635–650.

Bühler, K. (1931). Phonetik und Phonologie. *Réunion phonologique internationale tenue à Prague (18–21/XII 1930). Traveaux du Cercle Linguistique de Prague*. Prague: Jednota českých matematiků a fysiků, pp. 22–52.

Bühler, K. (1932). Das Ganze der Sprachtheorie, ihr Aufbau und ihre Teile. *Bericht über den 12. Kongreß der Deutschen Gesellschaft für Psychologie* (Hamburg 1931). Jena: Gustav Fischer, pp. 95–122.

Bühler, K. (1933). *Ausdruckstheorie. Das System an der Geschichte aufgezeigt.* Jena: Gustav Fischer.

Bühler, K. (1934). *Sprachtheorie. Die Darstellungsfunktion der Sprache*. Jena: Gustav Fisher. (2nd edition: Stuttgart: Gustav Fischer, 1965).

von Bunge, G. (1905). *Lehrbuch der Physiologie des Menschen*. 2 Vols. Leipzig: Vogel Verlag.

Burckhardt, G. (1891). Über Rindenexzisionen als Beitrag zur operativen Therapie der Psychosen. *Allgemeines Zeitschrift für Psychiatrie und Psychiatrisch-Gerichtliche Medizin, 47*, 463–548.

Bureau International d'Education (1928). *Le bilingualisme et l'éducation*. Travaux de la Conférence Internationale (2–5 avril 1928). Genève-Luxembourg.

Burnett, J. (1773–1792). *The origins and progress of man and language*. 6 Vols. Edinburgh: Kincaid & Creech; London: Cadell.

Busemann, A. (1925). *Die Sprache der Jugend als Ausdruck der Entwicklungsrhythmik*. Jena: Fischer.

Buswell, G.T. (1921) The relationship between eye-perception and voice response in reading. *Journal of Educational Psychology, 12*, 217–227.

Butfield, E. & Zangwill, O. (1946). Reeducation in aphasia: A review of 70 cases. *Journal of Neurology, Neurosurgery and Psychiatry, 9*, 75–79.

Butler, R.A. & Galloway, F.T. (1957). Factor analysis of the delayed speech feedback phenomena. *Journal of the Acoustical Society of America, 29*, 632–635.

Campe, J.H. (1785–1792). *Allgemeine Revision des gesammten Schul- und Erziehungswesens von einer Gesellschaft praktischer Erzieher*. 16 Vols. Hamburg: Carl Ernst Bohn.

Carroll, J.B. (1938). Diversity of vocabulary and the harmonic series law of word-frequency distribution. *Psychological Record, 2*, 379–386.

Carroll, J.B. (1939). Determining and numerating adjectives in children's speech. *Child Development, 10*, 215–229.

Carroll, J.B. (1941). A factor analysis of verbal abilities. *Psychometrika, 6*, 279–307.

Carroll, J.B. (1944). The analysis of verbal behavior. *Psychological Review, 51*, 102–119.

Carroll, J.B. (1951a). *Report and recommendations of the Interdisciplinary Summer Seminar in Psychology and Linguistics*. Cornell University, June 18–August 10.

Carroll, J.B. (1951b). The Interdisciplinary Summer Seminar on Linguistics and Psychology. *Social Science Research Council Items, 5*, 40–41.

Carroll, J.B. (1953). *The study of language. A survey of linguistics and related disciplines in America*. Cambridge, MA: Harvard University Press.

Carroll, J.B. (1958). A factor analysis of two foreign language aptitude batteries. *Journal of General Psychology, 59*, 3–19.

Carroll, J.B. (1960). Language acquisition, bilingualism, and language change. *Encyclopedia of Educational Research*. New York: Macmillan. Reprinted in Saporta (Ed., 1961), pp. 331–345.

Carroll, J.B. (1963). Linguistic relativity, contrastive linguistics and language learning. *International Review of Applied Linguistics, 1*, 1–20.

Carroll, J.B. & Casagrande, J.B. (1958). The function of language classifications in behavior. In: E.E. Maccoby, T.M. Newcomb & E.L. Hartley (Eds.). *Readings in social psychology*. New York: Henry Holt, pp. 18–31.

Carter Moore, K. (1896). The mental development of a child. *Psychological Review, Monograph Supplement #3*.

Cason, H. (1934). The verbal activities in the conditioning of human subjects. *Psychological Review, 41*, 563–571.

Cassirer, E. (1923). *Philosophie der symbolische Formen: Die Sprache.* Vol. I. Darmstadt: Wissenschaftliche Buchgesellschaft.

Cattell, J.M. (1885). Ueber die Zeit der Erkennung und Benennung von Schriftzeichen, Bildern und Farben. *Philosophische Studien, 2*, 635–650.

Cattell, J.M. (1886a). Psychometrische Untersuchungen. *Philosophische Studien, 3*, 452–492.

Cattell, J.M. (1886b). The time taken up by cerebral operations. Parts 1 & 2. *Mind, 11*, 220–242.

Cattell, J.M. (1886c). The time taken up by cerebral operations. Part 3. *Mind, 11*, 377–392.

Cattell, J.M. (1887a). The time taken up by cerebral operations. Part 4. *Mind, 11*, 524–538.

Cattell, J.M. (1887b). Experiments on the association of ideas. *Mind, 12*, 68–74.

Cattell, J.M. & Bryant, S. (1889). Mental association investigated by experiment. *Mind, 14*, 230–250.

Chamberlain, A.F. (1890). Notes on Indian child-language. *American Anthropologist, 3*, 237–241.

Chamberlain, A.F. (1893). Further notes on Indian child language. *American Anthropologist, 6*, 321–322.

Chamberlain, A.P. & I.C. (1904). Studies of a child. I. *Pedagogical Seminary, 11*, 264–291.

Chao, Y.R. (1951). The Cantian ideolect: An analysis of the Chinese spoken by a twenty-eight-months-old child. *University of California Publications in Semitic Philolology, 1*, 27–44.

Chase, S. (1938). *The tyranny of words.* London: Methuen & Co.

Cherry, C. (1953). Some experiments on the recognition of speech, with one and with two ears. *Journal of the Acoustical Society of America, 25*, 975–979.

Cherry, C. (1957). *On human communication.* Cambridge, MA: MIT Press.

Cherry, E.C., Halle, M., & Jakobson, R. (1953). Towards a logical description of languages in their phonemic aspects. *Language, 29*, 34–46.

Cherry, C. & Taylor, W.K. (1954). Some further experiments upon the recognition of speech, with one and with two ears. *Journal of the Acoustical Society of America, 26*, 554–559.

Chomsky, N. (1955). *The logical structure of linguistic theory.* Cambridge, MA: Mimeo MIT.

Chomsky, N. (1959). *Review of* B.F. Skinner, *Verbal Behavior. Language, 35*, 26–58.

Chotlos, J.W. (1944). A statistical and comparative analysis of individual written language samples. In: W. Johnson (Ed.) Studies in language behavior. *Psychological Monographs, 56*, Whole No. 255, pp. 77–111.

Claparède, E. (1914). Sur la réprésentation des personnes inconnus, et des lapsus linguae. *Archives de Psychologie, 14*, 301–304.

Clark, H. (1996). *Using language.* Cambridge: Cambridge University Press.

Clarke, H.M. (1911). Conscious attitudes. *American Jounnal of Psychology, 22*, 214–249.

Cofer, C.N. & Foley, J.P. (1942), Mediated generalization and the interpretation of verbal behavior: I. Prolegomena. *Psychological Review, 49*, 513–539.

Cofer, C.N., Janis, M.G., & Rowell, M.N. (1943). Mediated generalization and the interpretation of verbal behavior: III. Experimental study of antonym gradients. *Journal of Experimental Psychology, 32*, 266–269.

Cohen, A. (1966). Errors of speech and their implications for understanding the strategy of language users. *Zeitschrift für Phonetik, 21*, 177–181.

Cohen, M. (1952). Sur l'étude du langage enfantin. *Enfance, 5*, 181–249.

Cole, M. & Cole, S. (1979). *Epilogue: A portrait of Luria.* In: A.R. Luria: *The making of mind. A personal account of soviet psychology.* Cambridge, MA: Harvard University Press, pp. 189–225.

Colvin, S.S. & Allen, R.A. (1923). Mental tests and linguistic ability. *Journal of Educational Psychology, 14*, 1–20.

Compayré, G. (1893, 6th edition 1913). *L'évolution intellectuelle et morale de l'enfant.* Paris: Hachette.

Condon, E.U. (1928). Statistics of vocabulary. *Science, 67,* 300.

Conrad, K. (1954). New poblems of aphasia. *Brain, 77,* 491–509.

Cooley, C.H. (1908). A study of the early use of self-words by a child. *Psychological Review, 15,* 339–357.

Cooper, F.S., Liberman, A.M., & Borst, J. (1951). The interconversion of audible and visible patterns as a basis for research in the perception of speech. *Proceedings of the National Academy of Sciences, 37,* 318–325.

Cross, E. (1950). Some features of the phonology of a four-year-old boy. *Word, 6,* 137–140.

Cutler, A. (2001). Listening to a second language through the ears of a first. *Interpreting, 5,* 1–23.

Darcy, N.T. (1953). A review of the literature on the effects of bilingualism upon the measurement of intelligence. *Journal of Genetic Psychology, 82,* 21–57.

Darlington, C.D. & Mather, K. (1949). *On the Elements of Genetics.* London: Allen and Unwin.

Darwin, C. (1859). *On the origin of species by means of natural selection.* London: John Murray.

Darwin, C. (1871). *The descent of man.* 2 Vols. London: John Murray.

Darwin, C. (1872). *The expression of emotions in man and animal.* London: John Murray.

Darwin, C. (1877). A biographical sketch of an infant. *Mind. A Quarterly Review of Psychology and Philosophy, 2,* 285–294.

Dashiell, J.F. (1925). A physiological-behavioristic description of thinking. *Psychological Review, 32,* 54–73.

Davis, E. (1937). *The development of linguistic skills in twins, singletons with siblings, and only children from age five to ten years.* University of Minnesota Institute of Child Welfare Monograph Series, Number XIV. Minnesota: Minnesota Institute of Child Welfare.

Dax, M. (1865). Lésions de la moitié gauche de l'encéphale coïncident avec l'oublie des signes de la pensée. – Lu au Congrès méridional tenu à Montpellier en 1836, par le docteur Marc Dax. *Gazette Hebdomadaire de Médecine et de Chirurgie, 17,* 259–260.

Dax, M.G. (1865). Sur le même sujet. *Gazette Hebdomadaire de Médecine et de Chirurgie, 17,* 260–262.

Day, E.J. (1932). The development of language in twins. I. A comparison of twins and single children; I. The development of twins: Their resemblances and differences. *Child Development, 3,* 179–199; 289–316.

Dearborn, W.F. (1906). *The psychology of reading. An experimental study of the reading pauses and movements of the eye.* New York: Columbia University Contributions to Philosophy and Psychology, *14,* Whole No. 1

Decroly, O. (1934). *Comment l'enfant arrive à parler.* Brussels: Centrale de P.E.S. de Belgique.

Dediu, D. & Ladd, D.R. (2007). Linguistic tone is related to the population frequency of the adaptive haplogroups of two brain size genes, ASPM and Microcephalin. *Proceedings of the National Academy of Sciences,* 104, 10944–10949.

Deese, J. & Kaufman, R.A. (1957). Serial effects in recall of unorganized and sequentially organized verbal material. *Journal of Experimental Psychology,* 54, 180–187.

Degérando, J.-M. (1800). *Considérations sur les diverses méthodes à suivre dans l'observation des peuples sauvages.* Republished as "Documents anthropologiques: L'Ethnographie en 1800." *Revue d'Anthropologie (1883), IIe série. tome VI,* 152–182.

Degérando, J.-M. (1827). *De l'education des sourds-muets de naissance.* Paris: Méquignon l'Ainé Père.

De Groot, A. (2011). *Language and cognition in bilinguals and multilinguals.* New York: Psychology Press.

De Jaager, J.J. (1865). *De physiologische tijd bij psychische processen.* Utrecht: P. W. van de Weijer.

Dejerine, J. (1891). Sur un cas de cécité verbale avec agraphie suivi d'autopsie. *Mémoires de la Société de Biologie, 3,* 197–201.

Dejerine, J. (1892). Contribution à l'étude anatomo-pathologique et clinique des différents variétés de cécité verbale. *Mémoires de la Societé de Biologie,* 4, 61–90.

Dejerine, J. (1906a). L'aphasie sensorielle: Sa localisation et sa physiologie pathologique. *Presse Médicale, 57*, 437–439.

Dejerine, J. (1906b). L'aphasie motrice: Sa localisation et sa physiologie pathologique. *Presse Médicale, 57*, 453–457.

Dejerine, J. (1914). *Sémiologie des affections du système nerveux.* Paris: Masson et Cie.

Delacroix, H. (1924). *Le langage et le pensée.* Paris: Librairie Félix Alcan.

Delacroix, H. (1933) (Ed.). *Psychologie du langage.* Paris: Librairie Félix Alcan.

Delacroix, H. (1934). *L'Enfant et le langage.* Paris: Librairie Félix Alcan.

De Laguna, G. (1927). *Speech. Its function and development.* New Haven: Yale University Press.

Delattre, P.C., Liberman, A.M., Cooper, F.S., & Gerstman, L.J. (1952). An experimental study of the acoustic determinants of vowel color; observations on one- and two-formant vowels synthesized from spectrographic patterns. *Word, 8*, 195–210.

Delbrück, B. (1886). Amnestische Aphasie. *Jena'sche Zeitschrift für Naturwissenschaft, 20*, Supplement II.

Delbrück, B. (1901). *Grundfragen der Sprachforschung. Mit Rücksicht auf W. Wundts Sprachpsychologie erörtet.* Strasbourg: Trübner Verlag.

De Renzi, E. & Vignolo, L.A. (1962). The token test: a sensitive test to detect receptive disturbances in aphasics. *Brain, 85*, 665–678.

Descoeudres, A. (1921). *Le développement de l'enfant de deux à sept ans.* Paris: Delachaux & Niéstle.

de Sola Pool, I. (1959) (Ed.). *Trends in content analysis.* Urbana, IL: University of Illinois Press.

Deußing, H. (1927). *Der sprachliche Ausdruck des Schulkindes. Statistische und experimentelle Untersuchungen zur Entwicklungspsychologie der Sprache.* Langensalza: Jenaer Beiträge zur Jugend- und Erziehungspsychologie.

Deutsch, W. (Ed.) (1991). *Über die verborgene Aktualität von William Stern.* Frankfurt a.M.: Peter Lang.

Deutsch, W. (1994). Nicht nur Frau und Mutter – Clara Sterns Platz in der Geschichte der Psychologie. *Psychologie und Geschichte, 5*, 171–182.

Deutsch, W. (2009). Reference to self. A Sternian approach to the first steps in language development. In: W. Mack & G. Reuter (Eds.), *Social roots of self-consciousness.* Berlin: Akademie Verlag, pp. 187–203.

Deutsch, W. & Budwig, N. (1983). Form and function in the development of possessives. *Papers and Reports on Child Language Development, Stanford, 22*, 36–42.

Deville, G. (1890, 1891). Notes sur le développement du language. *Revue de linguistique et de philologie comparée: recueil trimestriel, 23*, 330–343; *24*, 10–42, 128–143, 243–257, 300–320.

Dewey, G. (1923). *Relative frequency of English speech sounds.* Cambridge, MA: Harvard University Press.

Dewey, J. (1894). The psychology of infant language. *Psychological Review, 1*, 63–66.

Dewey, J. (1910). *How we think.* Boston: Heath & Co.

Dewey, J. (1922). Knowledge and speech reaction. *Journal of Philosophy, 19*, 561–570.

Diamond, S. (2001). Wundt before Leipzig. In: W.R. Rieber & D.K. Robinson (Eds.). *Wilhelm Wundt in history. The making of scientific psychology.* Dordrecht/New York: Kluwer/Plenum, pp.1–68.

Diebold, A.R. (1965). A survey of psycholinguistic research, 1954–1964. In: C.E. Osgood & T.A. Sebeok, T.A. (Eds.) (1965). *Psycholinguistics. A survey of theory and research problems.* Bloomington: Indiana University Press, pp. 205–275.

Dittrich, O. (1903a). Besprechung Wilhelm Wundt, Völkerpsychologie. Die Sprache. *Zeitschrift für romanische Philologie, 27*, 198–216.

Dittrich, O. (1903b). *Grundzüge der Sprachpsychologie.* Halle: Max Niemeyer.

Dodge, R. (1896). *Die motorischen Wortvorstellungen.* Halle: Max Niemeyer.

Dodge, R. & Cline, T.S. (1901). The angular velocity of eye movements. *Psychological Review, 8*, 145–157.

Donders, F.C. (1864). Zur Klangfarbe der Vocale. *Annalen der Physik und Chemie, 123*, 527–528.

Donders, F.C. (1865). Over de snelheid der gedachte en der wilsbepaling: Voorloopige mededeeling. *Nederlandsch Archief voor Genees- en Natuurkunde, 1*, 518–521.

Donders, F.C. (1868a). Over de snelheid van psychische processen. *Onderzoekingen gedaan in het Physiologisch Laboratorium der Utrechtsche Hoogeschool* (1968–1869), *2*, 92–120.

Donders, F.C. (1868b). Die Schnelligkeit psychischer Processe: Erster Artikel. *Archiv für Anatomie, Physiologie und wissenschaftliche Medicin*, 657–681.

Donders, F.C. (1868c). La vitesse des actes psychiques. *Archives néerlandaises des sciences exactes et naturelles, 3*, 296–317.

Donders, F.C. (1870). De physiologie der spraakklanken. In het bijzonder van die der Nederlandse taal. Utrecht: C. van der Post.

Donovan, J. (1891). The festival origin of human speech. *Mind, 16*, 498–506.

Doran, E.W. (1907). A study of vocabularies. *Pedagogical Seminary, 14*, 401–438.

Draaisma, D. (2002). *The age of precision: F.C. Donders and the measurement of mind*. Nijmegen: F.C. Donders Centre for Cognitive Neuroimaging.

Dudley, H. & Tarnoczy, T.H. (1950). The speaking machine of Wolfgang von Kempelen. *Journal of the Acoustical Society of America, 22*, 151–166.

Ebbinghaus, H. (1885). *Über das Gedächtniss. Untersuchungen zur experimentellen Psychologie*. Leipzig: Duncker & Humber.

Ebbinghaus, H. (1897). *Grundzüge der Psychologie*. Leipzig: Veith & Comp.

Egan, J.P. (1948). Articulation testing methods. *Laryngoscope, 58*, 955–991.

Egger, É. (1879). *Observations et réflexions sur le développement de l'intelligence et du langage chez les enfants*. Paris: Picard.

Eimas, P.D., Siqueland, E.R., Jusczyk, P., & Vigorito, J. (1971). Speech perception in infants. *Science, 171*, 303–306.

Eisenson, J. (1938). *The psychology of speech*. New York: Crofts.

Eldridge, R.C. (1911). *Six thousand common English words, their comparativ frequency and what can be done with them*. Niagara Falls: Clement Press.

Eling, P. (Ed.) (1994). *Reader in the history of aphasia*. Amsterdam: John Benjamins.

Eling, P. (2006). Meynert on Wernicke's aphasia. *Cortex, 42*, 811–816.

Eling, P., Draaisma, D., & Conradi, M. (2011). Gall's Visit to The Netherlands. *Journal of the History of the Neurosciences*, 20, 135–150.

Elffers, E. (1996). Van Ginneken als psychosyntheticus. In: A. Foolen & J. Noordegraaf (Eds.), *De taal is kennis van de ziel. Opstellen over Jac. van Ginneken (1877–1945)*. Münster: Nodus, pp. 51–80.

Ellenberger, H. (1970). *The Discovery of the Unconscious: The History and Evolution of Dynamic Psychiatry*. New York: Basic Books.

Engels, H. (2006). *Emil Kraepelin's Traumsprache 1908–1926*. Wageningen: Ponsen & Looyen.

English, G. (1916). On the psychological response to unknown proper names. *American Journal of Psychology, 27*, 430–434.

Epstein, I. (1900). *La pensée et la polyglossie*. Lausanne: Librairie Payot.

Erdmann, B. (1900). Zur Erläuterung unserer tachistoskopischen Versuche. *Zeitschrift für Psychologie, 22*, 241–267.

Erdmann, B. & Dodge, R. (1898). *Psychologische Untersuchungen über das Lesen. Auf experimenteller Grundlage*. Halle: Niemeyer.

Erdmann, B. & Dodge, R. (1900). Zur Erläuterung unserer tachistoskopischen Versuche. *Zeitschrift für Psychologie und Physiologie der Sinnesorgane*, 22, 241–267.

Ernst, E. (2001). Commentary: The Third Reich – German physicians between resistance and participation. *International Journal of Epidemiology, 30*, 37–42.

Ervin, S.M. & Osgood, C.E. (1954). Second language learning and bilingualism. *Journal of Personality and Social Psychology, 58*, 139–145.

Eschbach, A. (Ed.) (1984). *Bühler Studien.* 2 Vols. Frankfurt: Suhrkamp.

Eschbach, A. (Ed.) (1988). *Karl Bühler's theory of language.* Amsterdam: John Benjamins.

Eschbach, A. & Halawa, M. (2005). *Karl Bühler.* Special issue of *Kodikas/Code. Ars Semiotica, 28*, issues 1–2.

Eschericht, D.F. (1853). Wie lernen Kinder sprechen? Lecture for the *Wissenschaftliche Verein zu Berlin.* Berlin.

Esper, E.A. (1918). A contribution to the experimental study of analogy. *Psychological Review, 25*, 468–487.

Esper, E.A. (1925). A technique for the experimental investigation of associative interference in artificial linguistic material. *Language Monographs* No. 1.

Esper, E.A. (1933). Studies in linguistic behavior. I. Characteristics of unstable verbal reactions. *Journal of General Psychology, 8*, 346–381.

Esper, E.A. (1968). *Mentalism and objectivism in linguistics.* New York: American Elsevier Publishing Cie.

Estoup, J.-B. (1902). *Gammes sténographiques. Recueil de textes choisis pour l'acquisition méthodique de la vitesse, précédé d'une introduction par J.-B. Estoup.* Paris: Institut Sténographique.

Exner, S. (1881). *Untersuchungen über die Localisation der Functionen in der Grosshirnrinde des Menschen.* Wien: Baumüller.

Exner, S. (1894). *Entwurf zu einer physiologische Erklärung der psychischen Erscheinungen.* Vol. I. Leipzig and Vienna: Franz Deuticke.

Fairbanks, H. (1944). The quantitative differentiation of samples of spoken language. In: W. Johnson (Ed.) Studies in language behavior. *Psychological Monographs, 56*, Whole No. 255, pp. 19–38.

Fanselow, G. (1985). What is a possible complex word? In: J. Toman (Ed.), *Studies in German Grammar.* Dordrecht: Foris, pp. 289–318.

Farrer, F.M. (1870). Philology and Darwinism. *Nature, 1, 527–529.*

Fauconnier, G. & Turner, M. (2003). *The way we think.* New York: Basic Books.

Fay, D. & Cutler, A. (1977). Malapropisms and the structure of the mental lexicon. *Linguistic Inquiry, 8*, 505–520.

Feifel, H. & Lorge, I. (1950). Qualitative differences in the vocabulary responses of children. *Journal of Experimental Psychology, 41*, 1–18.

Ferguson, C.A. & Slobin, D.I. (Eds.) (1973), *Studies of child language development.* New York: Holt, Rinehart, & Winston.

Ferrier, D. (1876). *The functions of the brain.* London: Smith, Elder & Co.

Feyeux, J.-A. (1932). *L'acquisition du langage et ses retards.* Paris: Éditions Médicales N. Maloine.

Fick, A. (1870–1871). *Vergleichendes Wörterbuch der Indogermanischen Sprachen.* 2 Vols. Vandenhoeck & Ruprecht, Göttingen.

Fischer, S. (1922). Über das Entstehen und Verstehen von Namen, mit einem Beitrage zur Lehre von den transkortikalen Aphasien. *Archiv für die gesamte Psychologie, 42*, 335–368; *43*, 32–63.

Fishman, J.A. (1966). A systematization of the Whorfian hypothesis. In: A.G. Smith (Ed.). *Communication and culture.* New York: Holt, Rinehart & Winston, pp. 505–516.

Flesch, R. (1946). *The art of plain talk.* New York: Harper & Brothers.

Flesch, R. (1955). *Why Johnnie can't read. And what you can do about it.* New York: Harper & Brothers.

Fletcher, H. (1929). *Speech and hearing.* New York: Van Nostrand Company.

Flourens P. (1842). *Recherches expérimentales sur les propriétés et les fonctions du système nerveux dans les animaux vertébrés.* Baillère: Paris.

Fodor, J.A. (1964). Review of *Symbol formation* by H. Werner & B. Kaplan. *Language, 40*, 566–578.

Fodor, J.A. (1965). Could meaning be an r_m? *Journal of Verbal Learning and Verbal Behavior, 4*, 73–81.

Foerster, O. & Penfield, W. (1930a). The structural basis of traumatic epilepsy and results of radical operation. *Brain, 53*, 99–119.

Foerster, O. & Penfield, W. (1930b). Der Narbenzug am und im Gehirn bei traumatischer Epilepsie in seiner Bedeutung für das Zustandenkommen der Anfälle und für die theurapeutische Behandlung derselben. *Zeitschrift für die gesammte Neurologie und Psychiatrie, 125*, 475–672.

Foisil, M. (Ed.) (1989). *Journal de Jean Héroard.* (Vol. 1, 1601–1608; Vol. 2, 1609–1628). Publication du Centre de Recherche sur la Civilisation de l'Europe moderne. Paris: Fayard.

Foley, J.P. & Cofer, C.N. (1943). Mediated generalization and the interpretation of verbal behavior: II. Experimental study of certain homophone and synonym gradients. *Journal of Experimental Psychology, 33*, 168–178.

Foley, J.P. & MacMillan, Z.L. (1943). Mediated generalization and the interpretation of verbal behavior. V. Free association as related to differences in professional training. *Journal of Experimental Psychology, 33*, 299–310.

Foley, J.P., & Matthews, M.A. (1943). Mediated generalization and the interpretation of verbal behavior. IV. Experimental study of the development of inter-linguistic synonym gradients. *Journal of Experimental Psychology, 33*, 188–200.

Fouché, P. (1952). *Phonétique historique du français. Introduction.* Paris: Klincksieck.

Fournié, E. (1877). *Essai de psychologie: La bête et l'homme.* Paris: Didier.

Fox, C.W. (1935). An experimental study of naming. *American Journal of Psychology, 47*, 545–579.

Frege, G. (1892). Ueber Sinn und Bedeutung. *Zeitschrift für Philosophie und philosophische Kritik, 100*, 25–50.

Freud, S. (1891). *Zur Auffassung der Aphasien. Eine kritische Studie.* Leipzig und Wien: Franz Deuticke. (1953 English translation: On aphasia. A critical study. New York: International University Press).

Freud, S. (1901, 1954). *Zur Psychologie des Alltagslebens.* Frankfurt aM: Gustav Fischer.

Friedeberger, M. (1896). *Zur Psychologie der Sprache mit besonderer Rücksicht auf die Zungensprache der Taubstummen.* Bern: Buchdruckerei Steiger & Cie.

Fries, C.C. (1952). *The structure of English.* New York: Harcourt Brace.

Fries, C.C. & Traver, A.A. (1950). *English word lists.* Ann Arbor, MI: George Wahr Publishing Co.

Frijda, N. & De Groot, A.D. (Ed.) (1991). *Otto Selz: His contribution to psychology.* The Hague: Mouton.

Frijda, N. & Van de Geer, J. (1961). Codability and recognition: An experiment with facial expressions. *Acta Psychologica, 18*, 360–365.

Fromkin, V. (Ed.) (1973). *Speech errors as linguistic evidence.* The Hague: Mouton.

Fröschels, E. (1918). *Kindersprache und Aphasie. Abhandlungen aus der Neurologie, Psychiatrie, Psychologie 3.* Berlin: Karger.

Fröschels, E. (1925). *Psychologie der Sprache.* Leipzig and Vienna: Franz Deuticke.

Froeschels, E. (1946). A peculiar intermediary state between waking and sleep. *Journal of Clinical Psychopathology and Psychotherapy, 7*, 825–833.

Froeschels, E. (Ed.) (1948). *Twentieth century speech and voice correction.* New York: Philosophical Library.

Froeschels, E. (1955). Grammar, a basic function of language-speech. *American Journal of Psychotherapy, 9*, 43–53.

Frontali, G. (1943–44). Lo sviluppo del linguaggio articolato nel bambino. *Vox Romanica, 7*, 214–243.

Fry, D. (1958). Experiments in the perception of stress. *Language and Speech, 1*, 126–152.

Fry, D. (1969). The linguistic evidence of speech errors. *BRNO Studies of English, 8*, 69–74.

Gale, M.C. & Gale, H. (1902). The vocabularies of three children in one family at two and three years of age. *Pedagogical Seminary, 9*, 422–435.

Gall, F.J. (1798). Des Herren Dr. F. J. Gall Schreiben über seinen bereits geendigten Prodromus über die Verrichtungen des Gehirns der Menschen und der Thiere, an Herrn Jos. Fr. von Retzer. *Der neue*

Teutsche Mercur [Wielands]. Translation by C. Heeschen in P. Eling (Ed.) (1994). *Reader in the history of aphasia*. Amsterdam: John Benjamins, pp. 17–27.

Gall, F.J. (1818, 1819). *Anatomie et physiologie du système nerveux en général et du cerveau en particulier; avec des observations sur la possibilité de reconnaître plusieurs dispositions intellectuelles et morales de l'homme et des animaux par la configuration de leur têtes*. Vols. III, IV. Paris: F. Schoell.

Gall, F.J. & Spurzheim, J.G. (1810, 1812). *Anatomie et physiologie du système nerveux en général et du cerveau en particulier, avec des observations sur la possibilité de reconnaître plusieurs dispositions intellectuelles et morales de l'homme et des animaux, par la configuration de leur tête*. Vols. I, II. Paris: F. Schoell.

Galton, F. (1879). Psychometric experiments. *Brain, 2*, 149–162.

Galton, F. (1883). *Inquiries into human faculty and its development*. London: Macmillan.

Garbini, A. (1892). *Evoluzione della voce nella infanzia. Memoria del M. E. Dott*. Verona: Stabilimento Tipo-Lit.

Gardiner, A.H. (1922). The definition of the word and the sentence. *British Journal of Psychology. General Section, 12*, 352–361.

Gardiner, A. (1932). *The theory of language and speech*. Oxford: Clarendon Press.

Gardner, H. (1993). *Multiple intelligences: The theory in practice*. New York: Basic Books.

Garrett, H.E. & Lemon, V.W. (1924). An analysis of several well-known tests. *Journal of Applied Psychology, 8*, 424–438.

Garrett, M. (1980). Levels of processing in sentence production. In B. Butterworth (Ed.), *Language Production* Vol. 1: *Speech and Talk*. London: Academic Press.

Geiger, L. (1869). *Der Ursprung der Sprache*. Stuttgart: Gotta'schen Buchhandel.

Geiger, L. (1868–72). *Ursprung und Entwickelung der menschlichen Sprache und Vernunft*. 2 Vols. Stuttgart: Gotta'schen Buchhandel.

Gelb, A. & Goldstein, K. (1920). Zur Psychologie des optischen Wahrnehmungs- und Erkennungsvorganges. In: A. Gelb & K. Goldstein, *Psychologische Analysen hirnpathologischer Fälle. Leipzig:* Johann Ambrosius Barth, pp. 1–142.

Geuter, U. (1994). The whole and the community: Scientific and practical reasoning in the holistic psychology of Felix Krueger. In: M. Renneberg & M. Walker, *Science, technology, and National Socialism*. Cambridge: Cambridge University Press. pp. 197–223.

Gheorgov, J. (1905). Die erste Anfänge des sprachlichen Ausdrucks für das Selbstbewußtsein bei Kindern. *Archiv für die gesammte Psychologie, 5*, 329–404.

Gheorgov, J. (1908). *Ein Beitrag zur grammatischen Entwicklung der Kindersprache*. Leipzig: Engelmann.

Gheorgov, J. (1910). *The vocabulary in child language*. [in Bulgarian]. Sofia: Annual of the University of Sofia.

Gillette, J.M. (1929). Extent of personal vocabularies and cultural control. *Scientific Monthly, 29*, 451–457.

Glässer, E. (1939). *Einführung in die rassenkundliche Sprachforschung*. Heidelberg: Carl Winter's Universitätsbuchhandlung.

Glozman, J. (1996). A Brief History of Russian Aphasiology. *Neuropsychology Review, 6*, 1–10.

Godel, R. (1966). De la théorie du signe aux termes du système. *Cahier F. de Saussure, 22*, 53–68.

Goldiamond, I. & Hawkins, W.F. (1952). Vexierversuch: The log relationship between word-frequency and recognition obtained in the absence of stimulus words. *Journal of Experimental Psychology, 56*, 457–463.

Goldin-Meadow, S., Wing Chee So, Özyürek, A., & Mylander, C. (2008). The natural order of events: How speakers of different languages represent events nonverbally. *Proceedings of the National Academy of Sciences, 107*, 9163–9168.

Goldman-Eisler, F. (1952). Individual differences between interviewers and their effect on interviewees' conversational behaviour. *Journal of Mental Science, 98*, 660–671.

Goldman-Eisler, F. (1954a). A study of individual differences and the interaction in the behaviour of some aspects of language in inteviews. *Journal of Mental Science, 100*, 177–197.

Goldman-Eisler, F. (1954b). On the variability of the speed of talking and on its relation to the length of utterances. *British Journal of Psychology, 45*, 94–107.

Goldman-Eisler, F. (1955). Speech-breathing activity – A measure of tension and affect during interviews. *British Journal of Psychology, 46*, 53–63.

Goldman-Eisler, F. (1956). A contribution to the objective measurement of the cathartic process – I. *Journal of Mental Science, 102*, 78–95.

Goldman-Eisler, F. (1957). Speech production and language statistics. *Nature, 180*, 1497.

Goldman-Eisler, F. (1958). Speech production and the predictability of words in context. *Quarterly Journal of Experimental Psychology, 10*, 96–106.

Goldman-Eisler, F. (1968). *Psycholinguistics.* London: Academic Press.

Goldschneider, A. & Müller, R.F. (1893). Zur Physiologie und Pathologie des Lesens. *Zeitschrift für Klinische Medizin, 26*, 131–167.

Goldstein, K. (1905). Zur Frage der amnestische Aphasie und ihrer Abgrenzung gegenüber der transcorticalen und glossopsychischen Aphasie. *Archiv fur Psychiatrie und Neurologie, 41*, 1–40.

Goldstein, K. (1913). *Über Rassenhygiene.* Berlin: Springer.

Goldstein, K. (1927). Über Aphasie. *Schweizer Archiv für Neurologie und Psychiatrie, 19*, Heft 6, 3–68.

Goldstein, K. (1933). L'Analyse de l'aphasie. In: H. Delacroix (Ed.). *Psychologie du langage.* Paris: Librairie Félix Alcan, pp. 430–496.

Goldstein, K. (1934). *Der Aufbau des Organismus. Einführung in die Biologie unter besonderer Berücksichtigung der Erfahrungen am kranken Menschen.* The Hague: Martinus Nijhoff.

Goldstein, K. (1947). *Human nature in the light of psychopathology.* Cambridge, MA: Harvard University Press.

Goldstein, K. (1948). *Language and language disturbances. Aphasic symptom complexes and their significance for medicine and theory of language.* New York: Grune & Stratton.

Goldstein, K. (1954). Bemerkungen zum Problem "Sprechen und Denken" auf Grund hirnpathologischer Erfahrungen. In: G. Révèsz (Ed.). *Thinking and speaking: A symposium.* Amsterdam: North-Holland Publishing Company, pp. 175–196.

Goltz, B. (1847). *Buch der Kindheit.* Frankfurt: Heinrich Zimmer.

Gomperz, H. (1908). *Weltanschauungslehre.* Vol. II/1: *Noologie.* Jena/Leipzig: Diederichs.

Goodenough, F.L. (1938). The use of pronouns by young children: a note on the development of self-awareness. *Journal of Genetic Psychology, 52*, 333–346.

Goodglass, H. & Hunt, J. (1958). Grammatical complexity and aphasic speech. *Word, 14*, 197–207.

Goodglass, H. & Quadfasel, F.A. (1954). Language laterality in left-handed aphasics. *Brain, 77*, 521–548.

Graf-Nold, A. (1991). Stern versus Freud: The prehistory and consequences of the controversy over child psychoanalysis. In: W. Deutsch (Ed.). *Über die verborgene Aktualität von William Stern.* Frankfurt a.M.: Peter Lang, pp. 50–91.

Grammont, M. (1895). *La dissimilation consonantique dans les langues indo-européennes et dans les langues romanes.* Dijon: Darantière.

Grammont, M. (1902). Observations sur le langage des enfants. In: D. Barbelenet, G. Dottin, R. Gauthiot, M. Grammont, A. Laronde, M. Niedermann, & J. Vendryes (Eds.). *Mélanges linguistiques. Offerts à M. Antoine Meillet par ses élèves.* Paris: Klincksieck, pp. 61–82.

Granich, L. (1947). *Aphasia: A guide to retraining.* New York: Grune & Stratton.

von Grashey, H. (1885). Ueber Aphasie und ihre Beziehung zur Wahrnehmung. *Archiv fur Psychiatrie und Nervenkrankheiten, 16*, 654–688. (English translation by R.de Bleser in: *Cognitive Neuropsychology* (1985), *6*, 515–546.).

Graumann, C.F. (Ed.) (1985). *Psychologie im Nationalsozialismus.* Berlin: Springer.

Gray, C.T. (1917). Types of reading ability as exhibited through tests and laboratory experiments. In: *Supplementary Educational Monographs, 1*, Whole No 5. Chicago: Chicago University Press, pp. 83–90.

Greenspoon, J. (1955). The reinforcing effect of two spoken sounds on the frequency of two responses. *American Journal of Psychology, 68*, 409–416.

Grégoire, A. (1937–1947). *L'apprentissage du langage.* 2 Vols. Paris: Librairie Félix Alcan.

Grigsby, O.J. (1932). An experimental study of the development of concepts of relationship in pre-school children as evidenced by their expressive ability. *Journal of Experimental Education, 12*, 144–162.

Grimm, J. (1822). *Deutsche Grammatik.* 4 Vols. 2nd edition. Göttingen: Dieterich.

Grimm, J. (1851). *Über den ursprung der sprache.* Veröffentlichungen der Preußischen Akademie. Republished (1858). Wiesbaden: Insel-Verlag.

Grings, W. (1942). The verbal summator technique and verbal mental states. *Journal of Abnormal and Social Psychology, 37*, 529–545.

Grossart, F. (1921). Das tachistoskopische Verlesen unter besonderer Berücksichtigung des Einflusses von Gefühlen und der Frage des objectiven und subjektiven Typus. *Archiv für die gesamte Psychologie, 41*, 121–200.

Guillaume, G. (1933). Esquisse d'une théorie psychologique de l'aspect. *Journal de Psychologie, 30*, 355–372.

Guillaume, P. (1925). *L'imitation chez l'enfant.* Paris: Librairie Félix Alcan.

Guillaume, P. (1927a). Les débuts de la phrase dans le langage de l'enfant. *Journal de Psychologie, 24*, 3–25.

Guillaume, P. (1927b). Le développement des éléments formels dans le langage de l'enfant. *Journal de Psychologie, 24*, 203–229.

Guillaume, P. & Meyerson, I. (Eds.) (1950). *Grammaire et psychologie.* Numéro spécial du *Journal de Psychologie.* Paris: Presses Universitaires de France.

Gutzmann, H. (1894). *Des Kindes Sprache und Sprachfehler. Gesundheitslehre der Sprache für Eltern, Erzieher und Ärzte.* Leipzig: Weber.

Gutzmann, H. (1897). Die Sprache des Kindes und der Naturvölker. In: *Dritter Internationaler Congress für Psychologie in München. 1896.* München: Lehmann, pp. 434–435.

Gutzmann, H. (1908). Über Hören und Verstehen. *Zeitschrift für angewandte Psychologie und psychologische Sammelforschung, 1*, 483–503.

Gutzmann, H. (1909). *Physiologie der Stimme und Sprache.* Braunschweig: Friedrich Vieweg.

Guyot, C. & Guyot, R.T. (1824). *Beschrijving van het Instituut voor Doof-Stommen te Groningen. Ten geleide van eene systematisch gerangschikte lijst der werken en geschriften over doof-stommen en het onderwijs aan doof-stommen.* Groningen: J. Oomkens.

Guyot, C. & Guyot, R.T. (1842). *Liste Littéraire Philosophe ou Catalogue d'étude de ce qui a été publié jusqu'a nos jours sur les sourds-muets: l'oreille, l'ouïe, sur la voix, le langage, la mimique, les aveugles etc, etc.* Groningen: J. Oomkens.

Gvozdev, A.N. (1948). *Usvoejenie rebenkom zvukovoy storony russkogo jazyka* [Acquisition by the child of the sound structure of the Russian language]. Moscow: Akademija Pedagogičeskix Nauk RSFSR.

Gvozdev, A.N. (1949). *Formirovanije u rebenka grammatičeskogo stroja russkogo jazyka* [The child's formation of the grammatical structure of the Russian language]. Moscow: Akademija Pedagogičeskix Nauk RSFSR <http://www.universite.ru/material/works/filolog/gvoz/gvozdev.html>.

Gvozdev, A.N. (1961). *Voprosy izuchenya detskoy rechi.* [Questions arising from the study of children's speech]. Moskow: Akademija Pedagogičeskix Nauk RSFSR.

Haarmann, H. & Kolk, H. (1991). A computer model of the temporal course of agrammatic sentence understanding: The effects of variation in severity and sentence complexity. *Cognitive Science, 15*, 49–87.

Haeckel, E. (1874). *Anthropogenie oder Entwicklungsgeschichte des Menschen.* Leipzig: Wilhelm Engelmann.

Hagoort, P., & Van Berkum, J.J.A. (2007). Beyond the sentence given. *Philosophical Transactions of the Royal Society.* Series B: Biological Sciences, *362*, 801–811.

Hagoort, P. & Levelt, W.J.M. (2009). The speaking brain. *Science*, *326*, 372–373.

Hale, H. (1886). The origin of language and the antiquity of speaking man. *Proceedings of the AAAS*, *35*, 1–47.

Hall, G.S. (1914). *Wilhelm Wundt. Der Begründer der modernen Psychologie.* Leipzig: Felix Meiner Verlag.

Hall, K.R.L. (1951). The fitness of signs to words. *British Journal of Psychology*, *42*, 21–33.

Hall, K.R.L. & Oldfield, R.C. (1950). An experimental study on the fitness of signs to words. *Quarterly Journal of Experimental Psychology*, *2*, 60–70.

Hall, W.S. (1896–1897). The first five hundred days of a child's life V. *Child Study Monthly*, *2*, 586–608.

Hansen, F.C.C. & Lehmann, A. (1895). Ueber unwillkürliches Flüstern. *Philosophische Studien*, *11*, 471–530.

Harris, L.J. (1999). Early theory and research on hemispheric specialization. *Schizophrenia Bulletin*, *25*, 11–39.

Harris, R. (2001). *Saussure and his interpreters.* Edinburgh: Edinburgh University Press.

Harris, Z.S. (1951). *Methods in structural linguistics.* Chicago: Chicago University Press.

Harris, Z.S. (1952). Discourse analysis. *Language*, *28*, 1–30.

Harris, Z.S. (1957). Co-occurrence and transformation in linguistic structure. *Language*, *33*, 283–340.

Hayakawa, S.I. (1939, 1941). *Language in action.* New York: Harcourt, Brace and Company.

Head, H. (1915). Hughlings Jackson on aphasia and kindred affections of speech. *Brain*, *38*, 1–27.

Head, H. (1926). *Aphasia and kindred disorders of speech.* 2 Vols. London: Cambridge University Press.

Hebb, D.O. (1949). *The organization of behavior. A neuropsychological theory.* New York: John Wiley.

Hécaen, H. & Angelergues, R. (1965). *Pathologie du langage; l'aphasie.* Paris: Larousse.

Hécaen, H., Penfield, W., Bertrand, C., & Malmo, R. (1956). The syndrome of apractognosia due to lesion of the minor cerebral hemisphere. *Archives of Neurology and Psychiatry*, *75*, 400–434.

Hei, G. (1993). . . .wirkliche Möglichkeiten für eine nationalsozialistische Philosophie? Die Reorganisation der Philosophie (Psychologie und Pädagogik) in Wien 1938 bis 1940. In: K.B.R. Fischer & F.W. Wimmer (Eds.), *Die geistige Anschluß. Philosophie und Politik an der Universität Wien 1930–1950.* Vienna: WUV-Universitätsverlag, pp. 130–170.

Heilbronner, K. (1906). Ueber Agrammatismus und die Störung der inneren Sprache. *Archiv für Psychiatrie und Nervenkrankheiten*, *41*, 653–683.

Heller, R. (1911). *Untersuchungen über die Gesamtform und ihre Bedeutung für das tachistoskopische Lesen im indirekten Sehen.* Dissertation Universität Zürich.

von Helmholtz, H. (1850). Messungen über den zeitlichen Verlauf der Zuckung animalischer Muskeln und die Fortpflanzungsgeschwindigkeit der Reizung in den Nerven. *Archiv für Anatomie, Physiologie und wissenschaftliche Medicin*, *17*, 176–364.

Henderson, E.N. (1903–1904). A study of memory for connected trains of thought. *Psychological Monographs*, *5*, Whole No. 6.

Henle, M. (1978). One man against the Nazis – Wolfgang Köhler. *American Psychologist*, *33*, 939–944.

Henning, H. (1917). Versuche über die Residuen. *Zeitschrift für Psychologie*, *78*, 198–243.

Henschen, S.E. (1918). Über die Hörsphäre. *Journal für Psychologie und Neurologie*, *22*, 319–474.

Henschen, S.E. (1920–1922). *Klinische und anatomische Beiträge zur Pathologie des Gehirns.* Vols. V–VII. Stockholm: Nordiska Bokhandeln.

Herbart, J.F. (1816). *Lehrbuch zur Psychologie*, Königsberg.

Herbart, J.F. (1824). *Psychologie als Wissenschaft, neu gegründet auf Erfahrung, Metaphysik und Mathematik*. 2 Vols. Königsberg: Unzer.

Herder, J.G. (1772). *Abhandlung über die Ursprung der Sprache*. Berlin: Voss. New edition by Th. Matthias (1901). Leipzig: Friedrich Brandstetter.

Herder, J.G. (1784). *Ideen zur Philosophie der Geschichte der Menschheit*. Stuttgart: Gotta'sche Buchhandlung. (New edition: H. Stolpe (1965), 2 Vols. Berlin, Weimar: Aufbau.)

Hermann-Cziner, A. (1923). Experimentelle Untersuchungen zum Problem des Verstehens. *Zeitschrift für Psychologie, 92*, 88–109.

Héroard, J. (1868). *Journal de Jean Héroard sur l'enfance et la jeunesse de Louis XIII (1601–1628)*. Soulié, E. & De Barthélemy, E. (Eds.). Paris: Firmin Didot Frères, Fils & Cie. <http://www.archive.org/details/journaldejeanhr00hrgoog> accessed 16 Aug 2012.

Herzog, H. (1933). Stimme und Persönlichkeit. *Zeitschrift für Psychologie, 190*, 300–379.

Heynick, F. (1993). *Language and its disturbances in dreams. The pioneering work of Freud and Kraepelin updated*. New York: John Wiley.

Heyse, K. (1856). *System der Sprachwissenschaft*. Berlin: Dümmler.

Hilmer, H. (1914). *Schallnachahmung, Wortschöpfung und Bedeutungswandel*. Halle: Niemeyer.

Hirst, W. (Ed.) (1988). *The making of cognitive science. Essays in honor of George A. Miller*. Cambridge: Cambridge University Press.

Hocart, A.M. (1912). The "psychological interpretation of language." *British Journal of Psychology, 5*, 267–80.

Hoijer, H. (1953). A relation of language to culture. In: A.L. Kroeber (Ed.), *Anthropology today*. Chicago: Chicago University Press.

Hoijer, H. (Ed.) (1954). Language in culture. *The American Anthropologist, 56*, Memoir 79.

Hofer, V. (2001). Konrad Lorenz als Schüler von Karl Bühler. Diskussion der neu entdeckten Quellen zu den persönlichen und inhaltlichen Positionen zwischen Karl Bühler, Konrad Lorenz und Egon Brunswik. *Zeitgeschichte, 28*, 135–159.

Hoffmann, J. (1926). Über die Stand der Lesepsychologie. *Archiv für Psychologie, 57*, 403–444.

Hofstätter, P.R. (1955). Über Ähnlichkeit. *Psyche, 9*, 54–80.

Holden, E.S. (1877). On the vocabularies of children under two years of age. *Transactions of the American Philological Association, 8*, 58–68.

Hollingworth, H.L. (1935). The conditions of verbal configuration. *Journal of Experimental Psychology, 18*, 299–306.

Holmes, U.T. (1927). The phonology of an English-speaking child. *American Speech, 2*, 219–225.

Holt, E.B. (1931). *Animal drive*. Vol. I. New York: Holt.

Hovland, C.I. (1951). Human learning and retention. In: S. S. Stevens (Ed.), *Handbook of experimental psychology*. New York: Wiley, pp. 613–689.

Howells, T. H. & Schoolland, J.B. (1934). An experimental study of speech perception. *Journal of General Psychology, 11*, 337–347.

Howes, D. (1954). On the interpretation of word frequency as a variable affecting speed of recognition. *Journal of Experimental Psychology, 48*, 106–112.

Howes, D. (1957). On the relation between the intelligibility and frequency of occurrence of English words. *Journal of the Acoustical Society of America, 29*, 296–305.

Howes, D. & Osgood, C.E. (1954). On the combination of associative probabilities in linguistic contexts. *American Journal of Psychology, 67*, 241–258.

Howes, D.H. & Solomon, R.L. (1951). Visual duration threshold as a function of word probability. *Journal of Experimental Psychology, 41*, 401–410.

Hoyer, A. & Hoyer, G. (1924). Über die Lallsprache eines Kindes. *Zeitschrift für angewandte Psychologie, 24*, 263–384.

Huey, E.B. (1898). Preliminary experiments in the physiology and psychology of reading. *American Journal of Psychology, 9*, 575–586.

Huey, E.B. (1900). On the psychology and physiology of reading. *American Journal of Psychology, 11*, 283–302.

Huey, E.B. (1908). *The psychology and pedagogy of reading*. New York: Macmillan.

Hughlings Jackson, J. (1874). On the nature of the duality of the brain. *Medical Press and Circular, 1*, 19, 41, 63. Reprinted in Hughlings Jackson (1996).

Hughlings Jackson, J. (1878–1879). On affections of speech from disease of the brain. *Brain, 1*, 304–330. Reprinted in Hughlings Jackson (1996).

Hughlings Jackson, J. (1879–1880). On affections of speech from disease of the brain. *Brain, 2*, 203–222. Reprinted in Hughlings Jackson (1996).

Hughlings Jackson, J. (1915). Papers on aphasia and kindred affections of speech. Edited by Henry Head. *Brain, 38*, 1–190.

Hughlings Jackson, J. (1996). Facsimile edition of *Selected writings* (1932), J. Taylor, G. Holmes & F.M.R. Walshe (Eds.). Nijmegen: Arts & Boeve.

von Humboldt, W. (1836). *Über die Kawisprache*. Berlin: Druckerei der Koniglichen Akademie der Wissenschaften/F. Dümmler.

von Humboldt, W. (1963). *Schriften zur Sprachphilosophie*. Darmstadt: Wissenschaftliche Buchgesellschaft.

Humphrey, M.E. & Zangwill, O. (1952). Effects of a right-sided occipito-parietal brain injury in a left-handed man. *Brain, 75*, 312–324.

Humphreys, M.W. (1880). A contribution to infantile linguistic. *Transactions of the American Philosophical Association, 11*, 2–17.

Hunt, J. (1869). On the localization of the functions of the brain, with special reference to the faculty of language. *Anthropological Review, 7*, 201–214.

Hunter, W.S. (1924). The symbolic process. *Psychological Review, 31*, 478–497.

Huth, A. (1919). Die Nebensätze in der Kindersprache. *Zeitschrift für pädagogische Psychologie, 20*, 163–183.

Hutton, C.M. (1999). *Linguistics and the Third Reich. Mother-tongue fascism, race and the science of language*. London: Routledge.

Ingram, D. (1981). *Procedures for the phonological analysis of children's language*. Baltimore, MD: University Park Press.

Ingram, D. & Le Normand, M-T. (1996). A diary study on the acquisition of Middle French: A preliminary report of the early language acquisition of Louis XIII. *Proceedings of the 20th annual Boston University conference on language development*. Boston, MA: Cascadilla Press, pp. 352–363.

Ipsen, G. (1933). *Blut und Boden*. Neumünster: Wachholtz.

Irwin, O.C. (1941a). Research on speech sounds for the first six months of life. *Psychological Bulletin, 38*, 277–285.

Irwin, O.C. (1941b). The profile as a visual device for indicating central tendencies in speech data. *Child Development, 12*, 111–120.

Irwin, O.C. (1942). The developmental status of speech sounds of ten feeble-minded children. *Child Development, 13*, 29–39.

Irwin, O.C. (1947). Development of speech during infance: Curve of phonemic frequencies. *Journal of Experimental Psychology, 37*, 187–193.

Irwin, O.C. (1948). Infant speech: Speech sound development of sibling and only children. *Journal of Experimental Psychology, 38*, 600–602.

Irwin, O.C. & Chen, H.P. (1941). A reliability study of speech sounds observed in the crying of newborn infants. *Child Development, 12*, 351–368.

Irwin, O.C. & Chen, H.P. (1947). Development of speech during infancy. *Journal of Experimental Psychology, 37*, 431–436.

Irwin, O.C. & Curry, T. (1941). Vowel elements in the crying vocalization of infants under ten days of age. *Child Development, 12*, 99–109.

Isserlin, M. (1922). Über Agrammatismus. *Zeitschrift für die gesamte Neurologie und Psychiatrie, 75*, 332–410.

Isserlin, M. (1936). Aphasie. In: O. Bumke & O. Foerster (Eds.), *Handbuch der Neurologie*. Vol. 6. Berlin: Springer, pp. 626–807.

Jacobson, E. (1911). On meaning and understanding. *American Journal of Psychology, 22*, 553–577.

Jaensch, E.R. (1913). Die Natur der menschlichen Sprachlaute. *Zeitschrift für Sinnesphysiologie, 47*, 219–290.

Jaensch, E.R. (1934). Der Gegentypus der deutschen völkischen Bewegung. In: O. Klemm: *Bericht über den XIII. Kongreß der Deutschen Gesellschaft für Psychologie in Leipzig vom 16.–19. Oktober 1933*. Jena: Gustav Fischer.

Jaensch, E.R. (1938). *Der Gegentypus. Psychologisch-anthropologische Grundlagen deutscher Kulturphilosophie ausgehend von dem was wir überwinden wollen*. Leipzig: Ambrosius Barth.

Jakobson, R. (1941). *Kindersprache, Aphasie und allgemeine Lautgesetze*. Frankfurt: Suhrkamp. English translation (1968): *Child language, aphasia and phonological universals*. The Hague: Mouton.

Jakobson, R. (1960). Linguistics and poetics. In: Th. A. Sebeok (Ed.), *Style in language*. Cambridge, MA: MIT Press, pp. 350–377.

Jakobson, R., Fant, C.G., & Halle, M. (1952). *Preliminaries to speech analysis. The distinctive features and their correlates*. Cambridge, MA: Acoustics Laboratory MIT.

Jakobson, R. & Halle, M. (1956). *Fundamentals of language*. The Hague: Mouton.

James, W. (1875). Review of Wundt's Principles of Physiological Psychology. *North American Review, 31*, 195–201. Reprinted in W.G. Bringmann & R.D. Tweney (Eds.) (1980), *Wundt studies. A centennial collection*. Toronto: Hogrefe, pp. 114–120.

James, W. (1890). *The principles of psychology*. 2 Vols. New York: Henry Holt

Jastrow, J. (1891). A study of mental statistics. *New Review, 5*, 559–568.

Jastrow, J. (1894). Community and association of ideas: A statistical study. *Psychological Review, 1*, 152–158.

Jastrow, R. (1896). Community of ideas of men and women. *Psychological Review, 3*, 68–71.

Javal, E. (1878). Essai sur la physiologie de la lecture. *Annales D'Oculistique, 80*, 61–73.

Jenkins, J.J., Russell, W.A., & Suci, G.J. (1958). An atlas of semantic profiles for 360 words. *Journal of American Psychology, 71*, 688–699.

Jespersen, O. (1894). *Progress in language*. London: Swan Sonnenschein & Co.

Jespersen, O. (1916). *Nutidssprog hos börn og voxne*. Copenhagen/Christiana: Gyldendal. (2nd revised edition (1923): *Bornesprog: En bog for forældere*. Copenhagen/Christiana: Gyldendal.)

Jespersen, O. (1922). *Language. Its nature, development and origin*. London: George Allen & Unwin.

Johnson, D.M. & Reynolds, F.A. (1941). A factor analysis of verbal ability. *Psychological Record, 4*, 183–195.

Johnson, S. (1775). *A dictionary of the English language*. London: W. Strahan.

Johnson, W. (1939). *Language and speech hygiene*. Chicago: Institute of General Semantics.

Johnson, W. (Ed.) (1944). Studies in language behavior. *Psychological Monographs, 56*, Whole No. 255.

Jones, W. (1786). *The Third Anniversary Discourse* (delivered 2 February, 1786, by the President, at the Asiatick Society of Bengal). Calcutta: Manuel Cantopher.

Joos, M. (1936). Review of G.K. Zipf, The psychobiology of language. *Language, 12*, 196–210.

Judd, C.H. (1918). Reading: Its nature and development. *Supplementary Educational Monographs, 2*, Whole No 4. Chicago: Chicago University Press.

Judd, C.H., McAllister, C.N. & Steele, W.M. (1905). Introduction to a series of studies of eye movements by means of kinetoscopic photographs. *Psychological Review Monograph Supplements, 7*, Whole No 1, pp. 1–16.

Jung, C.G. (1906a). Die psychologische Diagnose des Tatbestandes. *Juristisch-psychiatrische Grenzfragen, 4*, 1–61.

Jung, C.G. (1906b). Psychoanalyse und Assoziationsexperiment. C.G. Jung (Ed.): *Diagnostische Assoziationsstudien*. Vol. I. Leipzig: Johann Ambrosius Barth, pp. 258–281.

Jung, C.G. (1907). On psychophysical relations of the associative experiment. *Journal of Abnormal Psychology, 1*, 247–255.

Jung, C.G. (1910). The association method. *American Journal of Psychology, 31*, 219–269.

Jung, C.G. & Ricklin, F. (1906). Experimentelle Untersuchungen über Assoziationen Gesunder. In: C.G. Jung (Ed.): *Diagnostische Assoziationsstudien*. Vol. I. Leipzig: Johann

Kaeding, F.W. (Ed.) (1897/98). *Häufigkeitswörterbuch der deutschen Sprache/festgestellt durch einen Arbeitsausschuß der deutschen Stenographiesysteme*. Steglitz bei Berlin: Selbstverlag des Herausgebers.

Kafka, G. (1932). *Bericht über den XII. Kongreß der Deutschen Gesellschaft für Psychologie in Hamburg vom 12.–16. April 1931*. Jena: Gustav Fischer.

Kainz, F. (1930). Die großdeutsche Kultureinheit in der Literatur. In: F.F.G. Kleinwaechter & H. von Paller (Eds.) (1930). *Die Anschlussfrage in ihrer kulturellen, politischen und wirtschaftlichen Bedeutung*. Vienna: Braumüller, pp. 256–286.

Kainz, F. (1936). Zur Psychologie der Sprachfunktionen. *Zeitschrift für Psychologie, 139*, 38–97.

Kainz, F. (1941a). Deutsche Sprachdeutung. In: F. Maurer (Ed.). *Von deutscher Art in Sprache und Dichtung*. Stuttgart: Kohlhammer, pp. 97–151.

Kainz, F. (1941b). *Psychologie der Sprache I. Grundlagen der allgemeinen Sprachpsychologie*. Stuttgart: Ferdinand Enke.

Kainz, F. (1943). *Psychologie der Sprache II. Vergleichend-genetische Sprachpsychologie*. Stuttgart: Ferdinand Enke. (Revised edition 1960.)

Kainz, F. (1944). *Sprachgefühl*. Berlin: Karl Ohm Verlag.

Kainz, F. (1954). *Psychologie der Sprache III. Physiologische Psychologie der Sprachvorgänge*. Stuttgart: Ferdinand Enke.

Kainz, F. (1956). *Psychologie der Sprache IV. Spezielle Sprachpsychologie*. Stuttgart: Ferdinand Enke.

Kainz, F. (1956). *Linguistisches und Sprachpathologisches zum Problem der sprachlichen Fehlleistungen*. Vienna: Rudolf Rohrer.

Kainz, F. (1961). *Die "Sprache" der Tiere*. Stuttgart: Enke.

Kainz, F. (1965a). *Psychologie der Sprache V. Psychologie der Einzelsprachen I*. Stuttgart: Ferdinand Enke.

Kainz, (1965b). Geleitwort. In: K. Bühler (Ed). *Sprachtheorie. Die Darstellungsfunktion der Sprache*. New edition Stuttgart: Fischer, v–xxx.

Kainz, F. (1969). *Psychologie der Sprache V. Psychologie der Einzelsprachen II*. Stuttgart: Ferdinand Enke.

Kainz, F. (1972). *Über die Sprachverführung des Denkens*. Berlin: Duncker & Humblot.

Kantor, J.R. (1922). An analysis of psychological language data. *Psychological Review, 29*, 267–309.

Kantor, J.R. (1936). *An objective psychology of grammar*. Bloomington: Principia Press (reprinted 1952).

Kantor, J.R. (1977). *Psychological linguistics*. Chicago: Principia Press.

Kapustnik, O.P. (1930). [The interrelation between direct conditioned stimuli and their verbal symbols.] Trudy Laboratorii Fiziologii Vysshey Nervnoy Deyatel'nosti Rebyonka pri Leningradskom Pedagogicheskom Institute Gertzema, 2, 16–22. Abstracted in *Psychological Abstracts, 8*, 152.

Kardos, L. (1984). Erinnerungen an Karl Bühler. In: Eschbach, A. (Ed.) (1984). *Bühler Studien*. Vol. I. Frankfurt: Suhrkamp, pp. 31–39.

Karmiloff-Smith, A. (1979). *A functional approach to child language. A study of determiners and reference*. Cambridge: Cambridge University Press.

Karwoski, T.F. & Berthold, F. (1945). Psychological studies in semantics: II. Reliability of free association tests. *Journal of Social Psychology, 22*, 87–102.

Karwoski, T. F., Gramlich, F.W., & Arnott, P. (1944). Psychological studies in semantics: I. Free association reactions to words, drawings, and objects. *Journal of Social Psychology, 20*, 233–247.

Karwoski, T. F. & Schachter, J. (1948). Psychological studies in semantics: III. Reaction times for similarity and difference. *Journal of Social Psychology, 28*, 103–120.

Katz, D. & Katz, R. (1928). *Gespräche mit Kindern. Untersuchungen zur Sozialpsychologie und Pädagogik.* Berlin: Julius Springer.

Katz, J.J. & Fodor, J.A. (1963). The structure of a semantic theory. *Language, 39*, 170–210.

Kaufman, M.H. & Basden, N. (1995). Items relating to Dr. Johann Gaspar Spurzheim (1776–1832) in the Henderson Trust Collection, formerly the Museum Collection of the Phrenological Society of Edinburgh, with an abbreviated iconography. *Journal of Neurolinguistics, 9*, 301–325.

Keller, H. Sullivan, A., Macy, J.A., Shattuck, R., & Herrmann, D. (1903). *The story of my life.* New York: Doubleday, Page and Co.

von Kempelen, W. (1791). *Mechanismus der menschlichen Sprache nebst der Beschreibung einer sprechenden Machine.* Vienna: J.B. Degen. Facsimile edition by E. Brekle & W. Wildgen (Eds.) (1970). Stuttgart-Bad Cannstatt: Friedrich Frommann Verlag.

Kempen, G. (1996). "De zwoele groei van de zinsbouw". De wonderlijke levende grammatica van Jac. van Ginneken uit *De roman van een kleuter* (1917). In: A. Foolen & J. Noordegraaf (Eds.), *De taal is kennis van de ziel. Opstellen over Jac. van Ginneken (1877–1945).* Münster: Nodus, pp. 173–216.

Kent, G.H. & Rosanoff, A.J. (1910). A study of association in insanity. *American Journal of Insanity, 67*, 37–96.

Kenyeres, E. (1926). *A gyermek elsőszavai és a szófajok föllépesé.* Budapest: Kisdednevelés Kiadása.

Kenyeres, E. (1927). Les premiers mots de l'enfant et l'apparation des espèces de mots dans son langage. *Archives de Psychologie, 20*, 191–218.

Kern, A. (1929). Vom inneren Sprechen. Eine experimentelle Untersuchung. *Zeitschrift für Kinderforschung, 35*, 420–447.

Key, C.B. (1925–1926). Recall as a function of perceived relations. *Archives of Psychology, 13*, Whole No. 83.

King-Ellison, P. & Jenkins, J.J. (1954). The durational threshold of visual recognition as a function of word-frequency. *American Journal of Psychology, 67*, 700–703.

Kirkpatrick, E.A. (1891). Number of words in an ordinary vocabulary. *Science, 18*, 107–108.

Klein, W. (1978). Wo ist hier: Präliminarien zu eine Untersuchung der lokalen Deixis. *Linguistische Berichte, 58*, 18–40.

Klein, W. & Perdue, C. (1992). *Utterance structure. Developing grammars again.* Amsterdam: John Benjamins Company.

von Kleist, H. (1805). *Über die allmähliche Verfertigung der Gedanken beim Reden.* H. von Kleist (1878) *Werke und Briefe in vier Bänden.* Vol. 3, Berlin und Weimar: Spemann, pp. 385–460.

Kleist, K. (1914). Aphasie und Geisteskrankheit. *Münchener medizinische Wochenschrift, 61*, 8–12.

Kleist, K. (1916). Die Leitungsaphasie und grammatische Störungen. *Monatschrift für Psychiatrie und Neurologie, 40*, 118–199.

Kleist, K. (1933). Nachtrag zur Aussprache über eugenische Sterilisierung. *Westdeutsche Aerzte-Zeitung, 24*, 115–116.

Kleist, K. (1934). *Gehirnpathologie.* Leipzig: Johann Ambrosius Barth.

Kleist, K. (1936). Bericht über die Gehirnpathologie in ihrer Bedeutung für Neurologie und Psychiatrie. *Zeitschrift für die gesamte Neurologie und Psychiatrie, 158*, 159–193, 334–340.

Kleist, K. (1959). *Sensorische Aphasien und Amusien auf myeloarchitektonischer Grundlage. Drei Vorträge.* Stuttgart: Thieme.

Klemm, O. (Ed.) (1934). *Bericht über den XIII. Kongreß der Deutschen Gesellschaft für Psychologie in Leipzig vom 16.–19. Oktober 1933.* Jena: Gustav Fischer.

Klemm, O. (Ed.) (1935). *Psychologie des Gemeinschaftslebens. Bericht über den XIV. Kongreß der Deutschen Gesellschaft für Psychologie in Tübingen vom 22.–26. Mai 1934.* Jena: Gustav Fischer.

Klemm, O. (1937). *Gefühl und Wille. Bericht über den XV. Kongreß der Deutschen Gesellschaft für Psychologie in Jena vom 5.–8. Juli 1936.* Jena: Fischer.

Klemm, O. (1939). *Charakter und Erziehung. Bericht über den XVI. Kongreß der Deutschen Gesellschaft für Psychologie in Bayreuth vom 2.–4. Juli 1938.* Leipzig: Barth.

Klemm, O., Volkert, H. & Dürkheim-Montmartin, K. (Eds.) (1934). *Ganzheit und Struktur. Festschrift zum 60. Geburtstage Felix Kruegers.* Munich: Beck.

Knobloch, C. (2004). Die deutsche Sprachwissenschaft im Nationalsozialismus. Ein forschungsorientierter Überblick. *Kritische Ausgabe, 2,* 42–47.

Knobloch, C. (2005). *Volkhafte Sprachforschung. Studien zum Umbau der Sprachwissenschaft in Deutschland zwischen 1918 und 1945.* Tübingen: Niemeyer.

Koch, F. (1941). Vorwort. In: F. Maurer (Ed.), *Von deutscher Art in Sprache und Dichtung.* Stuttgart: Hohlhammer.

Koffka, K. (1921). *Die Grundlagen der psychischen Entwicklung. Eine Einführung in die Kinderpsychologie.* Osterwieck am Harz: Zickfeldt.

Köhler, W. (1910). Akustische Untersuchungen. II. *Zeitschrift für Psychologie, 58,* 59–140.

Köhler, W. (1929). *Gestalt Psychology.* New York: Horace Liferight.

Kojevnikov, A.Y. (1874). Aphasia and the central organ of speech. *Moscovsky vrachebny vestnik* (Moscow Medical Bulletin, May 6–June 17), 18–21. (In Russian.)

Kolk, H.H.J. & Heeschen, C. (1990). Adaptation symptoms and impairment symptoms in Broca's aphasia. *Aphasiology, 4,* 221–231.

Kollarits, J. (1914). Observations de psychologie quotidienne. *Archives de Psychologie, 14,* 225–240.

Korte, W. (1923). Über die Gestaltauffassung im indirekten Sehen. *Zeitschrift für Psychologie, 93,* 17–82.

Korzybski, A. (1933). *Science and sanity.* Lancaster, PA: Science Press Printing Company.

Korzybski, A. (1990). *Collected writings. 1920–1950.* Englewood, NJ.: Institute of General Semantics.

Korzybski, A. (1951). The role of language in the perceptual process. In: R.R. Blake & G.V. Ramsey (Eds.), *Perception. An approach to personality.* New York: Ronald Press.

Kotliarevsky, L.I. (1935). Cardio-vascular conditioned reflexes to direct and to verbal stimuli. *Psychological Abstracts, 13,* No. 4046.

Kraepelin, E. (1906). *Über Sprachstörungen im Traume.* Leipzig: Engelmann.

Kretschmer, P. (1923). Sprache. In: A. Gercke & E. Norden (Eds.), *Einleitung in die Altertumswissenschaft.* Vol. 1. Leipzig: Teubner, pp. 1–119.

von Kries, J. & Auerbach, F. (1877). Die Zeitdauer einfachster psychischer Vorgänge. *Archiv für Physiologie,* 297–378.

Kroeber, A. (1916). The speech of a Zuñi child. *American Anthropologist, 18,* 592–34.

<http://www.unileipzig.de/unigeschichte/professorenkatalog/leipzig/Krueger_89/> accessed 13 Aug 2012.

Kuiper, K. (1995). *Smooth talkers: The linguistic performance of auctioneers and sportscasters.* Mahwah, NJ: Lawrence Erlbaum.

Külpe, O. (1912). *Die Realisierung: Ein Beitrag zur Grundlegung der Realwissenschaften.* Vol. I. Leipzig: Hirzel Verlag.

Kussmaul, A. (1877). Die Störungen der Sprache. Versuch einer Pathologie der Sprache. In: H. Ziemssen. *Handbuch der Speziellen Pathologie und Therapie.* Anhang. (Textually unchanged 4th edition by Herman Gutzmann (1910)). Leipzig: F.C.W. Vogel.

Kussmaul, A. (1883). Entgegnung. *Fortschritte des Medicin, 20,* 310–313.

Kutzner, O. (1913). *Methodologisch Beiträge zur experimentellen Untersuchung der Lesevorgänge bei kurzen Expositionszeiten.* Dissertation Universität Zurich.

Kutzner, O. (1916). Kritische und experimentelle Beiträge zur Psychologie des Lesens mit besonderer Berücksichtigung des Problems der Gestaltqualität. *Archiv für die gesamte Psychologie, 35,* 157–251.

Lakoff, G. & Johnson, M. (1980). *Metaphors we live by.* Chicago: Chicago University Press.

Lamare, M. (1893). Des mouvements des yeux pendant la lecture. *Comptes rendus de la Société française d'Ophthalmologie,* 35–64.

Lambert, W.E. (1956). Developmental aspects of second-language acquisition: I. Associative fluency, stimulus provocativeness, and word order influence. *Journal of Social Psychology, 43,* 83–89.

Lambert, W.E., Havelka, J., & Crosby, C. (1958). The influence of language-acquisition contexts on bilingualism. *Journal of Abnormal and Social Psychology,* 56, 239–244.

Lamberti, G. (1995). *Wilhelm Maximilian Wundt (1832–1920).* Bonn: Deutscher Psychologen Verlag.

Lamiell, J.T. (2010). *William Stern (1871–1938): A brief introduction to his life and works.* Lengerich: Pabst Science Publishers.

Landar, H.L., Ervin, S.M., & Horowitz, A.E. (1960). Navaho color categories. *Language, 36,* 368–382.

Lane, H. (1984). *When the mind hears: A history of the Deaf.* New York: Random House.

Lashley, K.S. (1923a). The behavioristic interpretation of consciousness. I. *Psychological Review, 30,* 238–272.

Lashley, K.S. (1923b). The behavioristic interpretation of consciousness. II. *Psychological Review, 30,* 329–353.

Lashley, K.S. (1929). *Brain Mechanisms and Intelligence: A Quantitative Study of Injuries to the Brain.* Chicago: Chicago University Press.

Lashley, K.S. (1951). The problem of serial order in behavior. In: L.A. Jeffress (Ed.), *Cerebral mechanisms in behavior.* New York: Wiley, pp. 112–136.

Latif, I. (1934). The physiological basis of linguistic development and of the ontogeny of meaning, Part I, II, III. *Psychological Review, 41,* 55–85, 153–176, 246–264.

Laurie, S.S. (1890). *Lectures on language and linguistic method in school.* Cambridge: Cambridge University Press.

Lavater, J.C. (1775–1778). *Physiognomische Fragmente zur Beförderung der Menschenkenntnis und Menschenliebe.* Leipzig: Weidmanns Erben und Reich. (Ibid. Eine Auswahl (1984) Stuttgart: Reklam.)

Lecours, A.R. & Joanette, Y. (1984). François Moutier or "From folds to folds." *Brain and Cognition, 3,* 198–230.

Lee, B.S. (1950a). Some effects of side-tone delay. *Journal of the Acoustical Society of America, 22,* 639–640.

Lee, B.S. (1950b). Effects of delayed speech feedback. *Journal of the Acoustical Society of America, 22,* 824–826.

Lee, B.S. (1951). Artificial stutterer. *Journal of Speech and Hearing Disorders, 16,* 53–55.

Lee, D. (1944). Categories of the generic and the particular in Wintu. *American Anthropologist, 46,* 362–369.

Lee, P. (1996). *The Whorf theory complex.* Amsterdam: John Benjamins.

Lélut, L.F. (1865). Rapport sur le mémoire de M. Dax relatif aux fonctions de l'hémisphère gauche du cerveau (6 décembre 1864). *Bulletin de l'Académie Impériale de Médecine, 30,* 173–175.

Lenneberg, E.H. (1953). Cognition and ethnolinguistics. *Language, 29,* 463–471.

Lenneberg, E.H. (1957). A probabilistic approach to language learning. *Behavioral Science, 2,* 1–12.

Lenneberg, E.H. & Roberts, J.M. (1956). The language of experience. A study in methodology. *Supplement to International Journal of American Linguistics, 22, Memoir 13.*

Lenneberg, E.H. (1967). *The biological foundations of language.* New York: John Wiley & Sons.

Leontiev, A. (1971). *Sprache - Sprechen - Sprechtätigkeit.* Stuttgart: Kohlhammer.

Leopold, W. (1939, 1947, 1949, 1949). *Speech development of a bilingual child.* 4 Vols. New York: AMS Press.

Leopold, W. (1942). (Review of) Kindersprache, Aphasie und allgemeine Lautgesetze, by Roman Jakobson. *Language, 18,* 253–254.

Leopold, W. F. (1952). *Bibliography of child language.* Evanston: Northwestern University Press. Revised and augmented by Slobin, D.I. (1972). Bloomington: Indiana University Press.

Levelt, C. (1994). *On the acquisition of place.* Leiden: Holland Institute of Linguistics.

Levelt, W.J.M. (1974/2008). *Formal grammars in linguistics and psycholinguistics.* The Hague (1974): Mouton (1974) and Amsterdam (2008): John Benjamins.

Levelt, W.J.M. (1982). Linearization in describing spatial networks. In S. Peters & E. Saarinen (Eds.), *Processes, Beliefs, and Questions. Essays on Formal Semantics of Natural Language and Natural Language Processing.* Dordrecht: Reidel, pp. 199–220.

Levelt, W.J.M. (1983). Monitoring and self-repair in speech. *Cognition, 14,* 41–104.

Levelt, W.J.M. (1989). *Speaking: From intention to articulation.* Cambridge, MA: MIT Press.

Levelt, W.J.M. (2004). Speech, gesture and the origins of language. *European Review, 12,* 543–549.

Levelt, W.J.M. & Noordman, L.G.M. (1978). The noun–verb intersection method for the study of word meaning. *Methodology and Science, 11,* 86–113.

Levelt, W.J.M., Roelofs, A., & Meyer, A. (1999). A theory of lexical access in speech production. *Behavioral & Brain Sciences, 22,* 1–75.

Levinson, S.C. (2003). *Space in language and cognition. Explorations in cognitive diversity.* Cambridge: Cambridge University Press.

Levinson, S. (2012). Foreword. In: J.B. Carroll, S.C. Levinson and P. Lee (Eds.), *Language, thought, and reality: selected writings of Benjamin Lee Whorf.* 2nd edition. Cambridge, MA: MIT Press.

Levi-Strauss, C., Jakobson, R., Voegelin, C.F., & Sebeok, T.A. (1953). *Results of the conference of anthropologists and linguists.* Baltimore: Waverly Press.

Lévy-Brühl, L. (1910). *Les fonctions mentales dans les sociétés inférieures.* Paris: Alcan.

Lewis, M.M. (1936). *Infant speech.* London: Routledge & Kegan Paul.

Libby, W. (1910). An experiment in learning a foreign language. *Pedagogical Seminary, 17,* 81–96.

Liberman, A.M. (1957). Some results of research on speech perception. *Journal of the Acoustical Society of America, 29,* 117–123.

Lichtheim, L. (1885). Über Aphasie. *Deutsches Archiv für klinische Medizin, 36,* 204–268. Also: On Aphasia. *Brain,* 7, 433–484.

Liepmann, H. (1900). Das Krankheitsbild der Apraxie (Motorische asymbolie). *Monatschrift für Psychiatrie und Neurologie, 8,* 15–44.

Liepmann, H. (1909). Zum Stande der Aphasiefrage. *Neurologisches Centralblatt, 28,* 449–484.

Liepmann, H. (1914). Carl Heilbronner †. *Deutsche Medizinische Wochenschrift, 40,* 1941–1942.

Ligon, E.M.A. (1932). Genetic study of color naming and word reading. *American Journal of Psychology, 44,* 103–121.

Lindner, G. (1882). Beobachtungen und Bemerkungen über die Entwicklung der Sprache des Kindes. *Kosmos, 6,* 321–342, 430–441.

Lipps, H. (1958). *Die Verbindlichkeit der Sprache.* Frankfurt: Vittorio Klostermann.

Lipton, H. & Blanton, R.L. (1957). The semantic differential and mediated generalization as measures of meaning. *Journal of Experimental Psychology, 6,* 431–437.

Löbisch, J.E. (1851). *Entwicklungsgeschichte der Seele des Kindes.* Vienna: Carl Haas.

Logan, G. (2008). My adventures with Stroop. *De Psychonoom, 23,* 13–14.

Lombroso, P. (1894). *Saggi di psicologia del bambino.* Torino: Roux & Co.

Lordat, J. (1843). Analyse de la parole pour servir à la théorie de divers cas d'ALALIE et de PARALALIE (de mutisme et d'imperfection du parler) que les Nosologistes ont mal connus. *Journal de la Société de Médecine Pratique de Montpellier, 7*, 333–353, 417–433.

Lucy, J. (1992). *Language diversity and thought*. Cambridge: Cambridge University Press.

Luria, A.R. (1961). *The role of speech in the regulation of normal and abnormal behavior*. Oxford: Pergamon Press.

Luria, A.R. (1966). Kurt Goldstein and neuropsychology. *Neuropsychologia, 4*, 311–313.

Luria, A.R. (1970). *Traumatic aphasia. Its syndromes, psychology and treatment*. (revised translation of the 1947–1959 Russian original). Berlin: Walter de Gruyter & Co.

Luria, A.R. (1971). Towards the problem of the historical nature of psychological processes. *International Journal of Psychology, 6*, 259–272.

Luria, A.R. (1979). *The making of mind. A personal account of soviet psychology*. Cambridge, MA: Harvard University Press. <http://luria.ucsd.edu/index.html> accessed 8 Aug 2012.

Lyell, Ch. (1863). *The antiquity of man from geological evidences*. London: Murray.

Lyon, D.O. (1914). The relation of length of material to time taken for learning. *Journal of Educational Psychology, 5*, 1–9, 85–91, 155–163.

Maas, U. (2010). *Verfolgung und Auswanderung deutschsprachiger Sprachforscher*. 2 Vols. Tübingen: Stauffenburg Verlag.

Machado y Álvarez, D.A. (1887). Titin. A study of child language. *Transactions of the Philological Society, 20*, 68–74.

Maclay, H. (1958). An experimental study of language and non-linguistic behavior. *Southwestern Journal of Anthropology, 14*, 220–229.

Maclay, H., & Osgood, C.E. (1959). Hesitation phenomenain spontaneous English speech. *Word, 15*, 19–44.

MacLeod, C. M. (1991a). Half a century of research on the Stroop effect: An integrative review. *Psychological Bulletin, 109*, 163–203.

MacLeod, C. M. (1991b). John Ridley Stroop: Creator of a landmark cognitive task. *Canadian Psychology, 32*, 521–524.

MacNamara, N.C. (1908). *Human speech. A study in the purposive action of living matter*. London: Kegan Paul, Trench, Trübner & Co.

MacNeillage, P. (1964). Typing errors as clues to serial order mechanisms in language behavior. *Language and Speech, 7*, 144–159.

Major, D.R. (1906). *First steps in mental growth*. New York: Macmillan.

Mallery, G. (1879–1880). *Sign language among North American Indians*. First Annual Report of the Bureau of Ethnology. Washington: Smithsonian Institution, pp. 269–552.

Mandelbrot, B. (1954). Structure formelle des textes et communication. *Word, 10*, 1–27.

Mandler, G. (2007). Psychologists and the National Socialist access to power. *History of Psychology, 5*, 190–200.

Mandler, G. (2007). *Modern experimental psychology. From James and Wundt to cognitive science*. Cambridge, MA: MIT Press.

Marbe, K. (1901). *Experimentell-psychologische Untersuchungen über das Urteil, eine Einleitung in die Logik*. Leipzig: Engelmann.

Marie, P. (1906a). La troisième circonvolution frontale gauche ne joue aucun rôle spécial dans la fonction de langage. *La Semaine Médicale, 26*, 241–247.

Marie, P. (1906b). Que faut-il penser des aphasies sous corticales (aphasies pures)? *La Semaine Médicale, 26*, 493–500.

Marie, P. (1906c). L'aphasie de 1861–1866; essai de critique historique sur la genèse de la doctrine de Broca. *Semaine Médicale, 26*, 565–574.

Marie, P. (1922). Existe t'il chez l'homme des centres pré-formés ou innés du langage? *Questions Neurologique d'Actualité*. Paris: Masson et Cie, pp. 527–551.

Marie, P. (1926). *Travaux et Mémoires*. 2 Vols. Paris: Masson et Cie.

Markey, J.F. (1928). *The symbolic process and its integration in children: A study in social psychology*. New York: Harcourt Brace.

Markey, J.F. (1929). Review of *The psychology of language* by Walter B. Pillsbury and Clarence L. Meader. *American Journal of Sociology, 35*, 324–325.

Marshall, J.C. (1994). Henry Charlton Bastian. In: P. Eling (Ed.): *Reader in the histroy of aphasia*. Amsterdam: John Benjamins, pp. 105–116.

Marshall, J.C. & Gurd, J.M. (1994). Franz Joseph Gall: Genius or charlatan? *Journal of Neurolinguistics, 8*, 289–293.

Marslen-Wilson, W.D. (1984). Function and process in spoken word recognition. In: H. Bouma & D.G. Bouwhuis (Eds.), *Attention and performance: Control of language processes*. Hillsdale, N.J: Erlbaum.

Marslen-Wilson, W.D. & Welsh, A. (1978). Processing interactions during word-recognition in continuous speech. *Cognitive Psychology, 10*, 29–63.

Marty, A. (1908). *Untersuchungen zur Grundlegung der allgemeinen Grammatik und Sprachphilosophie*. Halle: Niemeyer.

McCarthy, D. (1929). The vocalizations of infants. *Psychological Bulletin, 26*, 625–651.

McCarthy, D.A. (1930). *The language development of the preschool child*. Westport: Greenwood Press.

McCarthy, D. (1931). Language development. In: C. Murchison (Ed.). *A handbook of child psychology*. Worcester: Clark University Press. (2nd edition, 1933, pp. 329–373).

McGinnies, E. (1949). Emotionality and perceptual defense. *Psychological Review, 56*, 244–251.

McGinnies, E. (1950). Discussion of Howes' and Salomon's note on "Emotionality and perceptual defense." *Psychological Review, 57*, 235–240.

McMurray, G.A. (1958). A study of "fittingness" of signs to words by means of the semantic differential. *Journal of Experimental Psychology, 56*, 310–312.

Mead, M. (1927). Group intelligence tests and linguistic disability among Italian children. *School and Society, 25*, 465–468.

Meischner, W. & Eschler, E. (Eds.) (1979). *Wilhelm Wundt*. Leipzig: Urania.

Melrose, J.A. (1922). The crux of the psychological problem. *Psychological Review, 29*, 113–131.

Meringer, M. (1897). Buchbesprechung Grammont (1895). *Indogermanische Anzeiger, 12*, 8–14.

Meringer, M. (1908). *Aus dem Leben der Sprache. Versprechen, Kindersprache, Nachahmungstrieb*. Berlin: Behr.

Meringer, R. (1909). Wörter und Sachen. *Germanisch-Romanische Monatsschrift, 1*, 593–598.

Meringer, R. (1923). Die täglichen Fehler im Sprechen, Lesen und Handeln. *Wörter und Sachen, 8*, 122–140.

Meringer, M. & Mayer, C. (1895). *Versprechen und Verlesen. Eine psychologisch-linguistische Studie*. Stuttgart: Göschen'sche Verlagshandlung. New edition by A. Cutler & D. Fay (Eds.) (1978). Amsterdam: Benjamins.

Merkel, J. (1885). Die zeitliche Verhältnisse der Willenstätigkeit. *Philosophische Studien, 2*, 73–127.

Messmer, O. (1904). *Zur Psychologie des Lesens bei Kindern und Erwachsenen*. Leipzig: Engelmann.

Métraux, A. & Herrmann, Th. (Eds.) (1991). *Otto Selz. Wahrnehmungsaufbau und Denkprozeß*. Bern: Huber.

Meumann, E. (1902). Die Entstehung der erste Wortbedeutungen beim Kinde. *Philosophische Studien, 20*, 152–214.

Meumann, E. (1903). *Die Sprache des Kindes*. Zürich: Zürcher & Furrer.

Meumann, E. (1914). *Vorlesungen zur Einführung in die experimentelle Pädagogik: Das sprachliche Unterrichtsgebiet*. Vol. 3. Leipzig: Engelmann.

Meyer, A. (1905). Aphasia. *Psychological Bulletin, 2*, 261–277.

Meyer, M. (1911). *The Fundamental Laws of Human Behavior: Lectures on the Foundations of any Mental or Social Science.* Boston: Richard G. Badger.

Meyer, M. (1921). *Psychology of the other-one.* Columbia, MI: Missouri Book Co.

Meyer, M. (1923). Review of Wilhelm Wundt, Eine Würdigung. *Psychological Bulletin, 20*, 695–696.

Meyer, M. (1934). Fitting into a silent world: The first six years of life. *University of Missouri Studies, 9*, No. 2.

Meyer, T. (1914). *Aus einer Kinderstube. Tagebuchblätter einer Mutter.* Leipzig: B.G. Teubner.

Meyer-Lindenberg, J. (1991). The holocaust German psychiatry. *British Journal of Psychiatry, 159*, 7–12.

Michael, W. & Crawford, C.C. (1927). An experiment in judging intelligence by voice. *Journal of Educational Psychology, 18*, 107–114.

Michelson, T. (1917). Ethnology – Remarks on American Indian languages, a study in method. *Journal of the Washington Academy of Sciences, 7*, 222–234.

Michotte, A. (1954). *Autobiographie.* Louvain: Nauwelaerts.

Miles, W.R. & Shen, E. (1925). Photographic recording of eye movements in the reading of Chinese in vertical and horizontal axes: method and preliminary results. *Journal of Educational Psychology, 8*, 344–362.

Miller, G.A. (1947). The masking of speech. *Psychological Bulletin, 44*, 105–129.

Miller, G.A. (1951). *Language and communication.* New York: McGraw-Hill.

Miller, G.A. (1953). What is information measurement? *American Psychologist, 8*, 3–11.

Miller, G.A. (1954a). Psycholinguistics. In: G. Lindzey (Ed.), *Handbook of Social Psychology.* Cambridge, MA: Addison-Wesley, pp. 693–708.

Miller, G.A. (1954b). Communication. *Annual Review of Psychology, 5*, 401–420.

Miller, G.A. (1956). The magical number seven, plus or minus two: Some limits on our capacity for processing information. *Psychological Review, 63*, 81–97.

Miller, G.A. (1965). Introduction to the second edition of G.K. Zipf, *The psychobiology of language.* Cambridge, MA: MIT Press.

Miller, G.A. (1962). *Psychology: The science of mental life.* New York: Harper & Row.

Miller, G.A. (1969). A psychological method to investigate verbal concepts. *Journal of Mathematical Psychology, 6*, 169–191.

Miller, G.A. (1991). *The science of words.* New York: Scientific American Library.

Miller, G.A., Bruner, J.S., & Postman, L. (1954). Familiarity of letter sequences and tachistoscopic identification. *Journal of General Psychology, 50*, 129–139.

Miller, G.A. & Frick, F.C. (1949). Statistical behavioristics and sequences of responses. *Psychological Review, 56*, 311–324.

Miller, G.A., Galanter, E., & Pribram. K. (1960). *Plans and the structure of behavior.* New York: Holt.

Miller, G.A., Heise, G.A., & Lichten, W. (1951). The intelligibility of speech as a function of the context of the test materials. *Journal of Experimental Psychology, 41*, 167–173.

Miller, G.A. & Johnson-Laird, P.N. (1976). *Language and perception.* Cambridge, MA: Harvard University Press.

Miller, G.A. & Licklider, J.C.R. (1950). The intelligibility of interrupted speech. *Journal of the Acoustical Society of America, 22*, 167–173.

Miller, G.A. & Nicely, P.E. (1955). An analysis of perceptual confusions among some English consonants. *Journal of the Acoustical Society of America, 27*, 338–352.

Miller, G.A., & Selfridge, J.A. (1950). Verbal context and the recall of meaningful material. *American Journal of Psychology, 63*, 176–185.

Miller, I. (1957). Perception of nonsense passages in relation to amount of information and speech-to-noise ratio. *Journal of Experimental Psychology, 53*, 388–393.

Mills, C.K. (1907). Discussion on aphasia, especially with reference to the views of Marie. *Journal of Nervous and Mental Disorders, 34,* 459–473.

Minami, M. (2002). Review of "Bilingualism in development: Language, literacy and cognition." by Ellen Bialystok. *Bilingual Research Journal, 26,* 730–735.

Mitchell, T.M., Shinkareva S.V., Carlson A., Chang K.-M., Malave V.L., Mason R.A., & Just M.A. (2008). Predicting human brain activity associated with the meanings of nouns. *Science, 320,* 1191–1195.

Mithen, S. (2006). *The singing Neanderthals. The origins of music, language, mind, and body.* Cambridge, MA: Harvard University Press.

von Monakow, C. (1897). *Gehirnpathologie.* Vienna: Alfred Hölder (2nd edition 1905).

Moore, T.V. (1915). The temporal relations of meaning and imagery. *Psychological Review, 22,* 177–225.

Morris, C.W. (1938). Foundations of the theory of signs. In: O. Neurath (Ed.), *International Encyclopedia of Unified Science.* Vol. 1, No. 2. Chicago: Chicago University Press, pp. 1–59.

Morris, C.W. (1946). *Signs, language and behavior.* New York: Prentice-Hall.

Mosier, C.I. (1941). A psychometric study of meaning. *Journal of Social Psychology, 13,* 123–140.

Moutier, F. (1908). *L'aphasie de Broca.* 2 Vols. Paris: Steinheil.

Mowrer, O.H. (1954). The psychologist looks at lnguage. *American Psychologist 9,* 660–694.

Mugdan, J. (1984). *Jan Boudouin de Courtenay (1845–1929). Leben und Werk.* München: Fink Verlag.

Mugdan, J. (1985). The origin of the phoneme. Farewell to a myth. *Lingua Posnaniensis 28,* 137–150.

Mulder, H. (1936). *Cognition and volition in language.* Groningen: Wolters Uitgevers-Maatschappij.

Müller, F. (1876–1888). *Grundriss der Sprachwissenschaft.* 4 Vols. Vienna: Alfred Hölder.

Müller, H. (1935). *Experimentelle Beiträge zur Analyse des Verhältnisses von Laut und Sinn.* Leipzig: August Pries.

Müller, J. (1833–1840). *Handbuch ders Physiologie des Menschen für Vorlesungen.* 2 Vols. Koblenz: J. Hölscher.

Müller, M. (1860, 2nd edition 1891). *The science of language.* 2 Vols. London: Longmans, Green, and Co.

Müller, M. (1861–1864). *Lectures on the Science of Language,* 2 Vols. London: Longman, Green, Longman, and Roberts.

Müller, M. (1870). *Darwinism tested by the Science of Language.* Translated from the German of Professor August Schleicher, by Dr. Alex.V.W. Bikkers. London: Hotten, 1869. *Nature,* 1, 256–259.

Müller, M. (1887). *The science of thought.* London: Longmans, Green, and Co.

Münsterberg, H. (1892). Studien zur Associationslehre. In: *Beiträge zur experimentellen Psychologie, 4,* 1–39. Freiburg: Mohr.

Neisser, U. (1954). An experimental distinction between perceptual processes and verbal response. *Journal of Experimental Psychology, 47,* 399–402.

Neumärker, K.-J. & Bartsch, A.J. (2003). Karl Kleist (1879–1960) – a pioneer of neuropsychiatry. *History of Psychiatry, 14,* 411–458.

Nevsky, I.M. & Levin, S. L. (1932). "Unconditioned and conditioned secretory activity in children during hypnosis." (In Russian). *Kazansky Meditzinsky Zhurnai, 38,* 344–351.

Newcombe, F. (1969). *Missile wounds of the brain: a study of psychological deficits.* Oxford: Oxford University Press.

Newman, S.N. (1931). Further experiments in phonetic symbolism. *American Journal of Psychology, 45,* 53–75.

Nice, M.M. (1917). Speech development of a child from eighteen months to six years. *Pedagogical Seminary, 24,* 204–243.

Nice, M.M. (1920). Concerning all-day conversations. *Pedagogical Seminary, 27,* 166–177.

Nice, M.M. (1925). Length of sentences as a criterion of a child's progress in speech. *Journal of Educational Psychology, 16,* 370–379.

Nida, E.A. (1949). *Morphology. The descriptive analysis of words.* Ann Arbor: University of Michigan Press.

Nielsen, J.M. (1936). *Agnosia, apraxia, aphasia. Their value in cerebral localization.* New York: Paul Hoeber (2nd edition 1946).

Niessl von Mayendorf, E.G. (1930). *Vom Lokalisationsproblem der artikulierten Sprache.* Leipzig: Johan Ambrosius Barth.

Noble, C.N. (1952). An analysis of meaning. *Psychological Review, 59,* 421–430.

Noble, C.N. & McNeely, D.A. (1957). The role of meaningfulness (*m*) in paired-associate verbal learning. *Journal of Experimental Psychology, 53,* 16–22.

Noiré, L. (1877). *Der Ursprung der Sprache.* Mainz: Victor v. Zabern.

O'Brien, J.A. (1921). *Silent reading.* New York: Macmillan.

O'Brien, J.A. (1926). *Reading: Its psychology and pedagogy.* New York: Century Co.

O'Connor, J. (1928). *Born that way.* Baltimore: William & Wilkins.

Oehl, W. (1933). *Das Lallwort in der Sprachschöpfung.* Freiburg: St Paulusdruckerei.

Ogden, C.K. (1930). *Basic English: A general introduction with rules and grammar.* London: Paul Treber.

Ogden, C.K. & Richards, I.A. (1923). *The meaning of meaning. A study of the influence of thought and of the science of symbolism.* London: Kegan Paul, Trench, Trubner & Co.

Ogle, W. (1867). Aphasia and agraphia. *St. George & Apostles Hospital Report, 2,* 83–122.

Ohms, H. (1910). Untersuchungen unterwertiger Assoziationen mittels des Worterkennungsvorgangs. *Zeitschrift für Psychologie, 56,* 1–84.

Ohwaki, Y. (1933). Die ersten zwei Jahre der Sprachentwicklung des japanischen Kindes. Ein Beitrag zur Psychologie der Kindersprache. *Tohoku Psychologica Folia, 1,* 71–110.

Oldfield, R.C. (1941). *The psychology of the interview.* London: Methuen.

Oldfield, R.C. & Wingfield, A. (1965). Response latencies in naming objects. *Quarterly Journal of Experimental Psychology, 17,* 273–281.

Oldfield, R. C. & Zangwill, O. (1938). The acquisition of verbal repetition habits. *British Journal of Psychology. General Section, 29,* 12–26.

Oltuszewski, W. (1897). *Die geistige und sprachliche Entwicklung des Kindes.* Monographic offprint from *Medizinische-pädagogische Monatschrift für die gesammte Sprachheilkunde, 6.* Berlin: H. Kornfeld.

Oltuszewski, W. (1901). *Psychologie und Philosophie der Sprache.* Berlin: H. Kornfeld.

Ombredane, A. (1933). *Le langage.* In: G. Dumas (Ed.), *Nouveau traité de psychologie.* Vol. 3, Paris: Presses Universitaires de France, pp. 363–458.

Ombredane, A. (1951). *L'Aphasie et l'élaboration de la pendée explicite.* Paris: Presses Universitaires de France.

Orwell, G. (1946). *Politics and the English language.* Re-issued in G. Orwell, *Essays.* London: Penguin Books, pp. 348–359.

Osgood, C.E. (1941). Ease of individual judgment-processes in relation to polarization of attitudes in the culture. *Journal of Social Psychology, 14,* 403–418.

Osgood, C.E. (1952). The nature and measurement of meaning. *Psychological Bulletin, 49,* 197–237.

Osgood, C.E. (1953). *Method and theory in experimental psychology.* New York: Oxford University Press.

Osgood, C.E. (1959). The representational model and relevant research materials. In: I. de Sola Pool (Ed.). *Trends in content analysis.* Urbana, IL: University of Illinois Press, pp. 33–88.

Osgood, C.E. (1971). Where do sentences come from? In: D. Steinberg & L. Jakobovits (Eds.), *Semantics: An interdisciplinary reader in philosophy, linguistics and psychology.* Cambridge: Cambridge University Press, pp. 497–529.

Osgood, C.E. (1975). A dinosaur caper: Psycholinguistics past, present, and future. *Annals of the New York Academy of Sciences, 263*, 16–26.

Osgood, C.E. & Bock, J.K. (1977). Salience and sentencing: Some production principles. In: S. Rosenberg (Ed.), *Sentence production: Developments in research and theory*. Hillsdale, NJ: Lawrence Erlbaum, pp. 89–140.

Osgood, C.E., May, W.H., & Miron, M.S. (1975). *Cross-cultural universals of affective meaning*. Chicago: University of Illinois Press.

Osgood, C.E. & Sebeok, T.A. (1954). *Psycholinguistics. A survey of theory and research problems*. Bloomington: Indiana University Press (2nd edition 1965).

Osgood, C.E., Suci, G.J., & Tannenbaum, P.H. (1957). *The measurement of meaning*. Urbana, IL: University of Illinois Press.

O'Shea, M.V. (1907). *Linguistic development and education*. New York: Macmillan.

Otis, A.S. (1920). Do we think in words? *Psychological Review, 27*, 399–419.

Paget, R. (1930). *Human speech. Some observations, experiments, and conclusions at to the nature, origin, purpose and possible improvement of human speech*. London: Kegan Paul, Trench, Trubner & Co.

Paul, H. (1880/1975). *Prinzipien der Sprachgeschichte*. Halle/Tübingen: Niemeyer.

Pavlov, I. (1904). Physiology of digestion. *Nobel Lecture*. In: Nobel Foundation (Ed.) (1967), *Nobel lectures: Physiology or medicine, 1901–1921*. New York: Elsevier, pp. 141–155.

Pavlovitch, M. (1920). *Le langage enfantin. Acquisition du serbe et du français par un enfant serbe*. Paris: Champion.

Pear, T.H. (1920). Is thinking merely the action of language mechanisms? *British Journal of Psychology, 11*, 71–80.

Pear. T.H. (1931). *Voice and personality*. London: Chapman and Hall.

Pear, T.H. (1932). Speech as an expression of personality. *British Journal of Educational Psychology, 2*, 139–152.

Pearson, K. (1911). *The Grammar of Science*. 3rd edition. London: Walter Scott.

Pelsma, J.R. (1910). A child's vocabulary and its development. *Pedagogical Seminary, 17*, 328–369.

Penfield, W. & Roberts, L. (1959). *Speech and brain mechanisms*. Princeton: University Press.

Perea, M., Moret-Tatai, C., & Carreiras, M. (2011). Facilitation versus inhibition in the masked priming same–different matching task. *Quarterly Journal of Experimental Psychology, 64*, 2065–2079.

Perez, B. (1878). *La psychologie de l'enfant. Les trois premières années de l'enfant*. Paris: Félix Alcan.

Perez, B. (1881). *Thierri Tiedemann et la science de l'enfant*. Paris: G. Baillière.

Perez, B. (1886). *L'enfant de trois à sept ans*. Paris: Félix Alcan.

Peterson, F., & Jung, C.G. (1907). Psycho-physical investigations with the galvanometer and pneumograph in normal and insane individuals. *Brain, 30*, 153–218.

Peterson, G.E. & Barney, H.L. (1952). Control methods in a study of the vowels. *Journal of the Acoustical Society of America, 24*, 175–184.

Piaget, J. (1923). *La langage et la pensée chez l'enfant*. Neuchatel: Delachaud & Niestlé. English translation (1926, 1959). *The language and thought of the child*. London: Routledge & Kegan Paul.

Piaget, J. (1954). Le langage et la pensée du point de vue génétique. In: G. Révèsz (Ed.), *Thinking and speaking: A symposium*. Amsterdam: North-Holland Publishing Company, pp. 51–124.

Piaget, J. (1962). Comments on Vygotsky's critical remarks concerning *The language and thought of the child*, and *Judgment and reasoning in the child*. Cambridge: MIT Press.

Pick, A. (1898). *Beiträge zur Pathologie und pathologische Anatomie des Zentralnervensystems*. Berlin: Springer.

Pick, A. (1909). *Über das Sprachverständnis*. Leipzig: Johann Ambrosius Barth.

Pick, A. (1913). *Die agrammatischen Sprachstörungen*. Vol. I. Berlin: Springer.

Pick, A. (1931). Aphasie. In: A. Bethe, G. von Bergmann, G. Embden & A. Ellinger (Eds.) *Handbuch der normalen und pathologischen Physiologie*. Vol. 15–2. Berlin: Springer, pp. 1416–1524.

Pickett, J.M. (1957). Perception of vowels heard in noises of various spectra. *Journal of the Acoustical Society of America, 29*, 613–620.

Piéron, H. (1923). *Le cerveau et la pensée*. Paris: Félix Arcan.

Piéron, H. & Meyerson, I. (1938). *Onzième Congrès International de Psychologie. Paris, 25–31 Juillet 1937*. Paris: Félix Alcan.

Pillsbury, W.B. (1897). A study of apperception. *American Journal of Psychology, 8*, 315–393.

Pillsbury, W.B. & Meader, C.L. (1928). *The psychology of language*. New York: Appleton.

Pisko, F.J. (1865). *Die neueren Apparate der Akustik: Für Freunde der Naturwissenschaft und der Tonkunst*. Vienna: Carl Gerold.

Ploog, D. (2002). Klaus Conrad (1905–1961). *History of Psychiatry, 13*, 339–357.

Ploog, D. (2003). Is the neural basis of vocalization different for non-human primates and Homo sapiens? *Proceedings of the British Academy, 106*, 121–135.

Pollock, F. (1878). An infant's progress in language. *Mind, 3*, 392–401.

Postman, L., Bruner, J.S., & McGinnies, E. (1948). Personal values as selective factors in perception. *Journal of Abnormal and Social Psychology, 43*, 142–155.

Pott, A.F. (1859–1876) *Etymologische Forschungen auf dem Gebiete der indogermanische Sprachen mit besonderem Bezug auf die Lautumwandlung im Sanskrit, Griechischen, Lateinischen, Littauischen und Gothischen*. 6 Vols. Detmold-Lemgo: Meyer'sche Hofbuchhandlung.

Potter, R.K. & Steinberg, J.C. (1950). Toward the specification of speech. *Journal of the Acoustical Society of America, 22*, 807–820.

Pound, L. (1934). On the linguistics of dreams. *American Speech, 9*, 175–180.

Preston, K.A. (1935). The speed of word perception and its relation to reading ability. *Journal of General Psychology, 13*, 199–203.

Preyer, W. (1882). *Die Seele des Kindes. Beobachtungen über die geistige Entwicklung des Menschen in den ersten Lebensjahren*. Leipzig: Th.Grieben's Verlag. New edition by Georg Eckhart (1989): Berlin: Springer

Preyer, W. Th. (1896). *Darwin. Sein Leben und Wirken*. Berlin: Hoffmann.

Prinz, W. (1985). Ganzheits- und gestaltpsychologie und Nationalsozialismus. In: C.F. Graumann (Ed.). *Psychologie im Nationalsozialismus*. Berlin: Springer. pp. 89–111.

Pronko, N.H. (1946). Language and psycholinguistics. *Psychological Bulletin, 43*, 189–239.

Quanz, J.O. (1897). Problems in the psychology of reading. *Psychological Review, 1*. Monogr. Suppl. Vol. II, *1*, 1–51.

Rasmussen, V. (1913). *Barnets sjælige udvikling in de første fire aar*. Copenhagen/Christiana: Gyldendal.

Rasmussen, V. (1922). *Et barns dagbog*. Copenhagen/Christiana: Gyldendal.

Rayner, K. & Pollatsek, A. (1989). *The psychology of reading*. Englewood Cliffs: Prentice-Hall.

Razran, G. (1939). A quantitative study of meaning by a conditioned salivary technique (semantic conditioning). *Science, 90*, 89–90.

Razran, G. (1961). The observable unconscious and the inferable conscious in current Soviet psychophysiology: Interoceptive conditioning, semantic conditioning, and the orienting reflex. *Psychological Review, 68*, 81–147.

Razran, G. (1965). Russian physiologists' psychology and American experimental psychology: A historical and a systematic collection and a look into the future. *Psychological Bulletin, 63*, 42–64.

Reinach, A. (1913). *Die apriorischen Grundlagen des bürgerlichen Rechtes*. Halle: Max Niemeyer.

Reinert, G. (1991). In memoriam Otto Selz. Spoken upon the occasion of the posthumous award of the Wilhelm Wundt Plakette in 1971. In: N. Frijda & A.D. De Groot (Eds.), *Otto Selz: His contribution to psychology*. The Hague: Mouton, pp. 13–19.

Révèsz, G. (1950). *Origine et pre-histoire du langage.* Paris: Payot.

Révèsz, G. (Ed.) (1954). *Thinking and speaking: A symposium.* Amsterdam: North-Holland Publishing Company.

Ribot, T. (1891). Enquête sur les idées générales. *Revue Philosophique, 32,* 376–388.

Rich, S. (1953). *The perception of emotion.* Unpublished Honors thesis. Radcliffe College.

Richter, E. (1912). *Wie wir sprechen.* Leipzig: Teubner Verlag.

Richter, F. (1927). *Die Entwicklung der psychologischen Kindersprachforschung bis zum Beginn des 20. Jahrhunderts.* Münster: Münsterverlag.

Riess, B.F. (1940). Semantic conditioning involving the galvanic skin reflex. *Journal of Experimental Psychology, 26,* 238–240.

Riess, B.F. (1946). Genetic changes in semantic conditioning. *Journal of Experimental Psychology, 36,* 143–152.

Rigg, M.G. (1938). A superior child who would not talk. *Child Development, 9,* 361–362.

Rizzolatti, G. & Arbib, M.A. (1998). Language within our grasp. *Trends in Neuroscience, 21,* 188–194.

Roelofs, A.P.A. (2003). Goal-referenced selection of verbal action: Modeling attentional control in the Stroop task. *Psychological Review, 110,* 88–124.

Romanes, G. (1882). *Animal intelligence.* London: Kegan Paul.

Romanes, G. (1889). *Mental evolution in man: Origin of human faculty.* London: Kegan Paul.

Ronjat, J. (1913). *Le développement du langage observé chez un enfant bilingue.* Paris: Librairie Ancienne H. Champion.

Rosenfeld, S. (2005). The political uses of sign language: The case of the French revolution. *Sign Language Studies, 6,* 17–37.

Röttger, F. (1931). *Phonetische Gestaltbildung bei jungen Kindern.* München: Beck.

Rousseau, J.-J. (1762). *Émile ou de l'éducation.* The Hague: Jean Néaulme.

Rousseau, J.-J. (1781). *Essai sur l'origine des langues où il est parlé de la mélodie et de la imitation musicale.* Paris: Nizet (re-issued 1981).

Rosenzweig, M.R. & Postman, L. (1958). Frequency of usage and the perception of words. *Science, 127,* 263–266.

Rubenstein, H. & Aborn, M. (1960). Psycholinguistics. *Annual Review of Psychology, 11,* 291–322.

Rüdin, E. (1937). Eröffnungsansprache zur Jahresversammlung der Gesellschaft Deutscher Neurologen und Psychiater. *Zeitschrift für die gesamte Neurologie und Psychiatrie, 158,* 1–6.

Ruederer, H. (1916). *Über die Wahrnehmung des gesprochenen Wortes.* Borna-Leipzig: Robert Noske.

Russell, O. (1931). *Speech and voice.* New York: Macmillan.

Russell, W.R. (1959). *Brain, memory, learning: a neurologist's view.* Oxford: Clarendon Press.

Russell, W.R. & Nathan, P.W. (1946). Traumatic amnesia. *Brain, 69,* 280–300.

Sachs, H. (1905). *Gehirn und Sprache.* Wiesbaden: Bergmann.

Sacks, H., Schegloff, I., & Jefferson, G. (1974). A simplest systematics for the organization of turn-taking in conversation. *Language, 50,* 696–735.

Saer, D.J. (1923). The effect of bilingualism on intelligence. *British Journal of Psychology. General Section, 14,* 25–38.

Saer, D.J., Smith, F., & Hughes, J. (1924). *The bilingual problem.* Averystwyth: University College Wales.

Salisbury, A. (1894). A child's vocabulary. *Educational Review, 7,* 289–290.

Salomon, E. (1914). Motorische Aphasie mit Agrammatismus und sensorisch-agrammatische Störungen. *Monatschrift für Psychiatrie und Neurologie, 35,* 181–208, 216–274.

Sander, F. (1934). Seelische Struktur und Sprache. Strukturpsychologisches zum Zweisprachenproblem. In: O. Klemm, H. Volkert, & K. von Dürckheim-Montmartin (Eds.), *Ganzheit und Struktur. Festschrift zum 60. Geburtstag Felix Kruegers.* München: Beck, pp. 59–67.

Sapir, E. (1921). *Language. An introduction to the study of speech.* New York: Harcourt, Brace and Company.

Sapir, E. (1924). The grammarian and his language. *American Mercury, 1,* 149–155.

Sapir (1926). Language as a form of human behavior. *English Journal, 16,* 421–433.

Sapir, E. (1927). Speech as a personality trait. *American Journal of Sociology, 32,* 892–905.

Sapir, E. (1929a). A study in phonetic symbolism. *Journal of Experimental Psychology, 12,* 225–239.

Sapir, E. (1929b). The status of linguistics as a science. *Language, 5,* 207–214.

Sapir, E. (1931). Conceptual categories in primitive languages. *Science, 74,* 578.

Sapir, E. (2008). *The collected works of Edward Sapir.* P. Swiggers (Ed.). Berlin: Mouton.

Saporta, S. (Ed.) (1961). *Psycholinguistics. A book of readings.* New York: Holt, Reinehart and Winston.

de Saussure, F. (1915). *Cours de linguistique générale.* (C. Bally & A. Sechehaye, Eds.), Paris-Lausanne: Payot. (English edition: Course in general linguistics. New York: McGraw-Hill, 1966).

Sayce, A.K. (1889). Children's language in the Omani dialect of Arabia. *The Academy, 36.* (see Leopold 1952).

Schaefer, J. (1928). Die erlebte Wortbedeutung bei Darbietung von Einzelwörtern. *Archiv für die gesamte Psychologie, 61,* 129–196.

Schiller, F. (1947). Aphasia studied in patients with missile wounds. *Journal of Neurology and Psychiatry, 10,* 183–197.

Schleicher, A. (1853). Die ersten Spaltungen des indogermanischen Urvolkes. *Allgemeine Monatsschrift für Wissenschaft und Literatur, 3,* 876–887.

Schleicher, A. (1860). *Die deutsche Sprache.* Stuttgart.

Schleicher, A. (1863). *Die Darwinische Theorie und die Sprachwissenschaft.* Weimar: Böhlau. Also in English translation by A. Bikkers (1869): *Darwinism tested by the Science of Language.* London: Hotten.

Schleicher, A. (1865). *Über die Bedeutung der Sprache für die Naturgeschichte des Menschen.* Weimar: Böhlau.

Schlossberg, H. & Heineman, C. (1950). The relationship between two measures of response strength. *Journal of Experimental Psychology, 40,* 235–247.

Schmalz, E. (1848). *Über die Taubstummen und ihre Bildung in ärtzlicher, statistischer, pädagogischer und geschichtlicher Hinsicht: nebst einer Anleitung zur zweckmässigen Erziehung der taubstummen Kinder im älterlichen Hause: mit vielen Tabellen.* Dresden und Leipzig: Arnold.

Schmidt, G. (1917). *Unsere Muttersprache als Waffe und Werkzeug des deutschen Gedankens.* Tat-Flugschriften 20. Jena: Eugen Diederichs.

Schmidt, J.B. (1871). Gehörs- und Sprachstörung in Folge von Apoplexie. *Allgemeine Zeitschrift für Psychiatrie, 27,* 304–306.

Schmidt, P., Diele, H., & Krüger, P. (2003). "Erziehungskunst kann noch lange nichts als Sammlung einzelner Erfahrungen sein." Anfänge der empirischen Kinderforschung im ausgehenden 18. Jahrhundert. In: A. Prengel (Ed.). *Im Interesse von Kindern? Forschungs- und Handlungsperspektiven in Pädagogik und Kinderpolitik.* Weinheim: Juventa Verlag.

Schmidt, W. (undated manuscript). *William Stern in the Netherlands.*

Schmidt-Rohr, G. (1932). *Die Sprache als Bildnerin der Völker.* Jena: Eugen Diedrichs.

Schmidt-Rohr. G. (1933). *Mutter Sprache – Vom Amt der Sprache bei der Volkwerdung.* Jena: Eugen Diedrichs.

Schmidt-Rohr, G. (1939). Rasse und Sprache. *Rasse, 6,* 161–168.

Schmidt, W.A. (1917). An experimental study in the psychology of reading. *Supplementary Educational Monographs, 1,* Whole No 5. Chicago: Chicago University Press.

Schmitz, H.W. (1985) Verständigungshandlungen – eine wissenschaftshistorische Rekonstruktion der Anfänge der signifischen Bewegung in den Niederlanden (1892–1926). Habilitationsschrift Bonn University. In Dutch translation: Schmitz, H.W. (1990). *De Hollandse Significa. Een reconstructie van de geschiedenis van 1892 tot 1926.* Assen/Maastricht: Van Gorcum.

Schuell, H. (1935). *Minnesota test of differential diagnosis of aphasia.* Minneanapolis: University of Minnesota Press.

Schultze, F. (1880). Die Sprache des Kindes. *Kosmos, 4,* 23–41.

Schumann, F. (1920). Das Erkennungsurteil. *Zeitschrift für Psychologie, 88,* 205–224.

Schwarz, F. H. C. (1829). *Erziehungslehre.* 3 Vols. Leipzig: Göschen.

Scripture, E.W. (1892). Ueber den associativen Verlauf der Vorstellungen. *Philosophische Studien, 7,* 50–146.

Scupin, E. & Scupin, G. (1907). *Bubis erste Kindheit. Ein Tagebuch.* Leipzig: Grieben.

Scupin, E. & Scupin, G. (1910). *Bubi im 4. bis 6. Lebensjahre.* Leipzig: Grieben.

Seashore, R.H. & Eckerson, L.D. (1940). The measurement of individual differences in English vocabularies. *Journal of Educational Psychology, 31,* 14–38.

Sechehaye, A. (1908). *Programme et méthodes de la linguistique théorique. Psychologie du langage.* Paris: Honoré Champion.

Sechehaye, A. (1926). *Essai sur la structure logique de la phrase.* Paris: Honoré Champion.

Seebohm, H.-B. (1991). The psychologist Otto Selz: His life and destiny. In: N. Frijda & A.D. De Groot (Eds.), *Otto Selz: His contribution to psychology.* The Hague: Mouton, pp.1–12.

Selz, O. (1913). *Uber die Gesetze des geordneten Denkverlaufs. Eine experimentelle Untersuchung. Erster Teil.* Stuttgart: Spemann.

Selz, O. (1922). *Zur Psychologie des produktiven Denkens und des Irrtums.* Bonn: Cohen.

Selz, O. (1924). *Die Gesetze der produktiven Geistestätigkeit: Kurzgefasste Darstellung.* Bonn: Cohen.

Senghas, A., Kita, S., & Özyürek, A. (2004). Children creating core properties of language: evidence from an emerging sign language in Nicaragua. *Science, 305,* 1779–1782.

Seuren, P. (1998). *Western linguistics. An historical introduction.* Oxford: Blackwell Publishers.

Seuren, P. (2009). *Language in cognition. Language from within.* Vol. 1. Oxford: Oxford University Press.

Shakow, D. & Rosenzweig, S. (1940). The use of the tautophone ("verbal summator") as an auditory apperceptive test for the study of personality. *Journal of Personality, 8,* 216–226.

Shannon, C.E. (1948). A mathematical theory of communication. *Bell Systems Technical Journal, 27,* 379–423, 623–656.

Shannon, C.E. and Weaver, W. (1949). *The mathematical theory of communication.* Urbana, IL: University of Illinois Press.

Sheridan, P. (1955). Research in language translation on the IBM type 701. *IBM Technical Newsletter, 9,* 5–24.

Shinn, M.W. (1900). *The biography of a baby.* Boston: Houghton, Mifflin.

Shirley, M.M. (1933). The first two years. A study of twenty-five babies. 3 Vols. Westport, CT: Greenwood Press.

Shvachkin, N.Kh. (1948). Razvitie fonematicheskogo vosprijatija rechi v rannem detstve [Development of phonematic speech perception in early childhood]. *Izvestija Akademji Pedagogicheckikh Nauk RSFSR,* Publ. No. 13.

Sidgwick, H. (1896). Experiments in voluntary whispering, and their bearing on alleged cases of thought-transference. *Dritter Internationaler Congress für Psychologie in München,* München: Lehmann, pp. 404–407.

Sievers, E. (1876). *Grundzüge der Lautphysiologie zur Einführung in das Studium der Lautlehre der indogermanischen Sprachen* Leipzig: Breitkopf & Härtel.

Sigismund, B. (1856). *Kind und Welt.* Braunschweig: Vieweg. New, undated edition by F. Förster. Leipzig: Jaeger.

Sikorsky, M. (1883). Du développement du langage ches les enfants. *Archives de Neurologie, 6,* 319–336.

Skinner, B.F., (1936). The verbal summator and a method for the study of latent speech. *Journal of Psychology, 2,* 71–107.

Skinner, B.F. (1937). The distribution of associated words. *Psychological Record, 1,* 71–76.

Skinner, B.F. (1957). *Verbal behavior*. Acton, MA: Copley Publishing Group.

Skinner, B.F. (1970). An autobiography. In: P.B. Dews (Ed.), *Festschrift for B.F. Skinner*. New York, NY: Appleton-Century-Crofts, pp. 1–22.

Skorupka, S. (1949). Obserwacje nad językiem dziecka [Observation of child language]. *Trudy Laboratorii Fiziologii Vysshey Nervnoy Deyatel'nosti Rebyonka pri Leningradskom Pedagogicheskom Institute Gertzena, 2*, 16–22. Abstracted in *Psychological Abstracts* (1934) 8, 153, 18.

Slobin, D.I. (1966). Soviet psycholinguistics. In: N. O'Connor (Ed.), *Present-day Russian psychology*. Oxford: Pergamon Press.

Slobin, D.I. (1973a). Cognitive prerequisites for the development of grammar. In: Ferguson, C. A. & Slobin, D.I. (Eds.), *Studies of child language development*. New York: Holt, Rinehart, & Winston, pp. 175–205.

Slobin, D.I. (1973b). Development of the phonemic system. Russian. In: Ferguson, C.A. & Slobin, D.I. (Eds.), *Studies of child language development*. New York: Holt, Rinehart, & Winston, pp. 91–127.

Slobin, D. I. (1987). Thinking for speaking. *Proceedings of the Thirteenth Annual Meeting of the Berkeley Linguistics Society, 13*, 435–444.

Slobin, D.I. (Ed.). (1985–1997). The crosslinguistic study of language acquisition. 5 Vols. Hillsdale, N.Y.: Lawrence Erlbaum.

Smith, B. (1988). Materials towards a history of speech act theory. In: A. Eschbach (Ed.), *Karl Bühler's theory of language*. Amsterdam: John Benjamins, pp. 125–152.

Smith, M.E. (1926). *An investigation of the development of the sentence and the extent of vocabulary in young children*. Iowa City: University of Iowa Press.

Smith, M.E. (1935). A study of the speech of eight bilingual children of the same family. *Child Development*, 6, 19–25.

Smith, M.E. (1939). Some light on the problem of bilingualism as found from a study of the progress in mastery of English among children of Non-American ancestry in Hawaii. *Genetic Psychology Monographs, 21*, 121–284.

Smith, M.K. (1941). Mesurement of the size of general English vocabulary through the elementary grades and high school. *Genetic Psychology Monographs, 24*, 311–345.

Smith, S. & Powers, F.F. (1930). The relative value of vocabulary and sentence practice for language learning. *Journal of Social Psychology, 1*, 451–461.

Smolenskaja, E. P. (1935). Verbal symbols of conditioned and differential stimuli. *Psychological Abstracts, 9*, 131, No. 1163.

Solomon, R.L. (1952). Frequency of usage as a determinant of recognition thresholds for words. *Journal of Experimental Psychology, 43*, 195–201.

Solomon, R.L. & Howes, D.H. (1951). Word probability, personal values, and visual duration thresholds. *Psychological Review, 58*, 256–270.

Solomon, R.L. & Postman, L. (1952). Frequency of usage as a determinant of recognition thresholds for words. *Journal of Experimental Psychology, 43*, 195–201.

Sommer, R. (1891). Zur Psychologie der Sprache. *Zeitschrift für Psychologie und Physiologie der Sinnesorgane*, 2, 143–163.

Spencer, H. (1855/87) *The principles of psychology*. 2 Vols. New York: D. Appleton and Company.

Spiker, C.C. & Irwin, O.C. (1949). The relationship between IQ and indices of infant speech sound development. (1949). *Journal of Speech Hearing Disorders*, 14, 335–343.

Spitzer, L. (1943). Why does language change? *Modern Language Quarterly, 4*, 413–431.

Sprague De Camp, L. (1946). Learning to talk. *American Speech, 21*, 23–28.

Sprenger, S.A., Levelt, W.J.M., & Kempen, G. (2006). Lexical access during the production of idiomatic phrases. *Journal of Memory and Language, 54*, 161–184.

Spurzheim, J.G. (1815). *The physiognomical system of Drs. Gall and Spurzheim; founded on an anatomical examination of the nervous system in general and of the brain in particular; and indicating the dispositions and manifestations of the mind.* London: Baldwin, Cradock and Joy.

Spurzheim, J. G. (1818). *Observations sur la phrénologie, ou la connaissance de l'homme moral et intellectuel fondée sur les fonctions du système nerveux.* Paris: Treuttel & Würtz.

Staal, F. (1986). *The fidelity of oral tradition and the origins of science.* Amsterdam: North-Holland Publishing Company.

Staats, A.W. & Staats, C.K. (1959). Meaning and *m*: correlated but separate. *Psychological Review, 66,* 136–144.

Stählin, W. (1914). Zur Psychologie und Statistik der Metaphern. *Archiv für die gesamte Psychologie, 31,* 297–425.

Stalin, J. (1950). Concerning Maxism in linguistics. Supplement to *New Times, 26,* June 28.

Stein, W. (1928). Tachistoskopische Untersuchungen über das Lesen mit sukzessiver Darbietung. *Archiv für die gesamte Psychologie, 64, 301–346.*

Steinthal, H. (1855). *Grammatik, Logik und Psychologie, Ihre Prinzipien und ihr Verhältniss zueinander.* Berlin: F. Dümmler. (New edition Hildesheim: Georg Olms, 1968).

Steinthal, H. (1871, 1881). *Einleitung in die Psychologie und Sprachwissenschaft.* Berlin: F. Dümmler.

Stern, C. & Stern, W. (1907). *Die Kindersprache. Eine psychologische und sprachtheoretische Untersuchung.* Leipzig: Johann Ambrosius Barth (fourth updated edition 1928. New facsimile edition: Darmstadt: Wissenschaftliche Buchgesellschaft). For the complete Stern diaries, see <http://www.mpi.nl/resources/data/stern-diaries> accessed 16 Aug 2012.

Stern, C. & Stern, W. (1909). *Erinnerung, Aussage und Lüge in der ersten Kindheit.* Leipzig: Johann Ambrosius Barth.

Stern, W. (1893). *Die Analogie im volkstümlichen Denken; Eine psychologische Untersuchung.* Berlin: Salinger.

Stern, W. (1905). *Helen Keller. Die Entwicklung und Erziehung einer Taubstummblinden. Als psychologisches, pädagogisches und sprachtheoretisches Problem.* Berlin: Reuter & Reichard.

Stern, W. (1914). *Psychologie der frühen Kindheit.* Leipzig: Quelle & Meyer (fifth updated edition 1928).

Stern, W. (1935). *Allgemeine Psychologie auf personalistischer Grundlage.* The Hague: Nijhoff.

Stern-Michaelis, E. (1991). Erinnerungen an meine Eltern. In: W. Deutsch (Ed.), *Über die verborgene Aktualität von William Stern.* Frankfurt: Peter Lang, pp. 131–141.

Sternberg, S. (1969). On the discovery of processing stages. In: W.G. Koster (Ed.) *Attention and performance II.* Amsterdam: North-Holland, pp. 276–315.

Stetson, R.H. (1928). *Motor Phonetics. A study of speech movements in action. Archives Néerlandaises de Phonétique Expérimentale; 3.* Amsterdam: North-Holland (re-issued 1951).

Stevens, S.S. (Ed.) (1950). Proceedings of the Speech Communication Conference at MIT. *Journal of the Acoustical Society of America, 22,* 689–806.

Stevenson, A. (1893). The speech of children. *Science, 21,* 118–120.

Stinchfield, S.M. (1928). *The psychology of speech.* Boston, MA: Expression Company.

Stocking, G.W. (1968). *Race, culture, and evolution.* New York: Free Press.

Storch, E.W. (1901). *Psychologische Untersuchungen über die Funktionen der Hirnrinde, zugleich eine Vorstudie zur Lehre der Afasie.* Breslau: Habilschrift.

Stout, G.F. (1899). *A manual of psychology.* London: W.B. Clive.

Stout, G.F. (1902). *Analytic psychology.* 2 Vols. London: Allen & Unwin.

Strayer, L.C. (1930). Language and growth: the relative efficacy of early and deferred vocabulary training studied by the method of co-twin control. *Genetic Psychology Monographs, 8,* 209–319.

Stroop, J.R. (1935). Studies of interference in serial verbal reactions. *Journal of Experimental Psychology, 28,* 643–662.

Strümpell, L. (1880). *Psychologische Pädagogik*. Leipzig: Georg Böhme.

Stumpf, C. (1883, 1890). *Tonpsychologie*. 2 Vols. Leipzig: Hirzel.

Stumpf. C. (1891a). Psychologie und Erkenntnistheorie. *Abhandlungen der Königlich Bayerischen Akademie der Wissenschaften, 19*, 465–516.

Stumpf, C. (1891b). "Wundts Antikritik," *Zeitschrift für Psychologie und Physiologie der Sinnesorgane, 2*, 266–293.

Stumpf, C. (1901). Eigenartige Sprachentwicklung eines Kindes. *Zeitschrift für Pädagogische Psychologie und Pathologie, 3*, 419–447.

Stumpf, C. (1914). Über neuere Untersuchungen zur Tonlehre. *Bericht über den VI. Kongreß für experimentelle Psychologie*. Leipzig: Johann Ambrosius Barth, pp. 305–344.

Stumpf, C. (1926). *Die Sprachlaute; experimentell-phonetische Untersuchungen* (nebst einem Anhang über Instrumentalklänge), Berlin: Springer.

Sturtevant, E.H. (1940). Review of Koppelmann 1929. *Language, 16*, 235–237.

Sully, J. (1895). *Studies of childhood*. New York: Longmans, Green and Co.

Sully, J. (1897). Review of *The mental development of a child* by K. Carter Moore. *Mind, 6*, 408–412.

Sumby, W.H. & Pollack, I. (1954). Visual contribution to speech intelligibility in noise. *Journal of the Acoustical Society, 26*, 212–215.

Sütterlin, L. (1902). *Das Wesen der sprachlichen Gebilde. Kritische Bemerkungen zu W. Wundts Sprachpsychologie*. Heidelberg:Winter.

Taine, H. (1870/1914). *De l'intelligence*. 2 Vols. Paris: Hachette.

Taine, H. (1876.) De l'acquisition du langage chez les enfants et dans l'espece humaine. *Revue Philosophique, 1*, 23–32.

Taine, H. (1877). Acquisition of language by children. *Mind, 2*, 252–259.

Tarnovsky, V.M. (1867). Some forms of the syphilitic aphasia. *Voenno-Meditsynsky Jurnal (Military Medical Journal)* (in Russian), *100*, 225–296.

Taschwer, K. & Föger, B. (2003). *Konrad Lorenz. Biographie*. Vienna: Zsolnay Verlag.

Taylor, H.C. (1934). Social agreement on personality traits as judged from speech. *Journal of Social Psychology, 5*, 244–248.

Templin, M.C. (1957). *Certain language skills in children*. Minneapolis: University of Minnesota Press.

Terman, L.M. & Childs, H.G. (1912). A tentative revision and extension of the Binet–Simon Measuring Scale of Intelligence. *Journal of Educational Psychology*, 3, 61–74, 133–143, 198–208, 277–289.

Terman, L.M. & Miles (1936). *Sex and Personality*. New Haven, CT: Yale University Press.

Tesak, J. & Code, C. (2008). *Milestones in the history of aphasia. Theories and protagonists*. Hove: Psychology Press.

Tesnière, L. (1959). *Éléments de syntaxe structurale*. Paris: Klincksieck.

Thiele, R. (1928). Aphasie, Apraxie, Agnosie. In: O. Bumke (Ed.). *Handbuch der Geisteskrankheiten*, Vol. 2. *Störungen des Wollens, Handelns und Sprechens*. Berlin: Springer, pp. 243–341.

Thomas, W. (1907). The mind of women and lower races. *American Journal of Sociology, 12*, 435–469.

Thorndike, E.L. (1921). *The teacher's word book of 10,000 words*. New York: Bureau of Publications, Teacher's College, Columbia University.

Thorndike, E.L. (1932). *A Teacher's Word Book of the Twenty Thousand Words Found Most Frequently and Widely in General Reading for Children and Young People*. New York: Bureau of Publications, Teacher's College, Columbia University.

Thorndike, E. (1937). On the number of words of any given frequency of use. *Psychological Record, 1*, 399–406.

Thorndike, E.L. (1941). *The Thorndike-Century senior dictionary*. New York: Appleton-Century.

Thorndike, E.L. & Lorge, I. (1944). *The teacher's word book of 30,000 words.* New York: Bureau of Publications, Teachers College, Columbia University.

Thorson, A. (1925). The relation of tongue movements to internal speech. *Journal of Experimental Psychology, 8,* 1–32.

Thumb, A. & Marbe, K. (1901), *Experimentelle Untersuchungen über die psychologische Grundlagen der sprachliche Analogiebildung.* Leipzig: Engelmann. New edition edited by D.J. Murphy (1978). Amsterdam: Benjamins.

Thurstone, L.L. (1928). An experimental study of nationality preferences. *Journal of General Psychology, 1,* 405–425.

Thurstone, L.L. (1938). *Primary mental abilities.* Chicago: University Press.

Tiedemann, D. (1772). *Versuch einer Erklärung des Ursprungs der Sprache.* Riga: Johann Friedrich Hartnoch.

Tiedemann, D. (1787). Beobachtungen über die Entwicklung der Seelenfähigkeiten bei Kindern. Hessische Beiträge zur Gelehrsamkeit und Kunst. (Bd. II. S. 313 ff. S. 486 ff.). New edition by Chr. Ufer (1897). Altenburg: Oskar Bonde.

Tinker, M.A. (1928a). Eye movement duration, pause duration, and reading time. *Psychological Review, 35,* 385–397.

Tinker, M.A. (1928b). Numerals versus words for efficiency in reading. *Journal of Applied Psychology, 12,* 190–199.

Tinker, M.A. (1932/1980). Wundt's doctorate students and their theses 1875–1920. *American Journal of Psychology, 44,* 630–637. Reprinted in: W.G. Bringmann & R.D. Tweney (Eds.), *Wundt studies. A centennial collection.* Toronto: Hogrefe, pp. 269–279.

Tinker, M.A. (1936). Eye movement, perception, and legibility in reading. *Psychological Bulletin, 33,* 275–290.

Tinker, M.A. (1951). Fixation pause duration in reading. *Journal of Educational Research, 44,* 471–479.

Tinker, M.A. (1965). *Bases for effective reading.* Minneapolis: University of Minnesota Press.

Titchener, E.B. (1910). *A text-book of psychology.* New York: Macmillan.

Titchener, E.B. (1914). Psychology as the behaviorist views it. *Proceedings of the American Philosophical Society, 13,* 1–17.

Tomasello, M. (2003). *Constructing a Language: A Usage-Based Theory of Language Acquisition.* Cambridge, MA: Harvard University Press.

Tomasello, M. & Kaminski, J. (2009). Like infant, like dog. *Science,* 325, 1213–1214.

Toni, I., de Lange, F.P., Noordzij, M.L., & Hagoort, P. (2008). Language beyond action. *Journal of Physiology-Paris, 102,* 71–79.

Trabue, M.R. (1916). *Completion-test language scales.* New York: Teacher's College Contributions to Education No. 77.

Tracy, F. (1892). The language of childhood. *American Journal of Psychology, 6,* 107–138.

Tracy, F. & Stimpfl, J. (1894, 1909). *The psychology of childhood.* Boston: Heath & Co.

Traugott, N.N. (1935). The interrelations of immediate and symbolic projections in the process of the formation of conditioned inhibition. *Psychological Abstracts, 9,* 131, No. 1166.

Trautscholdt, M. (1883). Experimentelle Untersuchungen über die Association der Vorstellungen. *Philosophische Studien, 1,* 213–250.

Trousseau, A. (1865). Discussion sur les localisations cérébrales et l'aphasie. *Gazette Hebdomadaire de Médecine et de Chirurgie, 17,* 266–269.

Trubetskoy, N. S. (1929). Zur allgemeinen Theorie der phonologischen Vokalsysteme. *Travaux du Cercle Linguistique de Prague, 1,* 39–67.

Trubetskoy, N.S. (1939). *Grundzüge der Phonologie.* Göttingen: Van den Hoeck & Ruprecht.

Trussell, M. (1939). The diagnostic value of the verbal summator. *Journal of Abnormal and Social Psychology, 34,* 533–538.

Tsao, J.C. (1948). Studies in spaced and massed learning: II. Meaningfulness of material and distribution of practice. *Quarterly Journal of Experimental Psychology, 1*, 79–84.

Tsuru, S. & Fries, H. (1933). A problem in meaning. *Journal of General Psychology, 8, 281–284.*

Turing, A.M., (1939). Systems of logic based on ordinals. *Proceedings of the London Mathematical Society, Series 2, 45*, 161–228.

Tversky, B. (2005). Visuospatial reasoning. In: K. Holyoak and R. Morrison, (Eds.), *Handbook of Reasoning*. Cambridge: Cambridge University Press, pp. 209–249.

Tylor, E.B. (1881). *Anthropology: An Introduction to the Study of Man and Civilization*. New York: Appleton.

Underwood, B.J. & Hughes, R.H. (1950). Gradients of generalized verbal responses. *American Journal of Psychology, 63*, 422–430.

Usnadze, D. (1924). Ein experimenteller Beitrag zum Problem der psychologischen Grundlagen der Namengebung. *Psychologische Forschung, 5*, 24–43.

Vander Beke, G.E. (1935). *French word book*. New York: Macmillan.

Van der Stroom, G. (1995). *The reception of van Ginneken's language biology*. Amsterdam: Stichting Neerlandistiek VU.

Van Ginneken, J. (1905–1906). *Grondbeginselen der psychologische taalwetenschap*. 2 Vols. Lier: Jozef van In & Cie.

Van Ginneken, J. (1907). *Principes de linguistique psychologique. Essai synthétique*. Amsterdam: Van der Vecht, Rivière & Harrassowitz.

Van Ginneken, J. (1914). *Handboek der Nederlandse taal*. 2 Vols. Nijmegen: Malmberg.

Van Ginneken, J. (1917). *De roman van een kleuter*. Nijmegen: Malmberg.

Van Ginneken, J. (1932). *De ontwikkelingsgeschiedenis van systemen der menschelijke taalklanken*. Amsterdam: North-Holland Uitgevers-Maatschappij.

Van Ginneken (1934). La biologie du langage. *Comte rendu de la première session du Congrès International des Sciences Anthropologiques et Ethnologiques*. London, pp. 320–322.

Van Ginneken, J. (1935). *Ras en taal*. Amsterdam: North-Holland Uitgevers-Maatschappij.

Van Ginneken, J. (1939). La reconstruction typologique des langues archaïques de l'humanité. *Verhandelingen der Koninklijke Nederlandsche Akademie van Wetenschappen. Nieuwe Reeks XLIV*, 1–182.

Van het Reve, K. (2008). *Verzameld werk*. Vol. I. Amsterdam: Van Oorschot.

Van Woerkom, W. (1921). La signification de certains éléments de l'intelligence dans la genèse des troubles aphasiques. *Journal de Psychologie Normale et Pathologique, 18*, 730–751.

Van Woerkom, W. (1931). Psychopathologische Beobachtungen bei Stirnhirngeschädigten. *Monatschrift für Psychiatrie und Neurologie, 80*, 274–331.

Velten, H.V. (1943). The growth of phonemic and lexical patterns in infant language. *Language, 19*, 281–292.

Vernon, M.D. (1930). An apparatus for the photographic recording of eye movements. *British Journal of Psychology. General Section, 21*, 64–67.

Vernon, M.D. (1931a). *The experimental study of reading*. Cambridge: Cambridge University Press.

Vernon, M.D. (1931b). Characteristics of proof-reading. *British Journal of Psychology, 21*, 368–381

Vernon, M.D. (1971). *Reading and its difficulties: a psychological study*. Cambridge: Cambridge University Press.

Vierordt, K. (1879). Die Sprache des Kindes. *Deutsche Revue, 3*, 26–49.

Villiger, E. (1911). *Sprachentwicklung und Sprachstörungen beim Kinde. Leipzig:* Engelmann.

Vygotsky, L. S. (1962). *Thought and language*. Cambridge, MA: MIT Press. Edited translation by E. Hanfmann & G. Vakar, of the 1934 Russian original.

Vygotsky, L.S. & Luria, A. (1929). The function and fate of egocentric speech. *Proceedings of the Ninth International Congress of Psychology. (New Haven, 1929)*. Princeton: Psychological Review Company, pp. 464–465.

Wagner, J. (1918). Experimentelle Beiträge zur Psychologie des Lesens. *Zeitschrift für Psychologie, 80*, 1–75.

Warneken, F. & Tomasello, M. (2006). Altruistic helping in human infants and young chimpanzees. *Science, 311*, 1301.

Washburn, M.F. (1916). *Movement and mental imagery*. Boston: Houghton Mifflin.

Washburn, M.F. (1922). Introspection as an objective method. *Psychological Review, 29*, 89–112.

Watson, J.B. (1913). Psychology as the behaviorist views it. *Psychological Review, 20*, 158–177.

Watson, J.B. (1914). *Behavior. An introduction to comparative psychology*. New York: Henry Holt.

Watson, J.B. (1920). Is thinking merely the action of language mechanisms? *British Journal of Psychology, 11*, 87–104.

Watson, J.B. & Rayner, R. (1920). Conditioned emotional responses. *Journal of Experimental Psychology, 3*, 1–14.

Wawrowska, W. (1938). Badania psychologiczne nad rozwojem mowy dzieca (od 1,0-2,3 lat) [Psychological study of child language development (from age 1,0 to 2,3)] In: S. Szuman (Ed.), *Prace psychologiczne*. Warsaw: Nankowe Towarzystwa Pedagogicznego.

Wedgwood, H. (1866). *On the origin of language*. London: N. Trübner and Co.

Wegener, P. (1885). *Untersuchungen über die Grundfragen des Sprachlebens*. Halle: Max Niemeyer.

Wegener, P. (1920). Der Wortschatz. *Indogermanische Forschungen, 39*, 1–26.

Weinreich, U. (1954). *Languages in contact*. New York: Linguistic Circle of New York.

Weinreich, U. (1958). Travels through semantic space. *Word, 14*, 346–366.

Weisenburg, T. & McBride, K. (1935). *Aphasia. A cilinical and psychological study*. New York: Hafner Publishing Company.

Weisgerber, L. (1929). *Muttersprache und Geistesbildung*. Göttingen: Vandenhoeck & Ruprecht.

Weisgerber, L. (1941). Sprachgeschichte und Volksgeschichte. In: F. Maurer (Ed.). *Von deutscher Art in Sprache und Dichtung*. Stuttgart: Kohlhammer, pp. 3–42.

Weisgerber, L. (1950). *Vom Weltbild der deutschen Sprache*. Düsseldorf: Pädagogischer Verlag Schwann.

Weiss, A.P. (1917a). Relation between structural and behavior psychology. *Psychological Review, 24*, 301–317.

Weiss, A.P. (1917b). Relation between functional and behavior psychology. *Psychological Review, 24*, 353–368.

Weiss, A.P. (1918). Conscious behavior. *Journal of Philosophy, Psychology, and Scientific Methods, 15*, 631–641.

Weiss, A.P. (1925a). One set of postulates for a behavioristic psychology. *Psychological Review, 32*, 83–87.

Weiss, A.P. (1925b). Linguistics and psychology. *Language, 1*, 52–57. *Psychological Review, 24*, 353–368.

Weiss, A.P. (1925c, 1929 second edition). *A theoretical basis of human behavior*. Columbus, Ohio: R.G. Adams.

Welborn, E.L. & English, H. (1937). Logical learning and retention: a general review of experiments with meaningful verbal materials. *Psychological Bulletin, 34*, 1–20.

Welby, V. (1911). *Significs and language: The articulate form of our expressive and interpretative resources*. London: Macmillan.

Weld, H.P. (1917). Meaning and process as distinguished by the reaction method. In: J.W. Pillsbury, J.W. Baird, & M.F. Washburn (Eds.), *Studies in psychology. Contributed by colleagues and former students of Edward Bradford Titchener*. Worcester, MA: L.N. Wilson, pp. 181–206.

Wellman, B.L., Case, I.M., Mengert, I.G., & Bradbury, D.E. (1931). *Speech sounds by young children*. Iowa City: University of Iowa Press.

Wells, F.L. (1906). *Linguistic lapses. With especial reference to the perception of linguistic sounds.* Columbia University contributions to philosophy and psychology. Vol. 14, No. 3. New York: The Science Press.

Wells, R. (1951). Predicting slips of the tongue. *Yale Scientific Magazine, 26,* 9–30. Reprinted in V. Fromkin (Ed.) (1973). *Speech errors as linguistic evidence.* The Hague: Mouton, pp. 82–86.

Wepman, J.M. (1950). *Recovery from aphasia.* New York: Ronald Press Company.

Werner, H. (1919). *Die Ursprünge der Metapher.* Leipzig: Engelmann.

Werner, H. (1932). *Grundfragen der Sprachphysiognomik.* Leipzig: Johann Ambrosius Barth.

Werner, H. (1940). *Comparative psychology of mental development.* New York: Harper.

Werner, H. & Kaplan, E. (1950). Development of word meaning through verbal context: An experimental study. *Journal of Psychology, 29, 251–257.*

Werner, H. & Kaplan, B. (1963). *Symbol formation: An organismic-developmental approach to language and the expression of thought.* New York: John Wiley & Sons.

Wernicke, C. (1874). *Der aphasische Symptomencomplex. Eine psychologische Studie auf anatomischer Basis.* Breslau: Max Cohn & Weigert. Also reproduced in Wernicke (1892/2006).

Wernicke, C. (1883a). Ein Fall von Taubheit in Folge von doppelseitiger Laesion des Schäfellappens. *Fortschritte des Medicin, 20,* 177–185. Reproduced in Wernicke (1892/2006), pp. 199–208.

Wernicke, C. (1883b). Replik auf die vorstehende Entgegnung. *Fortschritte des Medicin, 20,* 313–316.

Wernicke, C. (1884). Über die motorische Sprachbahn und das Verhältniss der Aphasie zur Anarthrie. Reproduced in Wernicke (1892). *Gesammelte Aufsätze und kritische Referate zur Pathologie des Nervensystems.* Facsimile reproduction (2006). Saarbrücken: VDM Verlag, pp. 71–91.

Wernicke, C. (1885/6). Einige neue Arbeiten über Aphasie. Reproduced in Wernicke (1892). *Gesammelte Aufsätze und kritische Referate zur Pathologie des Nervensystems.* Facsimile reproduction (2006). Saarbrücken: VDM Verlag, pp. 92–129.

Wernicke, C. (1903). Der aphasische Symptomencomplex. 13. Vorlesung, *Die Deutsche Kliniek, 6,* 487–556.

Wertheimer, M. & Klein, J. (1904). Psychologische Tatbestandsdiagnostik. *Archiv fur Kriminalanthropologie und Kriminalistik, 15,* 72–113.

West Loring, M. (1918). Methods of studying controlled word associations. *Psychobiology, 1,* 368–428.

Whipple, G. M. (1908). Vocabulary and word-building tests. *Psychological Review, 15, 94–105.*

Whispé, L.G. & Drambarean, N.C. (1953). Physiological need, word frequency, and visual duration threshold. *Journal of Experimental Psychology, 46,* 25–31.

White, W. (1925). Primitive mentality and the racial unconscious. *American Journal of Psychiatry, 4,* 663–671.

Whitney, D. (1872). Steinthal on the origin of language. *North American Review, 114,* 272–308.

Whorf, B.L. (1937). Grammatical categories. Reproduced in J.B. Carroll (Ed.) (1956), *Language, thought, and reality. Selected writings of Benjamin Lee Whorf.* Cambridge, MA: MIT Press, pp. 87–101.

Whorf, B. (1941) Languages and logic. Reproduced in J.B. Carroll (Ed.) (1956), *Language, thought, and reality. Selected writings of Benjamin Lee Whorf.* Cambridge, MA: MIT Press, pp. 233–245.

Whorf, B. (1956). *Language, thought and reality. Selected writings of Benjamin Lee Whorf.* J.B. Carroll (Ed.). Cambridge, MA: MIT Press.

zu Wied, M. (1841). *Reise in das Innere von Nordamerika, 1832–1834.* Vol. II, 645–653. Koblenz: Rhenania.

Wiegand, C.F. (1908). Untersuchung über die Gestaltqualität für die Erkennung von Wörtern. *Zeitschrift für Psychologie, 48,* 161–237.

Wiener, N. (1948). *Cybernetics: Or control and communication in the animal and the machine.* New York: John Wiley.

Wilkins, J. (1668). *An essay towards a real character and a philosophical language.* London: Gellibrand.

Williams, B. (1940). A note on the statistical analysis of sentence-length as a criterion of literary style. *Biometrika, 31*, 356–361.

Williams, H.M. (1937). An analytical study of language achievement in preschool children. *University of Ohio Studies in Child Welfare, 13*: Nr. 2, Part 1, 9–18.

Wiltse, S.E. (1888). II.- Experimental. *American Journal of Psychology, 1*, 702–705.

von Winterfeld, M.A. (1789, 1791). Tagebuch eines Vaters über sein neugebohrenes Kind. *Braunschweigisches Journal philosophischen, philologischen und pädagogischen Inhalts.* (1789) article 4, 404–441; (1791) article 6, 476–484.

Wirth, W. (1902). Zur Theorie des Bewußtseinsumfanges und seiner Messung. *Philosophische Studien, 20*, 487–669.

Wissemann, H. (1954). *Untersuchungen zur Onomatopiie.* 1. Teil. *Die sprachpsychologischen Versuche.* Heidelberg: Carl Winter Universitätsverlag.

Witasek, B. (1907). Über Lesen und Rezitieren in ihren Beziehungen zum Gedächtnis. *Zeitschrift für Psychologie, 44*, 161–185, 246–282.

Wittgenstein, L. (1921). *Logisch-Philosophische Abhandlung.* In: W. Ostwald (Ed.), *Annalen der Naturphilosophie, 14.*

Wittmann, J. (1934). Die physiognomische Urbedeutung des Wortes und das Problem der Bedeutungsentwicklung. In: O. Klemm (Ed.). *Bericht über den XIII. Kongreß der Deutschen Gesellschaft für Psychologie in Leipzig vom 16.–19. Oktober 1933.* Jena: Gustav Fischer, pp. 187–190.

Wolf, O. (1871). *Sprache und Ohr. Akustisch-physiologische und pathologische Studien.* Braunschweig: Vieweg.

Wolf, O. (1873). Neue Untersuchungen über Hörprüfung und Hörstörungen. *Archiv für Augen- und Ohrenheilkunde, 3*, 35–55.

Wolfle, D.L. (1932). The relation between linguistic structure and associative interference in artificial linguistic material. *Language, 8.* Language Monographs, Whole No. 11.

Wolfle, D.L. (1934). The rôle of generalization in language. *British Journal of Psychology, 24*, 434–444.

Woodrow, H. & Lowell, F. (1916). Children's association frequency tables. *Psychological Monographs, 22*, 5. Whole No. 97.

Woodworth, R.S. (1938). *Experimental psychology.* New York: Holt.

Wozniak, R.H. (1994). Albert Weiss and *A theoretical basis of human behavior.* Introductory chapter to A.P. Weiss, *A theoretical basis of human behavior.* New edition. London: Routledge, pp. V–XXII.

Wozniak, R.H. (1997a). http://www.brynmawr.edu/psychology/rwozniak/meyer.html

Wozniak, R.H. (1997b). http://www.brynmawr.edu/psychology/rwozniak/weiss.html

Wright-Sutherland, S. (1988). The forgotten research of Miles Albert Tinker. *Journal of Visual Literacy, 9*, 10–25.

Wundt, W. (1874). *Grundzüge der physiologischen Psychologie.* Leipzig: Engelmann.

Wundt, W. (1880). *Logik.* 2 Vols. Stuttgart: Enke.

Wundt, W. (1883). Ueber psychologische Methoden. *Philosophische Studien, 1*, 1–38.

Wundt, W. (1886). *Grundriss der Psychologie.* Leipzig: Kröner.

Wundt, W. (1900a). *Die Sprache.* 2 Vols. Leipzig: Engelmann.

Wundt, W. (1900b). Zur Kritik tachistoscopischer Versuche. *Philosophische Studien, 15*, 287–317.

Wundt, W. (1900c). Zur Kritik tachistoskopischer Versuche. Zweiter Artikel. *Philosophische Studien, 16*, 61–70.

Wundt, W. (1901a). (Review of) Thumb A. und Marbe K. Experimentelle Untersuchungen über die psychologischen Grundlagen der sprachlichen Analogiebildungen. Leipzig. Engelman 1901. *Flensburg Studien auf dem Gebiete der idg. Wurzelbildung und Altertumskunde, 12*, 17–20.

Wundt, W. (1901b). *Sprachgeschichte und Sprachpsychologie. Mit Rücksicht auf B. Delbrücks "Grundfragen der Sprachforschung".* Leipzig: Engelmann.

Wundt, W. (1907a). Über Ausfrageexperimente und über die Methoden zur Psychologie des Denkens. *Psychologische Studien, 3,* 301–360.

Wundt, W. (1907b). Letter to Oswald Külpe. In: Wontorra, M., Meischner-Metge, A. & Schröger, E. (Eds.) (2004). *Wilhelm Wundt (1832–1920) und die Anfänge der experimentellen Psychologie. Jubiläumsausgabe zur 125-Jahr-Feier seiner Institutsgründung.* CDROM 2007.

Wundt, W. (1908). Kritische Nachlese zur Ausfragemethode. *Archiv für die gesamte Psychologie, 11,* 445–459.

Wundt, W. (1914). *Grundriss der Psychologie.* (Zwölfte Auflage). Leipzig: Kröner.

Wundt, W. (1920). *Erlebtes und Erkanntes.* Leipzig and Stuttgart: Kröner.

Yule, G.U. (1939). On sentence-length as a statistical characteristic of style in prose: With application to two cases of disputed authorship. *Biometrica, 30,* 363–390.

Yule, G.U. (1944). *The statistical study of literary vocabulary.* Cambridge: Cambridge University Press.

Zangwill, O.L. (1954). Agraphia due to left parietal glioma in a left-handed man. *Brain, 77,* 510–520.

Zangwill, O.L. (1960). *Cerebral dominance and its relation to psychological function.* Edinburgh: Oliver and Boyd.

Zeitler, J. (1900). Tachistoscopische Untersuchungen über das Lesen. *Philosophische Studien, 16,* 380–463.

Zipf, G.K. (1929). Relative frequency as a determinant of phonetic change. *Harvard Studies in Classical Philology, 40,* 1–95.

Zipf, G.K. (1932). *Selected studies of the principle of relative frequency in language.* Cambridge, MA: Harvard University Press.

Zipf, G.K. (1935). *The psycho-biology of language.* Cambridge, MA: MIT Press.

Zipf, G.K. (1941). *National unity and national disunity.* Bloomington: Principia Press.

Zipf, G.K. (1943). Children's speech. *Science, 96,* 344–345.

Zipf, G.K. (1946). The psychology of language. In: P.L. Harriman (Ed.). *Encyclopedia of psychology.* New York: Philosophical Library, pp. 332–341.

Zipf, G.K. (1949). *Human behavior and the principle of least effort.* Cambridge, MA: Addison-Wesley Press.

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