A Survey of Psycholinguistic Research, 1954-1964

by A. Richard Diebold, Jr., Harvard University

The discursive survey which follows is an attempt to give what we might call an "intellectual history" of psycholinguistics, with special emphasis on developments in the field during the past ten years. It is not easy to define the term "psycholinguistics," which represents a miscellany of theoretical and experimental approaches to certain aspects of human language and verbal behavior. As we shall see, the reasons are rather tenuous for claiming that there actually is a science of psycholinguistics separable from the established disciplines from which it has evolved. Nevertheless, there does exist a range of research activities (and publications deriving from them) which linguists and other behavioral scientists agree in labelling "psycholinguistic," indicating that there is a special niche in psychology for linguistically-oriented research, and conversely in linguistics for psychologically-oriented research on language and verbal behavior.

Before proceeding to a more concerted attempt to characterize the field, I will offer as a broad working definition the one given by Osgood and Sebeok on page 4 of the present volume, viz., that psycholinguistics "is concerned in the broadest sense with relations between messages and the characteristics of human individuals who select and interpret them." The intellectual history in which Osgood and Sebeok's monograph has played a central role is an especially interesting one for linguistics, since it reflects a minor revolution within the history of linguistic theory as a whole. The need for such a revolution was early argued by Sapir:

It is peculiarly important that linguists, who are often accused, and accused justly, of failure to look beyond the patterns of their subject matter, should become aware of what their science may mean for the interpretation of human conduct in general. Whether they like it or not, they must become increasingly concerned with the many anthropological, sociological, and psychological problems which invade the field of linguistics (Sapir, 1929:214).

It is an easy task to document just such an "invasion" of linguistics as Sapir envisioned, by reviewing some of the problems proposed by behavioral scientists, engineers, philosophers, educators, and others. There has been no lack of interest in essentially linguistic problems on the part of scholars and laymen outside the ranks of linguists. Unfortunately, many of the problems posed have involved aspects of language and verbal behavior which linguists were poorly equipped to handle, or which they regarded with considerable disinterest. Much of the research ancestral to contemporary psycholinguistics thus evinces a striking lack of contact with linguistics, and the want of linguistic sophistication which is sometimes betrayed is no doubt partly a response of despair or estrangement.
on the part of outsiders who sought linguists' views. This is certainly the situation which has obtained in the United States, where the activity we would now call psycholinguistic grew up on the periphery of linguistics.

Why is there a field known as psycholinguistics? The best answer to this question is that linguists traditionally have been remarkably narrow in their purview of language. Some of the many topics which linguists have disdained as "irrelevant" or "uninteresting," and which they have relegated to the margins of their discipline, have developed into separate fields of inquiry. The reason for this is not far to seek. Especially if we focus our attention on descriptive linguistics and linguistic theory, it is clear that linguists have been primarily concerned with *langue*, in the sense that de Saussure used this term to refer to the abstract linguistic system underlying verbal behavior. Thus, within the broad definition of psycholinguistics offered above, linguists have been concerned at most with the descriptive characterization of the "messages" and with constructing abstract models which can be devised to account for their structure.

Traditionally, linguistics has not concerned itself as much with *parole*, that which de Saussure regarded as the overt, actualized aspect of verbal behavior, let alone with the relation of particular messages to each other, or to "the characteristics of human individuals who select and interpret them," or to the social and cultural matrix in which they are produced. Yet it is precisely the relations between messages and the characteristics of their users which have been of primary concern to the psychologist, and the relation of these to the social and cultural matrix are of concern not only to the psychologist but to the anthropologist and sociologist as well. Thus an argument could be advanced that linguistic theory has been almost exclusively involved with the formal accounting of *langue*, and has been vigorously anti-reductionist. Support for this argument could be mustered by examining the curiously marginal status accorded phonetics and semantics in much of linguistic theory, especially before widespread psycholinguistic activity began.

A proper appreciation of these basic differences in focus will explain some of the communication gaps which still separate linguistics from other behavioral sciences interested in verbal behavior. It is only strange that these differences in emphasis have been missed by psychologists, "who have long realized that a description of what an organism does and a description of what it knows can be very different things" (Chomsky, 1963:326). The full explanation for this misunderstanding must be sought in the history of psychological theory, a venture which will not be undertaken in this review, save by occasional oblique reference to the impact of behaviorism on psycholinguistic research. The contemporary deemphasis of extreme behaviorism in psychology is correlated with rapid developments in the investigation of the cognitive and neurophysiological aspects of language and verbal behavior; this is no more an accident,
I believe, than the prior association of Bloomfieldian linguistics (with its mechanistic concern for discovery procedures) and a general behavioristic approach in psychology, emphasizing readily observed and controlled stimulus-input and response-output sequences.

The events which have freed the psychologist to become interested in langue deserve recounting. Parts of this history have already been written (e.g., Alkon, 1959), but a non-partisan overview remains the most pressing epistemological need in psycholinguistics. The current results of this reorientation—which psychologists and linguists of a former generation would have concurred in calling “mentalistic”—will be a major theme in the present survey. It is interesting, and indicative of the positive impact of psycholinguistics on the theory of linguistic structure, that the most recent theoretical writings now evince a concern for the congruence of linguistic with psychological models. Linguists and psychologists, in a quest for corroboration of their theories, often reduce from linguistic to psychological findings, and vice versa. I will return to give many examples of this trend.

By the 1940s it was apparent in the United States that the attention of linguists was being attracted to certain developments in other behavioral sciences. Lévi-Strauss, writing in 1945 on the developing liaison between anthropology and linguistics, even spoke of the noblesse oblige enjoining linguistics to monitor work in adjoining fields where others were attempting to apply their methods and results (Lévi-Strauss, 1945).

Although “psycholinguistics” has been talked about since 1950 (the term itself appeared somewhat earlier, in the 1940s), it is evident that the jelling of the field has come about only recently. Its earlier internal diversity is reflected in the journals in which its practitioners published, according to their affiliations as psychologists, communications engineers, acousticians, or whatever. Unlike linguistics proper, with its many long-established journals and serial publications, psycholinguistics has had to publish its writings through such disparate organs as the Journal of Abnormal and Social Psychology, and the Journal of the Acoustical Society of America, to mention only two. More recently, to be sure, the British journal, Language and Speech, and several rarely perused German and Russian periodicals have supplied a more specialized outlet. Moreover, several American serial publications (most notably the Annual Review of Psychology and The Psychological Bulletin² have long provided some coverage of ongoing psycholinguistic research, although not always under that name. With the advent of the new Journal of Verbal Learning and Verbal Behavior, we can hope for a gathering together of some of the far-flung results of earlier effort (in spite of the monopoly, in the first issues of this journal, of papers relating to operant conditioning of verbal behavior). It is the jelling of diverse earlier efforts which is the focal point of this intellectual history.

Many histories have their particular turning points, often marked by
monuments which thereafter become charters for future developments. Psycholinguistics is no exception. In the early 1950s, the Social Science Research Council formed a Committee on Linguistics and Psychology. In the summer of 1953, after earlier occasional meetings of the Committee, the Council sponsored the Summer Seminar on Psycholinguistics, which brought many of the Committee members together at Indiana University. The exact purposes and membership of that gathering have been stated in the foreword and preface of this volume. The monograph herein, *Psycholinguistics: A Survey of Theory and Research Problems* (1954), resulted from this seminar. Within a year or two of its appearance, this monograph became the charter for psycholinguistics, firmly establishing the discipline’s name. It so successfully piqued the interest of linguists and other behavioral scientists that the volume itself was soon out of print, and also became notoriously difficult to obtain second-hand, or even in libraries. For these reasons alone, the decision of the Indiana University Press to reprint the monograph was a happy one.

Perhaps as an outgrowth of the interest aroused by the 1953 seminar and the 1954 survey, several other psycholinguistic projects were subsequently sponsored by the Social Science Research Council. Among the most notable of these is the Southwest Project in Comparative Psycholinguistics. The activities of this research project have been discussed in various preliminary reports by Casagrande (1956, 1960). Carroll has prepared a bibliography, complete as of February 1962, which lists 26 publications resulting directly from this research (Carroll, 1962).

Another extremely important development was the appearance in 1961 of a long-awaited reader, *Psycholinguistics: A Book of Readings*, edited by Sol Saporta. The Saporta reader reflects an awareness that the field of psycholinguistics now engages many personnel and possesses an extensive literature; it is especially welcome when one considers the scattered nature of the relevant writings. It is also a testament to the fact that there is an ever-growing number of university courses variously titled “psychology of language,” “psycholinguistics,” “linguistic psychology,” etc.

In my survey of recent research in the field of psycholinguistics, I will use as a framework the categories and the contents of Saporta’s reader. Although the categorization is somewhat arbitrary, these categories represent the various topical subfields implicitly recognized as constituting the subject matter of psycholinguistics. My goal will be to examine the readings contained in Saporta’s book as exemplars of the types of research and thinking that have been done in each of these subfields. The topical sections used by Saporta are as follows:

1. The nature and function of language.
2. Approaches to the study of language.
4. The sequential organization of linguistic events.
5. The semantic aspects of linguistic events.
6. Language acquisition, bilingualism, and language change.
7. Pathologies of linguistic behavior.
8. Linguistic relativity and the relation of linguistic processes to perception and cognition.

Cursory perusal of the sections will suggest some areas of omission, "areas" here being taken to mean subfields within psycholinguistics which are at least distinguished by having their own associated bodies of literature. In Section 9, three such areas are discussed (a) mass communication, (b) non-verbal communication, and (c) "zoosemiotics," the term recently proposed for "the discipline, within which the science of signs intersects with ethology, devoted to the scientific study of signalling behavior across animal species" (Sebeok, 1963:465).

Too strict an adherence to these topical categories would make it difficult to discuss developmental trends which are affecting psycholinguistics as a whole. Thus, whenever it has seemed important to examine such a trend, I have done so at the expense of continuity. This is one respect in which the present survey departs from my earlier study (1964b) on which it is based. Another departure is the decreased concern with the pedagogical value of the particular selections contained in Saporta's reader. In evaluating the selections, I have used instead, as a basic criterion, the extent to which a particular paper offers a truly representative sampling of the research activities associated with that subtopic. Finally, my earlier review concentrated on studies which were available to Saporta when his reader was in its final stages of compilation (i.e., up to 1960). The present survey covers many more recent materials which have appeared since the publication of the Saporta reader.

In the course of thus characterizing the field by surveying the activities being conducted within it, I have also consulted other survey studies which have either attempted to delimit the field of psycholinguistics as a whole or which have surveyed particular subareas.

1. The Nature and Function of Language

This first part of Saporta's reader contains two papers, J. Lotz's "Linguistics: symbols make man" (1956) and F. W. Householder's "On linguistic terms," based on his "On linguistic primes" (1959). Both authors deal with what language "is" is terms of what the linguist is principally interested in. The Householder paper, a most useful inclusion, acquaints the novice with various live issues in descriptive linguistics and linguistic theory. It is explicit about differences in approach, yet it treats this variation within one framework. In this respect, it compares favorably with a parallel paper written a decade ago by Haugen (1951).

What emerges immediately from these papers is that descriptive linguistic analysis has traditionally approached a language (usually a small sample of it) as a closed system, one in which it is always possible to segment the physical continua which constitute speech samples into the
"primes" and "mapping units" discussed by Householder. As will become apparent in Parts 3 and 4, the perception of primes by speakers of the language and the mechanics of their combination pose key problems for psycholinguistics.

But what should have been made apparent at the beginning, either by editorial comment or by a representative paper, are some of the basic problems posed by the primes themselves. I will mention three such problems here, the first and second are related. (1) Are these primes true universals of language? (2) And, if so, what sort of psychological reality have they for the speakers of a particular language? Householder addresses the first of these questions, assuring his readers that the "phoneme" and the "morpheme" (actually the "word") are true universals, and so too is the structure (the "mapping units"), in the sense that there are always stateable constraints on the random occurrence of the primes. By careful reading of the Lotz and Householder papers together, the newcomer may infer from various remarks that these primes have some sort of reality outside the mind and methods of the analyst. They do not provide a sufficient introduction to either language universals or to psychological reality, two crucial topics in contemporary research on language and verbal behavior.

Questions about language universals and language typology have been paired in recent decades, especially now that new trends and interests in the study of language (such as psycholinguistics) have revived a flagging concern with typology. It is significant that the Committee on Linguistics and Psychology of the Social Science Research Council (SSRC) recently organized a Conference on Language Universals, the proceedings of which have just been published (see Greenberg, 1963).

Furthermore, revisions in our views of grammatical theory incorporate a concern with universals as a major factor in the justification of grammatical models (e.g., Katz and Postal, 1964, esp. 159 ff.). The last few years have also seen the rise to prominence of a descriptive methodology which offers as yet unevaluated but pedagogically highly promising results; this is the so-called "contrastive grammar," which typically compares the structural similarities and differences between a target and source language. For theoretical purposes, continued pursuit of contrastive analyses will greatly increase our knowledge of language universals and typology, and the efforts in this direction by the Center for Applied Linguistics (which has been responsible for much activity in this area) should be encouraged.

The introductory papers in Saporta's reader seem to overlook the fact that it is not primarily the universality of a prime, like the phoneme, that is of interest, but rather the implicative universals relating to its perception, its feature composition, its sequential constraints, and other factors. Jakobson and Halle's paper, covered in Part 6, provides some examples of what is needed for an introduction to the study of univer-
sals. Many of these "near-universals" involve structural relationships which co-occur in diverse natural languages with more than chance frequency, and which therefore demand explanation and consideration by an adequate theory of language. The fact that many of these typological correlations must be statistically determined leads to another general problem for "the nature and function of language":

(3) The third basic problem posed by the existence of linguistic primes relates to their quantification and their incorporation into structural models. The most salient characteristic of linguistic primes themselves, in contrast to the physical continua which contain them, is their discreteness, their discontinuity. It is in this sense that linguistic units lend themselves so well to qualification (e.g., by statistical frequency) and to model-building, since they invite substitution into quantum mathematics. Moreover, it is obviously their quantitative attributes which distinguish linguistic primes from the species units of other behavioral sciences: e.g., the "culture trait" of anthropology, or the "response" of behaviorist psychology. This may explain why these other behavioral sciences have come to be very impressed with the operational procedures for analysis and with the techniques of descriptive taxonomy practiced by linguists. The "segmentability" and the quantum mechanics available to linguists strikes these other practitioners as very rigorous, and unattainable with the species units in their own fields.

The 1954 survey, especially in the sections on information theory written by Wilson and on entropy measures by Wilson and Carroll, presaged an important future role for mathematics in psycholinguistic research. Now, a decade later, with the appearance of such studies as those by Chomsky and Miller in the recent Handbook of Mathematical Psychology (Luce, Bush, and Galanter, 1963), it is obvious that the student seriously interested in psycholinguistics will have to master statistics and finite mathematics.

Such knowledge will be necessary in order to properly grasp the import of many relevant studies in which statistics is a basic tool for investigation and means for presentation. Applied statistics has supplied us with much raw data, some of which is still being analyzed; the Thorndike-Lorge frequency list (1927) is but one example. Zipf's investigations with rank-frequency distributions (1932, 1949) and Mandelbrot's (1954, 1961) papers interpreting Zipf are frequently cited examples of "linguistic statistics." Herdan's *Type-token Mathematics* (1960) is a good introductory guide to the application of statistical procedures to linguistic problems, and contains a useful annotated glossary of statistical terminology. Particular applications of statistics to psycholinguistic problems are found in Miller and Chomsky, 1963.

A knowledge of finite mathematics will help avoid a danger well posed by Plath: "One of the greatest dangers involved in the construction of mathematical models of language, particularly quantitative ones, is that
indiscriminate introduction of mathematical apparatus inevitably leads to the generation of meaningless and misleading results” (1961:22,23). While the danger of obtaining misleading results is certainly a real one, there is also a growing body of meaningful and heuristic results which are so couched in mathematical proofs and symbolic logic, that ever more mathematical competence is demanded of the student who would understand them. For a full appreciation of this warning, let the mathematically unsophisticated reader attempt to follow the arguments contained in several of Chomsky’s important papers (most notably those of 1956 and 1963); without some knowledge of mathematics, the task is insuperable.

Perhaps the major shortcoming of this introductory section of the psycholinguistics reader, like the 1954 survey, is its failure to clarify the distinction that must be made between a model which deals with linguistic competence alone and one which deals with various behavioral manifestations of that competence, such as language acquisition, speech perception, and speech production. In fairness to the collaborators of the 1954 survey, it must be stressed that their efforts occurred before the distinction had been dramatically clarified in linguistics by the emergence of a theory of language structure called “generative grammar.” This is one of the revolutionary developments which separates the intellectual climate of psycholinguistics today from that of 1954.

This difference in focus is one which psychologists might clarify by distinguishing between linguistic competence and linguistic performance, and linguists through the Saussurean dichotomy between langue and parole, referred to earlier. There can be little doubt that losing sight of this distinction has brought about a myopia which has contributed to communication gaps, not only between linguists and psychologists, but also between internal divisions within their respective ranks. Consider the possibilities for misunderstanding between strict behaviorists (in both linguistics and psychology), chiefly interested in linguistic performance, and their colleagues who are more concerned with the unobservable cognitive structures which constitute linguistic competence. There are unfortunately few discussions of the nonequivalence of linguistic competence and performance; Katz and Postal (1964) and especially Postal (1964a) contain interesting statements, as do Chomsky and Miller (1963) and Chomsky (1963).

2. Approaches to the Study of Language

Reading through the important papers in this second part of Saporta’s reader, it is easier to appreciate the diversity of thought within linguistics than to perceive any common denominator. The section includes: L. Bloomfield, “A set of postulates for the science of language” (1926); two chapters from N. Chomsky, Syntactic Structures (1957); C. F. Hockett’s review of C. L. Shannon and W. Weaver, The Mathematical Theory of Communication (1953); B. F. Skinner’s introductory chapter to his
Verbal Behavior (1957); and L. Krasner, "Studies of the conditioning of verbal behavior" (1958).

We might ask why Bloomfield's article was included, or, why it was included without editorial comment. In the first part of this paper, Bloomfield is very terse about the exclusion of psychology from any of the postulates which follow. Present-day students not better acquainted with Bloomfield's writings will seize this as confirmation of the overly mechanistic approach to language which it is now fashionable to attribute to him. To be sure, Bloomfield was interested in developing rigorous discovery procedures and historical techniques, at a time when too few existed. And connections do exist between these endeavors of Bloomfield and some latter-day traditions of descriptive analysis which have been legitimately criticized as "taxonomic" and "data-cataloguing" (see, for example, Lees, 1957, 1960).

What is improper, considering the intellectual legacy from Bloomfield, is to overstate his mechanistic view. It is too easy to equate mechanism with anti-psychology. Fries (1961) has supplied a strong counter-argument to the charge of Bloomfield's anti-psychology. He has admirably restressed what many knew already, viz., that Bloomfield was interested in avoiding the teleological reasoning about language which older mentalistic psychology had facilitated. Nowhere in Bloomfield's writings is there a denial of the role of psychological factors. For example, he writes: "Needless to state that sound-change and analogy are not, as far as we know, subject to our needs or expression, but are respectively, physiologic and psychologic processes that occur involuntarily and cannot be directed by our needs and desires" (Fries, 1961:200). There is, however, a denial of the possibility of interpreting linguistic phenomena in terms of psychology, and the most widely read statement of it is in the Bloomfield paper chosen for inclusion in Saporta's reader. It is not claimed by this extended observation that Bloomfield has been portrayed out of context in Saporta's reader. The point is rather that some sort of context should have been supplied to permit a greater appreciation of his role in the history of linguistics.

Several chapters from Chomsky's Syntactic Structures are found sprinkled throughout Saporta's reader. The two included in this part of the reader are Chapter 2, "The Independence of Grammar," and Chapter 6, "On the Goals of Linguistic Theory." These chapters are so well conceived that they lose none of their original persuasiveness when standing alone.

The "Independence of Grammar" contains a discussion of grammaticality which will clue the interested student into future problems awaiting psycholinguistic research. But there is a behaviorist objection to the notion of grammaticality which Chomsky's chapters will not dispel. This objection has nowhere been more bluntly formulated than in Olmsted's admonition concerning "one of the more persistent errors committed by
linguists who are not at home among the techniques of psychology; viz.,
inventing judgments which ‘the native speaker’ is said to make, in the
absence of adequate evidence, and sometimes without any evidence what-
soever, other than the investigator’s own overtrained bias” (1955:47).

Even if *Syntactic Structures* does not come to grips with the problem
determining how grammaticality is to be tested, several studies influ-
enced by Chomsky can nevertheless be cited which obviate Olmsted’s
reservations concerning the intuitive quality of the concept. One is
Maclay and Sleator’s (1960) carefully controlled study of native speakers’
responses to differentially grammatical tokens in their language. The
paper provides an insightful discussion of the “same or different” crite-
ration employed in procedural analysis, and then reports an experiment in
which respondents reported their judgments on the (1) “grammatical-
ity,” (2) “meaningful”-ness, and (3) “ordinariness” of a set of sen-
tences. Hill (1961) also deals with notions of grammaticality, although
not experimentally. Insightful discussions of the linguistic informant’s
judgments during field work are contained in Gudschinsky (1958), Hoijer
(1958), and Waterhouse (1961).

Chomsky’s chapters in the reader examine and reject some of the vague
operational approaches traditionally relied upon to specify grammatical-
ity (e.g., semantics, statistical approximation). They stress the need to
construct a recursive generator which will produce “all of the grammatic-
ical sequences of *L* and none of the ungrammatical ones.” The fact that
“all the grammatical sequences of *L*” is envisioned as an infinite set of
sentences, has disturbed many psychologists (and linguists) who are
well aware of the finite capacity of the speaker, “finite” at least in terms
of storage capacity and inductive faculties. Yet that the set of sentences
with which the speaker copes is for all practical purposes infinite, has
probably never been doubted. It is very easy to demonstrate that, both
in production and in recognition, the organism is coping with an over-
whelming proportion of tokens which are for him novel combinations.
As we shall see, an argument from the study of language acquisition
offers striking support of this.

One of the major contributions of generative grammar has been the
demonstration that “knowledge by a finite organism of an infinite set of
linguistic facts is neither paradox nor contradiction, but results from the
fact that there are kinds of finite entities which specify infinite sets of
objects” (Postal, 1964a:247). The specification of these particular “kinds
of finite entities” has, of course, been the seminal contribution of gener-
ative grammar theory: “Either we can try to develop an operational test
of some sort that will distinguish between sentences and nonsentences
or we can attempt to construct a recursive procedure for enumerating
the infinite list of sentences” (Chomsky and Miller, 1963:283). Most psy-
chologists have despaired of the former approach (despite the interest in
Sprachgefühl mentioned above with respect to the notion "grammatical"), but the differences in conceptualization between generative grammar and earlier approaches have been quite missed by some linguists (e.g., Bolinger 1960; Hill, 1961).

The specification of this recursive procedure itself could never have been developed without the mathematical prowess, mentioned in Part 1, which the theoreticians responsible for formulating generative grammar have brought to bear on the problem. This has certainly played a role in the conceptualization of two crucial properties of the generative grammar: its recursive character and the incorporation of a particular type of rule termed "transformational." The former property permits generation of an infinite set of sentences; the latter in part accounts for numerous aspects of linguistic competence (such as the relations "felt" between certain syntactic derivations) which have hitherto been left outside the province of linguistic theory. Basic pedagogical treatments of generative grammar include Bach (1964) and Katz and Postal (1964). The gap between generative grammar and earlier theories of linguistic structure, in their relative ability to explicate linguistic competence, is tellingly documented in Postal (1964b). With regard to the so-called "projection problem"—the formulation of "rules which project the finite set of sentences which he [the speaker] has fortuitously encountered to the infinite set of sentences of the language" (Katz and Fodor, 1963:171) —the difference between the two types of theory is no longer relative, but revolutionary.

At the same time it now appears that grammatical transformations, more than any other notion of the generative concept of grammar, have contributed to a recent reblurring of the distinction between the formal linguistic account of a language and the production of actual sentences in that language. "Transformations are often erroneously conceived to be direct descriptions of processes that a speaker follows in constructing sentences (the same statement holds for generative theories as a whole)" (Bach, 1964:64). An example of this blurring is found in Osgood (1963c). The status of Miller's work with the cognitive transformations inferred as mirroring grammatical transformations (Miller, 1962a; Miller, Ojemann, and Slobin, 1962) remains to be fully appraised (see below).

Chomsky, far more than most linguists, has been concerned with the problem of justification of grammars, and with the relationship of grammatical models to (hopefully corresponding) psychological models of perception, cognition and acquisition:

In part, these questions belong to theoretical psychology. But purely linguistic research can play a fundamental role in adding substance to these speculations. A perceptual model that does not incorporate a descriptively adequate generative grammar cannot be taken very seriously. Similarly, the construction of a model of acquisition (whether a model of learning, or a linguistic procedure for discovery of grammars)
cannot be seriously undertaken without a clear understanding of the nature of the descriptively adequate grammars that it must provide as output, on the basis of primary linguistic data (Chomsky, 1962a:556).

It is evident from current research that some of the psycholinguistic questions touched upon in *Syntactic Structures* are receiving attention. The timing of Saporta’s reader did not permit inclusion of some particularly interesting studies which have been stimulated by Chomsky’s work. Miller, for example, has articles and manuscript materials which treat various psycholinguistic problems in the context of generative grammar theory. His recent papers describe experiments in perception (testing apprehension of the relationships between certain syntactically related but physically dissimilar utterances) which argue strongly for the “psychological reality” of grammatical transformations (Miller, Ojemann, and Slobin, 1962; Miller, 1962a).

Shannon’s “The mathematical theory of communication” and Weaver’s “Recent contributions to the mathematical theory of communication” (Shannon and Weaver, 1949) have exerted a tremendous influence on the development of psycholinguistics, at least to judge from the frequency of references to it since its appearance. Shannon’s paper presented an analysis of information transmission, based on probability theory, providing a means for quantifying the probability of occurrence, both between paradigmatic alternatives and among syntagmatic combinations. The basic probability concept, “entropy,” and its quantum, the “bit,” are now a part of the metalanguage of linguistics. Highly formulaic and embedded in mathematical proofs, the Shannon paper must have resisted full comprehension by many of its readers. The truth of the matter is that most of us read Weaver’s exposition and understood some of Shannon’s by virtue of Weaver’s less mathematical approach.

Some were even more enlightened in 1953 and 1954 by reading one of three papers which appeared in those years. The first is George Miller’s “What is information measurement?” (1953), which provides an elementary and very clear answer to the question raised in its own title. It is doubly valuable in its attempts to show the relevance of information theory for psychology.

The second paper is Kellogg Wilson’s “The information theory approach,” a section of the 1954 survey, which offers perhaps the most useful of all of the introductions to the technical mathematical aspects. Wilson concludes with a consideration of the applicability of information measurement, and generalizes that the measures will be most heuristic for psycholinguistics in their investigation of “how much effect the pattern of antecedent events has on the occurrence of subsequent events, and hence the degree to which sequences of such events are structured (i.e., non-random).”

The third paper is Hockett’s (1953) lengthy review of Shannon and
Weaver's book. Not nearly as concise as the Miller or Wilson paper, Hockett's review is of far greater importance to the development of psycholinguistics, since it goes to its great length in attempting to show the relevance of information theory to linguistic problems. Indeed, the bulk of the review (after a several page reexposition of the theory itself) is devoted to considering various linguistic applications, such as to phonemicizing. The review singles out problems which have only recently gained attention, e.g., the role of paralinguistic phenomena such as voice modifications in conveying information in the channel, and several questions are raised about the continuity of segments whose implications still remain undeveloped. This is a key paper for psycholinguistics.

The article by Skinner which follows in the reader is the first chapter of his book Verbal Behavior (1957). An important point made in this paper is that other disciplines, notably linguistics, have approached verbal behavior without consideration of the speaker, and that the only domain in which the speaker can receive proper treatment is in psychology. As Lounsberry has so well generalized, the linguist traditionally has been only rarely "concerned with the stimulus conditions under which a verbal response is produced or with the nature of the stimulus-response connection and its establishment in the individual" (1963:552-3). But as regards his "causal or functional" analysis, I am at a loss to see how Skinner's approach is an appreciable improvement over "the doctrine of the expression of ideas" which he attacks; perhaps it is an improvement in that it avoids introspection as a research technique. Both the functional analysis of verbal behavior and the antique psychology of ideas, however, are alike in assigning much of the speech act to the unobservable internal make-up of the speaker.

Since it is obvious that the present writer suffers from a tendency to malign operant models, it would be fair to first describe the instrumental approach used by Skinner and his followers, or at least to define the term "operant." "Operant" is one of those psychological terms (together with "latency" and "mediational") which recur frequently in psycholinguistics, but which defy precise definition or, depending on the whims of their immediate employer, exhibit a polysemy of frightening flexibility. Most commentators on Skinner's work agree in calling the approach "instrumental learning" and in treating it within a typology of learning theories. Lambert (1962), Mowrer (1960), and Osgood (1963a, 1963c), offering extensive discussions of the role of learning theory in psycholinguistics, all agree on a basic distinction within the typology which separates "instrumental learning" from the other varieties which in the main are modern descendants of classical Pavlovian learning theory.

Skinner himself has called the classical approach the study of "respondent" behavior, or behavior which is elicited by (i.e., in response to) receipt of specific stimuli. In contrast to respondent behavior, Skinner sees higher animals (and humans using verbal behavior in particular) as
emitting behavior which is not directly elicited and whose recurrence depends upon its reinforcement. [When reinforcement is rewarding such that the originally spontaneous response becomes instrumentally conditioned to a rewarding reinforcing stimulus, the established sequence is called an operant.] To measure the probability or strength of emission of the desired operant is to measure its frequency. Since Skinner believes that verbal behavior is exemplary operant behavior, the way to account for it is "to look for the conditions under which various stimuli can come to control or influence the probability of emission of particular verbal responses" (Osgood, 1963a:250), and it is precisely on this quest that *Verbal Behavior* takes off.

In contrast to instrumental learning, there are several models which ultimately derive from classical Pavlovian learning theory. Lambert's and Osgood's approaches are closely related and are classified as the same type by those writers. Lambert calls these models of learning "classical," whereas Osgood calls them "two-stage mediational models." They agree that this model differs in certain fundamental respects from Skinner's.

In the original classical conditioning situation there is always a pairing of an arbitrary stimulus with a "natural" stimulus, the natural stimulus having an established elicitative bond with a specific response in the organism. Learning is said to occur as the arbitrary ("conditioned") stimulus comes to elicit the response originally appropriate only to the natural ("unconditioned") stimulus. The basic mechanics of this learning process are attested by a wealth of empirical information drawn from experimentation with many species of animals.

Learning theory has been used as a means for analyzing two basic psycholinguistic problems: the first is "how words are used in communication, either as units or as elements in larger response sequences" and the second is the "symbol-referent problem." It is the second which has attracted most of Osgood's attention and, although discussion of his work in semantics will be reserved for Part 5, there are several important concepts that should be mentioned at this point. Both Lambert and Osgood admit that their model is weak in its assignation of much of the speech process to unobservable states within the speaker; both speak of the difficulty in explaining how (especially in verbal behavior) the transfer of evocative capacity is effected, the more so since one has to posit natural (unconditioned) response sequences as a basis for the conditioned transfer to the verbal symbols.

Osgood (1963c), however, has been more bold in offering a hypothesis to account for this transfer, and it is in this context that it is important to understand the meaning of "mediational." According to Osgood, the transfer occurs when the symbols learned "come to evoke some miniature replica of the actual responses made to the referent, and these responses, referred to as 'representational mediational responses,' constitute the meanings of the symbol. They represent or stand for the full pattern of
responses made to the referent, they *mediate* or link the responses made to the referent with those made to the symbol, and they are some form of nervous system response*"* (Lambert, 1962:5,6). The diagram below (*Figure 1*) illustrates Osgood's conceptualization of the process underlying the two-stage stimulus-response nexus; the instance here diagrammed is

\[
\begin{align*}
S_1 & \rightarrow R_1 \\
[\text{pain (spanking)}] & [\text{total R to pain}]
\end{align*}
\]

\[
\begin{align*}
S_2 & \rightarrow r_m \rightarrow s_m \rightarrow R_2 \\
[\text{parent says "bad"}] & [\text{R to "bad"}]
\end{align*}
\]

*Figure 1.* (Adapted from Cofer, 1961, p. 93.)

the child's learning of the sign stimulus "bad" (S₂) through associative conditioning with spanking (S₁). The sequence (rₘ → sₘ) represents, for Osgood, the meaning of "bad." Osgood now has more elaborate "three-stage" models which purportedly obviate some of the objections (to be discussed in Part 5) which have been levelled at the two-stage model (Osgood, 1963a, 1963c).

Many problems are involved in any evaluation of these different approaches. There are issues here, involving past and current trends in psychology, which will escape the notice of many linguists. Not the least is the superficial resemblance of Osgood's concept of "representational mediational responses" to the now discredited views of Watson, the founder of behaviorism, concerning subvocal somatic correlates to speech which he viewed as the basis for thinking and cognition (e.g., see Watson, 1920). Thorson's research (1925) disproved Watson's propositions, but their influence has persisted up through contemporary work on language acquisition (see Part 6).

Clearly what is needed in any psycholinguistics reader is some sort of introduction to the psychological theory and methods which psychologists have applied to verbal behavior. If I had my choice of two additional papers to suggest for inclusion in Saporta's reader, I would without hesitation have selected Chomsky's review (1959) of Skinner's *Verbal Behavior* as one. It is of interest to note parenthetically that several Soviet psychologists, working particularly with language acquisition, were amazed and amused by *Verbal Behavior*, in spite of a demonstrably behaviorist orientation. Suffice it to note here that Tikhomirov's review (1959), arguing on different premises, arrives at much the same conclusions as Chomsky did in his. In this country *Verbal Behavior* also received straw-man treatment in reviews by Osgood (1958) and Morris (1958).
The other paper I would have selected would have been a psychologist’s account of the differences between various learning models available for psycholinguistic theory, and would have included some sort of caution about the reduction of linguistic to psychological phenomena. I think it accurate to say that there was only one paper with these qualifications available at the time when the contents of Saporta’s reader were drawn up; this is Jenkins’ section in the 1954 survey entitled “The learning theory approach.” Apart from the clear distinctions which he draws between particular approaches, Jenkins has presented a discussion of the general nature of psychological theories which, considering its condensed scope, is as informative as many book-length treatises.

It might be wondered why I choose to discuss at this point the neurophysiological bases of language. The answer is that the operant conditioning approach in psycholinguistics has consciously and successfully striven to avoid this topic. Because Skinner seems to reject considerations of theory, it is difficult to gather from his writings just how far the operant conditioning approach has neglected the internal organization of the speaker and therefore also consideration of the various “intervening variables” which others, even strict behaviorists, have on occasion imposed between the stimulus input and the response output. It is peculiar that so much of the work carried on in operant conditioning does so without reference to the findings of other specialties such as descriptive linguistics and, of immediate concern now, neurophysiology.

Many psycholinguists are becoming increasingly concerned with the neurophysiological matrix of speech. The development is of course related to and reinforced by the growth of neurophysiological psychology, which is an important contemporary trend in American psychology. There is even a label available for the designation of the linguistic phase of this interest, namely, “neurolinguistics,” recently proposed by Edith Trager (1961).

Neurophysiology has of course an older association with the study of speech disorders, dating from the writings of Head and others at the beginning of this century and back to Broca in the last. Recently this association has been renewed by the appearance of an important study by Penfield and Roberts (1959) and the equally important review of it by Lenneberg (1960a). But the context in which I especially want to mention neurophysiology is the recurrence of references to it in current theorizing about the acquisition and functioning of the speaker’s linguistic competence. Consider the following remarks taken from recent comments by Chomsky:

The fact that all normal children acquire essentially comparable grammars of great complexity with remarkable rapidity suggests that human beings are somehow specially designed to do this, with data-handling or “hypothesis-formulating” ability of unknown character and complexity (1959:57); and with reference to perception, that “it is possible that ability to select out of the complex auditory input those features
that are phonologically relevant may develop largely independent of reinforcement, through genetically determined maturation. To the extent that this is true, an account of the development and causation of behavior that fails to consider the structure of the organism will provide no understanding of the real processes involved” (1959:44).

Compelling statements proposing the neurophysiological basis of speech are found in Lenneberg’s recent paper, “Language, evolution and purposive behavior” (1960b). The thesis of this study is that man “may be equipped with highly specialized innate propensities that favor, and, indeed, shape the development of speech in the child and that the roots of language may be deeply grounded in our biological constitution, as for instance our predisposition to use our hands” (1960b:869). One of the major problems here is phylogenetic: we are still quite ignorant about the inferred origins of language in a non- or pre-human communication system and are thus denied the insights that a “linguistic paleontology” might provide. Rather we must rely essentially on comparative anatomy, and on comparative neurophysiology in particular, and also on comparative studies of primate communication systems (see Part 9). As neurologists and ethologists fill in the gaps, these arguments become ones of logic, as they must be. There is no independent witness to the evolution of language; e.g., there is no corroboratory evidence in the form of an archaeological record such as we have for cultural evolution, nor of a fossil record such as we have for biological evolution.

The implications of my comments so far might appear to be that learning theory as an approach to verbal behavior is not amenable to integration with neurophysiology. Fortunately, however, this is not the case. Many of the possibilities which both Chomsky and Lenneberg envision with regard to a neurophysiological approach to language were anticipated or subsequently experimented with in work by Hebb and his associates at McGill University (e.g., Hebb, 1949, 1958). Lambert describes Hebb’s aim as extending “the significance of psychological concepts, especially those concerned with complex cognitive processes, by relating them to what is known about the neurology of the central nervous system” (Lambert, 1962:13). From Hebb’s work, significant evidence has accumulated to say that there is verbal input storage, retainable over long periods of time, which can be released without the environmental stimulation conceived as necessary by instrumental learning theory; this is a crucial finding for psycholinguistics.

The great number of papers surveyed in Krasner’s “Studies of the conditioning of verbal behavior” (1958) still does not convey the true extent of the enormous literature concerning word association and other verbal conditioning experiments. No attempt will be made here to survey all the relevant literature, for this is a research area in which there has been considerable repetition. There has been much time for studies of verbal conditioning to accumulate; they were first made famous by Jung shortly after the turn of the century, and go back at least as far as Ebbinghaus’
endeavors in the late 19th century. Indeed, Cofer sees the whole field as a single tradition, stemming from Ebbinghaus, and relentlessly concerned with "the learning and retention of lists of discrete items in an effort to describe and to explain basic associative processes and the conditions of which they are a function" (1961:1).

This reviewer perceives the mainstream of the movement as being isolated from other psycholinguistic research activities and quite encysted in experimentation with numerous variables (many relatively minor) which have been observed or suggested to exert differential effects on rote learning. It is therefore not surprising to discover that this is an area of little interdisciplinary communication, and relative to the amount of research effort expended, one from which linguistic and psycholinguistic theory have profited the least. The communication gap cuts both ways, for there is frequently manifest within the field a baleful ignorance of relevant and heuristic developments in linguistics.

There are some gaps in Krasner's coverage of both older and more recent research in the area of verbal conditioning. Some earlier omissions can be culled from Miller (1951a, 1951b), from more up-to-date surveys by Adams (1957), Dulany (1961), and Salzinger (1959), who survey this specific topic, and by Osgood (1963a). Much ongoing research is reported in articles appearing in the Journal of Verbal Learning and Verbal Behavior and the Journal of Abnormal and Social Psychology, and to a lesser extent in the Journal of Experimental Psychology. General methodological critiques are available; Marshall and Cofer (1963) and Postman (1962) are very useful. More specific methodological problems, such as the analysis of recognition as a measure of retention (e.g., Murdock, 1963), have received concerted attention at several recent symposia devoted to verbal learning and associational processes. These symposia include three from which the proceedings have been published in readily accessible form, namely, Cofer (1961), Cofer and Musgrave (1963), and Jenkins (1959).

Developmental trends in this area chiefly involve experimentation with the host of variables which have been proposed as effecting differences in the acquisition, transfer, and retention of rote-learned verbal materials. Out of these have no doubt emerged some important results. Certainly one of the more inescapable is the realization of the importance, in verbal conditioning, of the context in which the verbal units occur. Context factors such as sequential expectancies are now viewed as a major source of variance in verbal conditioning experiments. Some of the pertinent research, in view of its relevance to the study of transitional dependencies, will be discussed in Parts 3, 4, and 5 below.

One real value of the Krasner paper lies in its discussion of areas other than learning theory in which the results of verbal conditioning experiments might contribute substantial insights and applications. The potential contribution which most impressed Krasner was in the techniques
of psychotherapy, which he views as “directive in nature” and “teachable.” “The therapist uses cues, often without his own awareness, to modify, guide, or manipulate the patient’s verbal behavior.” Equally important applications to teaching are discerned by Lambert, who sees the value of operant conditioning for language teachers (or language machines) “who can be either effective or ineffective as social reinforcers of their students’ attempts to develop appropriate verbal habits” (1962:11).

Krasner leaves undeveloped a consideration which he raises about the design of many verbal conditioning experiments, the subjects’ occasional perception of the relationship between the experimenter’s induced stimulus and associate. Typically such subjects are then excluded from further participation, thus imposing prevalent but as yet unanalyzed variable factors. A few experimenters have actually turned their attention to this problem; it is mentioned in Adams (1957), and is the primary focus in Spielberger (1962), Spielberger and Levin (1962), and Levin (1962). A study by Fuhrer and Eriksen (1960) examined the implications of the subjects’ “unconscious perception” of the conditioning relationship, and an earlier study (Eriksen and Kuete, 1956) reports successful avoidance conditioning achieved without the subjects’ awareness. The dependence of verbal conditioning on the performer’s awareness is obviously one of the more important queries to be answered in this field.

The interest in the “directive” aspects of verbal learning in such social interactions as psychotherapy, manifested in Krasner’s paper as well as in the studies of Fuhrer and Eriksen (1960) and Eriksen and Kuete (1956), suggests hitherto unappreciated possibilities for using verbal conditioning not only in psychiatry, but in experimental clinical and social psychology as well (see also Ullmann, Krasner, and Collins, 1961). The diagnostic value (of perturbations in expected associations) for various types of anxiety, hypertension, and motivational problems, has been discussed by Brody (1964), Buss and Gerjuoy (1958), Korchin and Levine (1957), Sarason (1957), and Taylor (1958). “Other-directedness” is apparently manifest in verbal as well as other social behavior, as shown in recent studies of verbal conditioning as related to various sorts of social approval, ranging from small groups (e.g., Crowne and Strickland, 1961) to audience-speaker interaction (e.g., Levin, Baldwin, and Gallywey, 1960). The experimenter-bias effects of certain types of verbal cuing is discussed by Hildum and Brown (1956) and Sandler (1962). (The directive role of non-verbal cues is discussed in Part 9.)

3. Speech Perception

Part III of Saporta’s reader contains: J. D. O’Connor, “Recent work in English phonetics” (1957); E. Fischer-Jörgensen, “What can the new techniques of acoustic phonetics contribute to linguistics?” (1958); A. M. Liberman, “Some results of research on speech perception” (1957); and

Interest in speech perception, at first concentrating mainly on the auditory recognition of speech sounds, has enjoyed great currency since the beginning of the 1950's. This can be regarded as a core area of psycholinguistics, the result of the coming together of acoustic phonetics and communications engineering, and those psychologists who found in the contact of these fields a novel and fertile area in which to investigate perception. G. A. Miller's book, *Language and Communication* (1951b), was an early and influential response, and his chapter on the "Perception of speech" was an important early synthesis.

Fischer-Jørgensen makes the point in her paper that the communications engineer (unlike the acoustic physicist) has interests in speech which closely parallel those of the linguists, viz., that he "is directly interested in the functional aspect of speech sounds; he wants to find those sound features which are relevant for communication; he will also, like the linguist, be interested in relating the acoustic phenomena to the activity of the speaker and to the perception of the hearer." Since it was engineers who were designing all the attractive new gear for acoustic analysis, interdisciplinary contact was inevitable. Moreover, the contact proved to be a fruitful one for all parties, as amply demonstrated in Miller's book and in work which proceeded to emerge from various projects of the Haskins Laboratories, from the Bell Telephone Laboratories, and from the M.I.T. Acoustics Laboratory.

The weighty *Handbook of Experimental Psychology* (Stevens, 1951), containing important papers by Miller, Licklider, and von Békésy, had the desirable effect of calling the attention of psychologists to the linguistic aspects of communication and perception. *The Journal of the Acoustical Society of America* (particularly during a peak period of activity in the mid-1950's) was soon laden with articles on speech perception and became a more familiar periodical to many American linguists than the older phonetics journals. Typical of the activity at that time, in 1952 the Acoustical Society of America sponsored "A conference on speech analysis," bringing together linguists, psychologists, and communications engineers, and producing an important group of papers published in the society's journal; these included Fischer-Jørgensen (1952), Pike (1952), and Licklider (1952).

It would be to wander too far afield to mention available introductions to acoustics and acoustic phonetics in this survey. Good bibliographic coverage through the year 1956 is provided in Fischer-Jørgensen (1958), and in Fant (1960), who carries the coverage into 1960. A disarmingly simple and sound introduction is available in a new study by Denes and Pinson (1963), while Fant (1960) remains the standard major reference text.

The O'Connor paper, principally a review of results, makes a distinc-
tion among the personnel interested in perception, between "linguistic phoneticians" and "linguistic experimentalists." Most of the psycholinguistic work in perception has been experimental. And the development of the "new techniques" discussed by Fischer-Jørgensen has made for the possibility of very carefully controlled experimentation indeed. Unfortunately, many of these new techniques (and the instruments which they use) are highly complex and it might reasonably be argued that papers descriptive of the techniques and methods themselves might have been included in Saporta's reader. For example, Miller's paper, "The perception of speech" (1956), offers a far clearer discussion of the notion of psychological similarity of stimuli (and of the "analysis of confusion" designed as a technique for measuring similarity) than is available in his and Nicely's applied study in the reader.

The analysis of confusion practiced by Miller and his colleagues is not the only experimental approach to the recognition of speech sounds. A more dramatic approach involves the use of speech synthesizers. An informative discussion of this approach is offered in the Liberman paper, which reviews some results of the Haskins Laboratories' various quests for "the acoustic cues on which . . . perception depends." Using spectrograms and instruments such as their Pattern Playback, they proceed to "make controlled changes in various aspects of the acoustic pattern, and then to evaluate the effects of those changes on the sound as heard." Some of their results will be mentioned below.

There is yet a third experimental approach to the recognition of speech sounds, which is not represented in the reader. This is experimentation with tape-splicing, which is more easily and less expensively carried out when synthesizing, and hence more in range for student projects and budgets. I missed either Schatz's (1954) or Malécot's (1960) interesting papers in the reader.

Liberman brings up the issue of "auditory feedback," and his comments, together with Fischer-Jørgensen's, outline some of the possible explanations to account for tie-ins between the articulatory, acoustic, and auditory phases of speech sounds. Chiefly on the basis of experiments with English consonants, Liberman argues that "speech is perceived by reference to articulation—that is, that the articulatory movements and their sensory effects mediate between the acoustic stimulus and the event we call perception," and that "the mediating articulation not only produces distinctive proprioceptive stimuli, but also external sounds which can be matched against the sounds being perceived." The present viewpoint of the Haskins investigators, expressed recently by Abramson, is that the psychological invariance underlying perception must reside in the neural codings of the articulatory movements:

A link between the perception of speech sounds and feedback from the articulatory movements that the hearer would use in producing these sounds, leads to categorical
perception when the phonemic distinctions involve no intermediate articulations; in phonemic distinctions which rest on articulatory continua, perception will be less categorical, and differential discrimination ought then to be fairly constant throughout the range of variation (1961).

The fact that some sort of neurological servosystem does monitor coding cannot be doubted; numerous experiments have suggested this, although the exact motor processes have all been inferred (e.g., Fairbanks, 1954). But it is equally obvious from recent investigation of language acquisition and speech disorders, that receptive perception, i.e., the decoding of messages by the hearer, cannot depend on such a servosystem as described, the obvious countercase being where speech is understood by the hearer who is congenitally defective for speech production (see Part 6). The outstanding problem in this area may well turn out to be our relative ignorance of the receptive apparatus itself and its neurophysiological functioning. Recent investigations by Mol, von Békésy, and others may fill in some of the gaps in the future.

The selection of readings contained in this section of Saporta's reader is a good one, but is of course restricted to the perception of phonological units. Many other experiments have focused rather on the perception of larger, meaningful units (usually the "word"). Most of these experiments have been concerned with the role of redundancy in recognition and learning tasks, and one of the major psycholinguistic generalizations which we are able to offer derives from them, viz., that recognition and learning performance increase directly with the experimental increase in redundancy. These experiments will receive extensive discussion in Part 4 below.

Accumulating evidence from numerous experiments in perception during the past decade suggests that the "distinctive auditory cue" is a complex stimulus, rarely simply correlated to the phoneme segment which purportedly includes it. Oversimplified models of perception have produced simple-minded notions about the process of decoding. H. Mol has been active in combatting some of these inadequate models, and his comments about one prevalent view of perception are very timely: "The mechanism of recognizing spoken words and sentences is no doubt voice-operated but it may not be regarded as the acoustic counterpart of an electrical teleprinter which prints letters that are unambiguously labelled by the electric signals it receives from a transmission line" (1963:50). The papers included in the reader all herald this conclusion, although it actually is not very clearly formulated in these readings.

Phonemic theory has long held that there is a constancy or invariance in the production of phonemes, and that contrast is implemented by the distinctive common denominator which all allophones of a particular phoneme share. The various non-distinctive attributes of the allophones were correctly viewed as the conditioned features dependent on the larger (usually immediately including) phonemic context, such that one could
speak of this intersegmental dependency as a form of redundancy. The contemporary challenge to the concept of the phoneme as a psycholinguistic prime derives from the fact that its perception is not a direct function of phonetic substance; it is an indirect function, in which as much information for discriminatory perception is derived from the context. And it is extremely important to appreciate that the concept of "context" itself has become much more catholic during the course of the past decade. An asemantic immediate environment (defined in terms of adjacent phonological units) has been found inadequate, and it has been well argued that the much larger grammatical context is part of the cue-producing environment.

The view that perceptual invariance cannot be simply contained in the signal input is gaining ever wider acceptance. It has been most compellingly argued by Chomsky (1962a), Halle (1959), and Lees (1962), but it is not entirely novel to their writings. Pike (1959), for instance, conceded some alternatives to the view of language involving the phonological principles of invariance and linearity.

Psycholinguistic evidence for the new view derives from a number of experiments. That hearers can correctly identify vowel phonemes in a restricted context such as a CVC-syllable was early thrown open to question by Peterson and Barney's study (1952). Brown and Hildum (1956) have shown that perceptual discrimination, particularly the identification of given phonemes, is related to the probabilities of occurrence which those phonemes have in the specific larger context in which they were presented. Mol and Uhlenbeck (1959) have summarized experiments which indicate that contextual clues yield information useful for perception, and that their suppression greatly decrease the hearer's correct decodings of chosen tokens. The reviewer concurs again with Mol's evaluation, viz., "the lesson of all those investigations is that the phonetic data the ear extracts from the sound waves do not form the only source on which the listener bases his identifications" (1963:52). Again fuller discussion is deferred until the following section.

Much of the work carried out in perception and speech has involved correlations of auditory with acoustic and articulatory units. Some investigations have been concerned, however, with the auxiliary role of visual clues in speech perception. Rubenstein and Aborn (1960) report on experiments by Sumby and Pollack (1954) and O'Neill (1954); the former demonstrate the increased importance of visual clues under conditions of interference with the speech signal, and the latter show that visual clues contribute more to accurate perception of consonants than of vowels, more for vowels than words, and more for words than phrases. The importance of visual clues was subtly involved in Householder's (1956) experiments with hearers' perception of English final unreleased p, t, k.

One interesting alley off the thoroughfare of perception studies involves
the variable which we might classify as the hearer’s “personal attention.” I refer now to channel noise over which the hearer has some voluntary control, and not to the experimentally produced channel distortions which occur in many perception studies. This attention variable would include conscious attempts to block out or distort the impinging message. It would also include the hearer’s effort to single out one message from a channel choked with several, e.g., as in the co-occurrence of messages from radio stations transmitting at the same point on the band. Broadbent (1957, 1958, 1962), who has done some work with the individual hearer’s perception of several simultaneous messages, has found that if the hearer is exposed to a channel with two voice messages, he usually understands only one. Broadbent’s work in this area leads him to conclude that there is a selective mechanism in the central nervous system (an “attention mechanism”) which permits differential perception.

A problem wide open for psycholinguistic experimentation is the validation of “distinctive-feature analysis,” the basic conceptualization of which is presented in Halle (1957, 1959, 1962), and further discussed in Chomsky (1962a) and Chomsky and Miller (1963). Particularly important for a full understanding of the speech recognition process would be experimentation with “feature opposition”; it would not seem too difficult, for instance, to determine whether or not oppositions are inherently binary. The Miller and Nicely study (1955) offers one research design through which such experimentation could be implemented. The conclusion of their study, as a matter of fact, indicates that several oppositions within the distinctive feature roster have discriminatory value of great power, although these were more effective in distinguishing one class of phonemes from another, than in distinguishing between members of a given class (such as “voiceless stops”). Also relevant here is the continuing experimentation with speech synthesizers (e.g., Liberman, Harris, Eimas, Lisker, and Bastian, 1961; Fry, Abramson, Eimas, and Liberman 1962). Most of this research indicates that perceptual distinctiveness is a very direct function of articulatory contrasts. Related are first, the vaguer issue of the “psychological reality” of phonological units (for which Sapir’s 1933 classic still provides rewarding reading) and, second, the more important issue of the incorporation of perception into a general theory of language. Some models proposed for speech recognition are discussed by Miller and Chomsky (1963), including an important paper by Halle and Stevens (1962). The further pitfalls of confusing a model of linguistic structure with a model of speech performance (here recognition models) is touched upon in Katz and Postal (1964) (esp. pp. 166 ff) and in Postal (1964a).

4. The Sequential Organization of Linguistic Events

Part IV of Saporta’s reader contains another chunk from N. Chomsky’s Syntactic Structures, his Chapter 3, “An elementary linguistic theory”
(1957); K. S. Lashley, "The problem of serial order in behavior" (1951); G. A. Miller and J. A. Selfridge, "Verbal context and the recall of meaningful material" (1950) [the paper is incorrectly dated 1953 in the reader]; G. A. Miller, "Free recall of redundant strings of letters" (1958); and D. Howes and C. E. Osgood, "On the combination of associative probabilities in linguistic contexts" (1954).

In spite of the relatively early dates of some of these studies, they excellently represent the work in this area. The problems relating to sequential organization are among the knottiest of all those awaiting resolution. Moreover, they constitute an area which behaviorist psychology in this country has avoided altogether. There is no better evidence for this than Lashley's study, which is widely quoted in psychology (and now in linguistics as well) as a declaration of interest in the overlooked internal make-up of the organism responsible for its integrated sequential behavior. Since the time of its appearance, interest has focused more and more on the intervening hook-ups between easily observed stimulus input and response output sequences.

During the same time, advances in neurophysiological psychology have conferred an earlier lacking legitimacy to dissecting the "black box." Numerous associated endeavors could be cited. It is revealing to observe the florescence of studies in cognition which these developments encouraged, a trend which will receive only this nodding recognition here. It is linguistically interesting to observe the neologisms and euphemisms which have evolved to break away from the mechanistic behaviorist terminology of earlier decades: "perceptual organization," "cognitive schemata," etc., which finally permitted the reappearance of "mind." For example, "I now believe that mind is something more than a four-letter Anglo-Saxon word—human minds exist and it is our job as psychologists to study them" (Miller, 1962a:761). But a more important observation is that, in the past decade, verbal behavior has become a frontier area for psychologists concerned with studying the integration of behavior: "Moreover, I believe that one of the best ways to study a human mind is by studying the verbal system that it uses" (Miller, 1962a:761). This sentiment was anticipated by Lashley in the paper included in the reader: "Certainly language presents in a most striking form the integrative functions that are characteristic of the cerebral cortex and that reach their highest development in human thought process."

Lashley sets the stage for psycholinguistic investigation in this area: "This is the essential problem of serial order: the existence of generalized schemata of action which determine the sequence of specific acts, acts which in themselves or in their associations seem to have no temporal valence." The relevant experimentation with these schemata has to date been far removed from any examination of their possible neurophysiological bases. (The consequent importance of the study of speech disorders will receive comment in Part 7.) Much is known about the specific affer-
ent paths leading from the receptors to the cortex and similarly about the efferents leading to the motor areas used in speech. Much has also been discovered about the localization of specific functions in the cerebral cortex (through the work of Penfield and others). Very little is known, however, about the precise processes of decoding and encoding; let alone the problems of storage and linkage between the sensory projection and motor areas. Hebb and Osgood have been much concerned with working out the central nervous system’s substructure for speech and the above mentioned problems of storage and linkage. I would characterize their efforts as partially complementary, and more crudely by the image of Hebb working from the inside (from the neurophysiological bases) outward, and Osgood from the outside (from the behavioral manifestations of speech) inward. A promising point of contact between Osgood’s model of speech and Hebb’s is that the “cell assemblies” (reverberatory circuits) postulated by the latter may be the somatic counterpart to Osgood’s “mediational response” processes. A too brief and therefore tantalizing glimpse into this train of thought is available in Osgood’s discussion of his own “three-stage” model of speech (1963a, 1963c); another brief and enthusiastic glimpse is afforded by Lambert’s recent review (1962). The psycholinguistic contributions toward an understanding of behavioral organization, as we shall see, have all been inferential; but this in no way reduces their importance for a theory of language.

An important point about the development of psycholinguistic theory is not mentioned in the papers included in this section of Saporta’s reader. Just when grammatical theory and studies of perception were becoming productively involved in the sequential aspects of speech, information theory (largely through the impact of Shannon and Weaver and their interpreters, such as Hockett) offered an attractive approach to its quantification which obscured the psychological insights offered by Hebb and Lashley. It obscured their insights because it sharpened focus on only one narrow aspect of sequential behavior, viz., its stochastic properties. The result of this restriction was a great emphasis on “transitional dependencies,” which were investigated with a variety of techniques concentrating solely on the effect of antecedent contexts of the message on the occurrence of subsequent units. These transitions were measured in terms of their probabilities, and the whole frame of reference permitted a model of speech characterized by associative probabilities.

Linguistic method... identifies sets of discrete alternative units at each level of analysis. Learning theory models involve the central notion of associative hierarchies. At each level of behavioral organization, we must assume that particular antecedents are associated with sets of alternative subsequent events (divergent competitive hierarchies) and that particular subsequents are associated differentially with sets of antecedent events (convergent facilitative hierarchies). A major point of articulation is that between these associative hierarchies of learning theory and the substitution classes of linguistics, and information theory provides a highly generalizable method of describing the structure or degree of organization of these associative systems which
result in linguistic responses. The convergent effects of antecedent context upon modifying the probabilities of alternative responses, raising the probabilities of some and lowering others, corresponds to the reduction of conditional uncertainty or entropy in information measurement (Osgood, 1963a:270).

The cognate model for speech acquisition, using associative probabilities, is a primitive learning theory model in which entropy profiles are transposed into “response probabilities” and “associative hierarchies.” The learner is purported to acquire these response patterns (let us temporarily call them “types”) inductively through exposure to numerous well-formed “tokens” which he hears uttered in his community. One is reluctant, however, to push this model beyond the sorts of simple phrase-structure sequences involved in most psycholinguistic experiments. In many experiments, typically, positional “types” are equated with the form-classes (and their simple linear orderings) yielded by immediate constituent analysis. One of the difficulties inheres in immediate constituent analysis itself, since that model treats as unrelated events many sequence types which are intuitively perceived as closely related by the native speakers (e.g., the relationship of an active sentence to its passive transform).

The fact that a simple stochastic model poses an insuperable obstacle to the child’s actual learning of his language has been demonstrated by Miller in his chapter on language acquisition in Plans and the Structure of Behavior (Miller, Galanter, and Pribram, 1960). More general comments on the limitations of the stochastic model, both as a model for acquisition and as a model for production, is contained in Galanter and Miller (1960), and a more recent analysis of its employment in psycholinguistics is given in Miller and Chomsky (1963).

All these immediate context models have recent support, however, from communications engineering and a decade’s worth of psycholinguistic endeavor involving experimentation with the role of sequence constraints as contextual clues. It was early discovered in perception experiments dealing with “distinctive” speech sounds, that if contextual clues are eliminated or suppressed, perceptual constancy is correspondingly reduced (see Part 3). So, too, with various identification and recall tasks involving words and phrases presented with and without their grammatical contexts. Earlier studies, such as that of Miller, Heise, and Lichtern (1951), embody the stochastic model directly from information theory: the greater intelligibility of words in context is attributed to the reduction of possible alternates because of the sequence constraints on certain form-classes. Entropy rises as the antecedent contexts are eliminated; i.e., the hearer’s uncertainty increases as sequential predictability is disturbed.

This pattern can be seen in the design of the Miller and Selfridge (1950) paper: “By verbal context, as opposed to total context, we mean only the extent to which the prior occurrence of certain verbal elements influences the talker’s present choice.” These authors construct a clever learning
task involving strings with sequences of varying degrees of approximation to permitted English word order. Their results suggest that sequential dependencies, rather than meaning, enhance correct recall: "If the nonsense [of remoter approximations] preserves the short range associations of the English language that are so familiar to us, the nonsense is easy to learn."

Saporta’s organization of the readings in this section of his reader is superb. Miller’s article on “Free recall of redundant strings of letters” (1958) follows on the heels of the Miller and Selfridge paper in the reader. This pairing very clearly offers an example of the cumulative effects of research in this area, since the second builds on the first, spelling out very clearly how its results have been incorporated into other experiments and theory-building. The paper itself reports on a very well-controlled investigation of recall of structured (therefore redundant) strings of letters as opposed to random strings. The results show predictably that the redundant strings are learned faster, but that with the increase in redundancy comes a decrease in information conveyed. Equally important, Miller integrates his findings in this experiment with the 1958 Chomsky and Miller paper concerning finite state models.

With apologies to the authors because of their tremendous investment in time, I find the Howes and Osgood (1954) paper very difficult to read. Moreover, in spite of (or perhaps because of) the formulae which bristle on every page, the relevance is of the variables being tested is not clear. The authors describe a word association experiment designed to test the effects of a varied compound stimulus on an expected associate; the experiment shows that the effects of their variables ("interposition," "density," and "frequency") are statistically significant.

The selections in this part of Saporta’s reader are not rich bibliographically. The reader can gain an appreciation of the range of research problems in this area that were awaiting investigation a decade ago, by reference to Section 5 ("Sequential psycholinguistics") of the 1954 Psycholinguistics monograph reprinted in the present volume. Good reviews of subsequent developments may be found in Rubenstein and Aborn’s survey (1960), especially in their section “Probability of language segments,” and in Osgood’s more recent survey (1963a).

Structural context as it relates to auditory perception is discussed in Pollack and Picket (1964), with reference to experiments on the intelligibility of speech segments excerpted from larger discourses. An excellent paper by O’Neill (1957) shows that more words are recognized in noisy channels when included in larger redundant contexts than when produced alone. Bruce (1958) reports on the variable accuracy in perception of sentences, depending on the context-induced set of the hearer—in this case, merely whether or not the subject was apprised of the topic of a possible (but not presented) larger surrounding discourse. More general issues relating to contextual constraints upon learning (as well
as upon perception) are discussed in a number of interesting papers, including Aborn, Rubenstein, and Sperling (1959); Cofer (1960); Howes (1957); and Rubenstein and Pollack (1963). The specific role of the grammatical form class receives attention in Deese (1962b); Ervin (1963); McNeill (1963); and Miller (1962b).

The problem of isolating the psycholinguistically distinctive units of sequential encoding has elicited some interesting experiments with pausal phenomena, which are investigated not only as possible boundary indicators but also as measures of latency of transitional dependencies. Goldman-Eisler has written several papers on this topic (1955, 1961), although her primary interest is in the study of prosodic features as indicators of certain types of personality disorders (see Part 9). An especially interesting paper in this vein is Maclay and Osgood's (1959) study of hesitation phenomena in English, which suggests that pauses are a function of increased entropy at certain stateable sequential positions. The constraints imposed by syntactic rules are discussed in several papers by Epstein (1961, 1962, 1963), in Miller (1962a), and in Mehler (1963), and the constraints of semantic rules are dealt with in Marks and Miller (1964) and also in Miller (1962a).

Another fascinating tack for the interested student is to explore the implications for sequential decoding and encoding (and storage) contained in Miller's work with immediate memory span; his "magical number" paper (1956) has already had some impact on grammatical theory. Apart from the fact that a model for speech production must represent the speaker with a finite memory, there are interesting suggestions that certain grammatical constructions (e.g., some relative and passive transformations in English) are devices which reduce overloading of the storage capacity of the speaker.

Yngve has discussed storage capacity in various syntactic structures (chiefly in English) and has concluded that many constructions function so as to minimize "grammatical depth" and to prevent overload. He has argued this convincingly in several papers (1960, 1961, 1962) in which the focus, to be sure, is rather on the temporary memory spans of computers being programmed for sentence construction tasks. Brief discussion of Yngve's suggestions is available in a recent paper by Chomsky (1961b), and in another by Plath (1961), and more extensive treatment is available in Miller and Chomsky (1963).

Of the many insights offered by the several Chomsky chapters, none is more relevant here than the conclusion of his Chapter 3, "An elementary linguistic theory," which forms part of this section of the reader: "It seems quite clear that no theory of linguistic structure based exclusively on Markov process models and the like, will be able to explain or account for the ability of a speaker of English to produce and understand new utterances, while he rejects other new sequences as not belonging to the language." This and Chomsky's Chapter 2 (included in Part II of
the reader) are too succinctly presented to paraphrase or explicate here. His demonstration of the independence of grammar from some of the various approaches to its formulation (through semantics and statistical orders of approximation), and the demonstration of the inadequacy of the finite state model, have become widely known. The student who reads Chomsky’s Chapter 3 must wonder, in the face of such forceful argumentation, whether the articles which follow it in this part of Saporta’s reader (Lashley’s excepted) really point the way to fruitful psycholinguistic research for the future, since all are involved with limited left-to-right structures.

Some confusion will be avoided if the student appreciates the fact that Chomsky is not immediately concerned with constructing a model of the language user (see Part 2). This fact has been more sharply enunciated by Chomsky since the publication of his *Syntactic Structures*; e.g., “The attempt to develop a reasonable account of the speaker has, I believe, been hampered by the prevalent and utterly mistaken view that a generative grammar in itself provides or is related in some obvious way to a model for the speaker” (1961b:14). The question of ultimate goals for linguistic theory has also received more expanded exploration by Chomsky in his more recent articles, and the student should apprise himself of the content of several of these later studies (e.g., 1961a, 1961b, 1962a, 1962b, and 1963).

It is especially exciting that the work of Miller and others is beginning to offer corroboratory support for the “psychological reality” of many of the abstract characterizations of the user’s linguistic competence which are contained in a formalized generative grammar (see Part 2); a case could be made for Chomsky’s being overly cautious in his appraisal of the explanatory power of generative grammar theory.

5. *The Semantic Aspects of Linguistic Events*


This assortment represents a tremendous tour de force on a topic which traditionally has supported as many approaches as there were investigators willing to approach it. It is interesting to notice that these readings, with the exception of Wells’s, are parts of much larger pronouncements on verbal behavior and/or linguistics, all of which have been
obliged to cope with "the reference problem," i.e., with "meaning." Considering the diversity of thought represented by these readings, one might conclude correctly that this has been an avenue of inquiry characterized by much individual and little concerted effort. This aspect of the climate of semantics has been aptly characterized by Katz and Fodor in their recent proposal for a semantic theory. These writers point out that "semantics suffers not from a dearth of facts about meanings and meaning relations in natural languages, but rather from the lack of an adequate theory to organize, systematize, and generalize these facts" (1963:170). Weinreich similarly maintains that there is little theory available for a consideration of semantic universals, and that even such undirected particularistic studies of meaning as are available suffer from being "on the whole preoccupied with the one semiotic process of naming, i.e., with the use of designators in theoretical isolation; they pay relatively little attention to the combinatory semiotics of connected discourse" (1963:115), let alone undertaking to carry out extensive cross-language comparisons.

There are a number of rather basic differences between the linguist's approach to the analysis of expression (which has been the main concern of descriptive linguists, at least in this country) and his initial probings into the analysis of meaning or content. I will not consider here the many pronouncements in American writings asserting that the analysis of expression is the only legitimate concern of descriptive linguists, nor the logical gymnastics performed by many analysts in attempting to avoid problems of meaning in their actual analyses; for a discussion of this topic, see Wells (1951). The less extreme view in American descriptive linguistics merely claims that the expression plane of a language can be descriptively analyzed more or less independently of the content plane which is in some way correlated with it. The proviso "more or less independently" allows for the fact that expression is only rarely analyzed in total abstraction from content, since the operational procedures employed in isolating linguistic primes involve the criterion of "same or different."

"Same or different" is the basis for a game played between analyst and informant that most behavioral scientists would regard as a primitive controlled experiment in which, by manipulation of units in the expression, the linguist contrives to effect or avoid a correlated change in content. The resemblance of "substitution" and "commutation" procedures in linguistic analysis has been explicitly recognized by Haugen (1951). Further discussion of this interesting parallel is available in Fischer-Jørgensen (1956) for phonology, and in Diderichsen (1958) for phonology and morphology.

How have linguists attempted to describe the structure of content? Let us first restrict our attention to a consideration of discovery and segmentation procedures. Although there have been several general approaches to devising an operational procedure which will consistently
yield a semantic prime, the prime itself has been very reluctant to emerge. Wells has argued that if content is subjected to analysis in total abstraction from expression, the linearity which permits segmentation of expression does not obtain: "since the C-plane has no proper counterpart to temporal order, it cannot have analogues to those E-facts [viz., phonemes and morphs] that depend essentially on temporal order" (1958:659).

Related problems revolve about the factor of continuity, which one is obliged to take into account in semantic analysis. Traditionally, the procedures used to isolate linguistic units of expression have been based on the discovery of discrete discontinuous attributes (see Part 1). Linguists have been frustrated in their attempts to carry over these discovery procedures into semantic data which subvert the quantum mathematics. Nevertheless, there are prevalent one-to-one correspondences between the planes of content and expression, as manifest in the much-studied relationships of words to their referent-objects. Analysis of these correspondences has led several workers, including Jakobson (1948) and Ebeling (1962), to delimit as a semantic prime those recurrent distinctive features of meaning which Ebeling calls "semantic minimums," viz., "the ultimate semantic constituents which are separately interchangeable in the positions where they occur. . . . In this sense, the semantic minimums are on a par with the distinctive features in phonemics" (1962:92).

A commutation-like procedure is employed by Ebeling to yield these features. Their recurrences and discreteness are established by proportional equations (e.g., stallion:mare = ram:ewe), which Ebeling claims are amenable to precise psycholinguistic experimentation with untutored speaker-subjects. Ebeling's method is considerably more refined than this review will be able to demonstrate. What is important to add is that his method assumes as co-occurrent the discovery of significant units between content and expression. Moreover, recognition of semantic resemblances has some priority since this "furnishes the directives for the decomposition of the chain into morphemes" (1962:113): "A relevant morphemic feature presents itself to the investigator as a correspondence between one or more phonemes on the one hand and one or more semantic minimums on the other" (1962:114).

Several problems are associated with postulating a semantic prime, and some have immediate psycholinguistic relevance. Operational procedures for morphological analysis early ran afoul of so-called "submerged" semantic components, a variety of semantic minimum never discretely associated with a given unit of expression (e.g., with a morph). Such submerged or covert features are often especially evident in inflectional paradigms wherein several obvious semantic dimensions (e.g., number, aspect) are operating, but where the words cannot be segmented into morphs that are discretely associated with any one component of meaning.

Many pseudosolutions such as the postulation of "portmanteau" segments were proposed, but the only development of any immediate fruit-
fulness was that which we now loosely call "componential analysis." Componential analysis will receive further and more detailed attention in Part 8, especially with reference to its psycholinguistic implications. Here it will suffice to mention some of the earlier influential studies in componential analysis and to indicate some of the implications this approach might have for broader problems in semantic analysis. One early influential demonstration of componential analysis was Harris (1948). An insightful exploration of the applicability of componential analysis to meaning is contained in Lounsbury (1956), and an extended application to lexicography is contained in Malkiel (1959).

Componential analysis has forced its practitioners to be much more explicit about their semantic theories and methods for semantic analysis than has hitherto been customary. Since componential analysis yields (often covert), paradigmatic structure within semantic domains, investigators have become much more aware of the contrastive and hierarchical structuring of content and, in particular, of the distinction which many European linguists make between "form" and "substance" within content. This is suggested by the terminology that is now appearing in anthropological literature devoted to ethnoscience. Here appear terms such as "segregate," "contrast set," "(criterial) attributes," and others, which reveal their backgrounds chiefly in cognition psychology but also in lexicography. Definitions of these terms are given in Frake (1962) and in Conklin (1962), among others. The relationship of componential analysis and ethnoscience to cognition psychology becomes especially evident upon comparing these papers with Brown's "Language and categories," an appendix to Bruner, Goodnow, and Austin (1957), and with perception theory in Kilpatrick (1955).

Although I am not certain that the parallel will be accepted, to me these terms seem to reflect an indigenous American attempt to devise a set of discovery procedures for the descriptive analysis of the "form of content," applying to "form" and "substance" the basic dichotomy which glossemics makes in both expression and content. I use "glossemics" here to refer to an approach. The approach itself is penetratingly discussed in Wells (1951) and Fischer-Jørgensen (1949). A general introduction to the methods and theory of glossemics is available in Hjelslev and Uldall (1957), and applications of the approach to the analysis of content can be found in Recherches structurales 1949, particularly in the papers by Diderichsen (1949) and Lotz (1949).

Some of the features of the glossematic theory which find possible relevance here include the manner in which the relationship between expression and content is envisioned. Analysis of the form of expression and content can be carried out in abstraction from the descriptive characterization of their substances, and it is this study of formal properties which is of most interest linguistically. The antithesis between form (the abstract relational properties of a system) and substance (the specification
of the physique of individual units in the system, as in the phonetic description of a particular allophone) in part finds expression in the American systems of hierarchical analysis. Thus a "higher level" is in some ways a formal statement of a lower level which, relative to the upper, specifies substance. For example, as above, a phonemic analysis is in part a formal statement based on the substantive facts of the underlying phonetics of the language. The distinction, not always easy to apply, is not only a logical nicety; it is crucial for much of psycholinguistic research.

The whole concept of a psychological equivalence category devolves on a distinction between form and substance. Linguists, unlike some social scientists, have available highly efficient techniques for deciding when two events are the same, i.e., in some way linguistic equivalents of each other. This is not a matter of crude physical measurement; rather, it is a way of measuring psychological equivalence, and the psychological equivalence of two entities often obtains in spite of physically measurable differences in substance between them.

Let the student beware, however, that while these discovery procedures have been remarkably well worked out for the analysis of expression, this has not been the case for the analysis of content, notwithstanding the promising advances of componential analysis. This will become abundantly clear from the selections contained in this section of Saporta's reader. What will also become clear is that the selections have precious little to say about two central topics: a general theory of semantics, and the different "kinds" of meaning which might be distinguished.

It is most difficult at this stage of our knowledge to outline the relevance of a general semantic theory to psycholinguistics, or to predict in what ways psycholinguists might be helpful in the construction of such a theory. As an example of the sort of contributions which psycholinguistic research might offer, one can cite the exciting experiments concerning semantically determined combinatory rules which have been reported by Miller and Isard (1963). In these experiments the respondents' auditory perception of test sentences differed according to whether these were grammatical, anomalous (i.e., semantically devious or non-meaningful, although grammatical), or ungrammatical. The report is especially interesting for its discussion of semantic rules and the role of these rules in the perception of speech. More recent experiments with "semantic rules" are reported in Marks and Miller (1964).

Of the proposals for a semantic theory surveyed by the reviewer, only one adequately spells out the general form that such a theory must take and the sort of linguistic competence to be accounted for by that theory. This is the study by Katz and Fodor (1963) referred to above. Katz and Fodor deal with the "speaker's ability to interpret sentences." They set as the practical goals of a semantic theory the description and explanation of this "interpretative ability of speakers by accounting for their
performance in determining the number and content of the readings of a sentence, by detecting semantic anomalies, by deciding on paraphrase relations between sentences, and by marking every other semantic property or relation that plays a role in this ability” (1963:176). Their paper offers considerable clarification of the issue of the separation of grammar and semantics.

This issue has several facets. One that has been raised by Chomsky in his *Syntactic Structures* (1957) is that the property of grammaticality cannot be equated with the meaningfulness of the utterance being tested for that property. The independence of grammaticality from semantics has been questioned by Putnam (1961), who finds that the putative sharp demarcation between ungrammatical and semantically devious is difficult to discern.

Another facet of the problem is raised by Chomsky in “Syntax and semantics,” Chapter 9 of his selection from *Syntactic Structures*, appearing in this section of Saporta’s reader. This is the question of whether discovery procedures in linguistic analysis and the linguistic primes which they yield can be defined independently of meaning. Chomsky again builds a strong case for the autonomy of grammar from semantics, and again Putnam (1961) offers reservations, in this case independently echoed in two critical reviews of Chomsky’s work, by Haas (1958) and by Matthews (1961).

But it is interesting to notice that a semantic component is now viewed as the second of two important interpretive elements in the generative grammar, the other interpretive component being phonological. Katz and Postal’s recent *An Integrated Theory of Linguistic Descriptions* is to a large extent an attempt to “provide an adequate means of incorporating the grammatical and the semantic descriptions of a language into one integrated description” (1964:x). For the limits of the semantic competence specified by their model, see Postal (1964a) and Katz and Fodor (1963).

It is unfortunate that no paper is included which discusses more clearly the relationship of content and expression, or which attempts to describe the possible varieties of meaning. The Bloomfield “Meaning” paper (1933) fails as a candidate on both fronts. The welter of linguistic, stylistic, and paralinguistic phenomena which are paraded in this paper adds support to Bloomfield’s contention that a scientific semantic analysis is beyond the means of linguistic methodology. But the case for this view is surely overstated. It is true that Bloomfield’s position strongly influenced subsequent developments in American linguistics, which has neglected semantic analysis, but this does not emerge from the paper itself, and should have been supplied by editorial comment. The student-reader may already hold to the widespread fallacy that Bloomfield opted to ignore meaning altogether. Although a careful reading of his 1933 paper will dissuade him from this belief, the possibility for confusion might have
been lessened by inclusion of one of Bloomfield’s later papers (e.g., 1939) wherein the whole topic of meaning receives a more precise and coherent treatment.

Within the reader, Wells’s paper, “Meaning and use,” is very interesting in its own right. Moreover, it puts Bloomfield’s approach to meaning into a broader framework by examining its position in an intellectual history of recent philosophical treatments of meaning, chiefly those of Russell and Wittgenstein. Quine’s paper, “The problem of meaning in linguistics,” offers an extensive discussion of synonymy and its position in the conceptualization and analysis of meaning.

A still not outdated work by Charles W. Morris might have provided the framework we seek. This is his famous “Foundations of the theory of signs” (1939), a classic paper which might have considerably enhanced Saporta’s reader. In it, Morris sets out to carefully distinguish between different kinds of meaning: “The confusion regarding the meaning of ‘meaning’ lies in part in the failure to distinguish with sufficient clarity the dimension of semiosis which is under consideration” (1939:121). For reasons that we shall discern shortly, American psychologists and linguists are most familiar with the imperfect dichotomy between “denotative” and “connotative” meaning. This dichotomy can be related only in a haphazard manner to the different kinds of meaning which are so clearly described in Morris’ paper. A strict usage of “denotative” in Morris’ sense would restrict us to examining “the relations of signs to the objects to which the signs are applicable” (1939:84), the area of inquiry he calls “semantics” proper. But surely most linguists would want to include the study of meanings implicated by the relations of signs one to another, what Morris calls the “syntactic dimension of semiosis.” (It is the confluence of the semantical and syntactical dimensions of semiosis which underlies the distinction, often difficult to effect, between linguistic and grammatical meaning.) “Connotative meaning” relates largely to what Morris called the “pragmatical dimension of semiosis,” i.e., the study of the “relation of signs to interpreters.”

One of the best programmatic accounts of meaning is Carroll’s recent study (1964), in which he discusses, among other topics, the relationship of semantics to the categorization of experience and concept formation. The paper also contains one of the clearest expositions of the distinction between denotative and connotative meaning.

The relevance of meaning for verbal learning has received considerable recent attention. Some recent work is surveyed in Bousfield (1961). Recent papers include Deese (1962a); Goss (1961); Noble (1952); and Staats (1961), in which there is concern for the role of conditioning in concept formation. A number of different dimensions are being investigated, including the process called semantic satiation; see, for example, Lambert and Jakobovits (1960) and Miller (1963).

It is evident that what energy linguists have invested in semantic
analysis has been devoted to studying the denotative relationships between words and their referents. Among psychologists interested in semantics, many have similarly concerned themselves with this phase of semiosis. This interest is apparent, for instance, in discussions of language acquisition wherein the establishment of the contracts between signs and referents is of central concern. One theoretical approach for studying the assignation of denotative meanings is presented in Skinner's "The problem of reference," included in this part of the reader; the approach itself has been discussed earlier in Part 2.

On the other hand, psychologists have also been very much concerned with the relationship between linguistic signs and their users, the relationship which Morris calls the "interpretant" and which he treats as a central problem of pragmatic analysis. Morris' definition of "interpretant" involves assuming dispositional tendencies in the interpreter: "The interpretant is the habit of the organism to respond, because of the sign vehicle, to absent objects which are relevant to a present problematic situation as if they were present" (1939:109). It is easy to see here the influence which Morris exerted on both Osgood's and Skinner's views on semantics. However divergent the approach and focus of the latter two men are from each other and from Morris, both represent attempts to construct a theory of dispositions based on reinforcement.

The psychologist's interest in the interpretant is best seen in Osgood's development of the "semantic differential," a technique for pragmatic analysis designed "to assess certain symbolic processes assumed to occur in people when signs are received and produced" (Osgood, 1959:192). The semantic differential, derived ultimately from investigations with synesthesia, is a technique using multiple factor analysis which discloses dimensional structure within "semantic space." Various concepts are subjected to semantic differentiation along certain adjectival scales which express connotative, affective values by which the speaker-subject judges the concept.

The technique was discussed at length in a book which has become well known to psychologists and which has become an object of controversy among linguists. It is not surprising, considering its sweeping title, that Osgood, Suci, and Tannenbaum's The Measurement of Meaning (1957) confused many linguist readers. What was expected, evidently, was a new approach to the lexicographic problems viewed as the central concern of semantic analysis. But instead of a technique for analyzing the denotative meaning of lexical items, the reader encountered "a kind of dimensional analysis of their affective and evaluative meanings" (Lounsbury 1959:196).

Unfortunately, the distinctions between denotative meanings, which are linguistically relevant, and connotative meanings, which are psychological, are not clearly developed by the authors of The Measurement of Meaning. But the lack of clarity had one very fruitful result: a series of
analytic (and in one instance, critical) reviews appeared by linguists who sought to distinguish sharply between the different types of meaning and to explicate the mechanics of the factor-analysis procedures used in the semantic differential. In order of appearance, these reviews include Wells (1957), Weinreich (1958a), and Carroll (1959). It is interesting that this distinction was again blurred by Osgood in a recent paper (1963b), reporting on the quantification of meaning.

It is important that Saporta’s reader introduces the student to the semantic differential, since this technique has gained wide use in psychology. Moss (1960) reviews some of the current research. Not only is the original Osgood, Suci, and Tannenbaum book in wide currency, but so also are several derivative sources, including a “semantic atlas” of a lexicon of 360 items and a study of the distances between the concepts involved (Jenkins, Russell, and Suci, 1958, 1959). Within a relatively short period of time, the semantic differential has been adapted for use in an impressive array of investigations, including its applications “to the study of attitude change, to changes in meaning during psychotherapy, to the generality of semantic factors across language and culture, and to a variety of communication problems in aesthetics, advertising, and mass media effects” (Osgood, 1963a:272).

Particularly interesting for the reviewer were the applications of this technique to the measurement of bilingual dominance (Lambert, Havelka, and Crosby, 1958) and to clinical investigations (Osgood and Luria 1954, Luria, 1959), and its tentative application as a tool for measuring cross-cultural differences in connotative meanings. This last category includes a large group of papers: Kumata and Schramm (1956); Maclay and Ware (1961); Prothro and Keehn (1957); Suci (1960); Tanaka, Oyama, and Osgood (1963); and Triandis and Osgood (1958). The reviewer concurs with its users that the semantic differential, however inappropriately named, offers promising results.

Another aspect of psychologists’ research with the nature of the interpretant is manifest in “content analysis,” which has been recently characterized as a method for “using events in messages as a means of either drawing inferences about their sources or making predictions about effects on their receivers” (Osgood, 1963a:301). The ongoing research, its methods, problems, and findings, are ably sketched in Pool’s paper, “Trends in content analysis today: a summary” (1959), the final selection in this part of Saporta’s reader.

6. Language Acquisition, Bilingualism, and Language Change

guages in contact" (1953); U. Weinreich, "Mechanisms and structural causes of interference" (1953); E. Haugen, "The bilingual individual" (1956); and W. E. Lambert, J. Havelka, and C. Crosby, "The influence of language-acquisition contexts on bilingualism" (1958).

Scanning these titles might prompt one to ask what they all have to do with one another, and more particularly, what bearing any has on linguistic change. The thematic unity perhaps will become apparent below, but the answer in part must be that the topics in this section were taken over wholesale from Chapter 6 ("Diachronic psycholinguistics") of the 1954 Psycholinguistics monograph, which had the same organization. More important to register is the observation that many of the suggestions for research made during the 1953 Social Science Research Council conference were subsequently carried out in the articles included in the reader. This is clearly an area where psycholinguistics has much to offer for a general theory of language, and where the stimulus of the 1953 conference has already produced some of the most noteworthy gains.

It is remarkable how little attention linguists have given to the acquisition of speech in children, to the loss of speech through age or pathology, and to the consideration of those abnormal situations wherein speech is not acquired at all. It would be interesting if future chroniclers of linguistics in this country were able to show that a strict ahistorical bent in descriptive linguistic theory had successfully postponed the linguist's involvement with linguistic ontogeny until this late date.

"Language acquisition," further unspecified, can clearly refer to two rather separate developments: (1) the learning of the primary language, normally a joint enterprise of the child's maturation and socialization, and (2) the learning of a secondary language (without restriction as to the age of the speaker). All available evidence, Berlitz notwithstanding, indicates that these are qualitatively distinct developments except in those cases when a child at an early age becomes a coordinate bilingual in a secondary language. Particularly if we inquire into the later acquisition of a secondary language (and the practical and pedagogical implications which are involved in foreign language teaching), these are best viewed as related but quite separate topics.

"Linguistic ontogeny" is a more happy cover term than "learning" for the aspect of language acquisition which concerns primary language development. Moreover, it has the virtue of indicating one very important tie-in between psycholinguistics and linguistic problems that are usually considered "historical" in nature. That is, linguistic ontogeny can be understood as either concerning the child's acquisition of his language, or as concerning the impact which the children of a speech community effect on its langue, with their own innovations. Inquiry into the innovating role of the child and his contribution to linguistic change is a fascinating topic, one which has been little studied and which is unfortunately more amenable to speculation than observation. Hockett's (1950) excel-
lent article on this aspect of ontogeny has not elicited the interest it de-
served, and hopefully it will be selected for inclusion in some future
reader.

The child’s acquisition of language, however, has a long history of in-
vestigation, and today many of the psychologists interested in language
are concerned with this subject in particular. Perusal of the contents of
recent linguistics journals reveals that this interest is no longer exclusive
to the psychologists.

It is noteworthy that longitudinal studies of children’s development of
verbal behavior have generally examined the acquisition of either the
reference system (content) or of the linguistic system (expression); only rarely has the total integrated task been investigated. This is abun-
dantly clear when we examine those studies centrally concerned with the
child’s acquisition of a reference system and his exploitation of that
system in perceptual and cognitive development, as manifest in the child’s
evolving conceptualization of his world. Relevant here are the studies of
Piaget and his colleagues and students in Switzerland, of Ljublinskaja
and others in Russia, and of Bruner and his associates in the United
States. Piaget’s work is rapidly growing; much of his earlier work is
reevaluated in Piaget (1955) and Inhelder and Piaget (1958). Flavell
(1963) offers a recent comprehensive introduction to this school. Luria
and Yudovich (1959) is an exemplar of a large body of recent Russian
literature on concept formation; additional earlier references are con-
tained in Luria (1959a).

Bruner, Goodnow, and Austin’s *A Study of Thinking* (1957) exempli-
fies the heavily experimental bent of some of the current research on
concept formation in the United States. Some earlier general considera-
tions on concept formation are contained in Heidbreder (1945, 1948);
Levit (1953); and Vinacke (1951, 1954). Carroll (1964) has supplied
the most readable and insightful of recent articles on concept formation;
both this and his 1963 paper are discussed below. Church’s *Language and
the Discovery of Reality* (1961) has an informative discussion of the
development of thought concerning concept formation, and Hunt has
written a recent monograph on the topic, entitled *Concept Learning: An
Information Processing Problem* (1962). (Some contributions of verbal
learning experiments to concept formation are discussed in Part 5.)

The accusation that much of the European work on concept formation
is a-linguistic in approach seems justified. To be sure, the child’s acquisi-
tion of a new concept (e.g., measurement of space) is inferred from the
child’s new verbalizations, but Piaget, Heidbreder, and others, are inter-
ested in this event as a manifestation of the child’s maturing intellect,
and not in the linguistic correlates. As Berko and Brown have com-
mented, “Piaget is inclined to see through words as though they were
not there and to imagine that he directly studies the child mind” (1960:
536).
More serious criticism is offered by Carroll, who objects also to the term "concept formation" itself, claiming that the concepts involved were often formed at an earlier age. He suggests that "most problem-solving situations involve concept evocation rather than the formation of new concepts" (Carroll, 1958c:196). Osgood has different reservations; he points out that there have actually been very few studies of concept formation, and that, moreover, what is involved "is essentially the problem of learning meanings" (Osgood 1963a:228).

Carroll has concluded a recent article with a summary justification for this alinguistic approach:

It has been necessary . . . to point out that concepts are essentially nonlinguistic (or perhaps better, alinguistic) because they are classes of experience which the individual comes to recognize as such, whether or not he is prompted or directed by symbolic language phenomena. Because the experiences of individuals tend to be in many respects similar, their concepts are also similar, and through various processes of learning and socialization, these concepts come to be associated with words (Carroll 1964:201,2).

Dramatic support for Carroll's and similar assertions concerning the independence of concept formation from productive linguistic skills comes from the investigations of concept formation in the absence of language, as in deaf children. Pertinent here are the carefully executed and thoughtfully presented experiments by Furth (1961). In passing, one must note the exclusion of any study of "concept formation" in the Piaget vein from this section of Saporta's reader; nor did the topic receive anything but brief mention in the 1953 SSRC conference.

More linguistically oriented expositions of the child's acquisition of reference are discussed in several of the papers in this part of the reader. Brown's chapter, "The original word game" (1958), is very instructive. Its purview of the total task facing the child acquiring language is more comprehensive than either the Carroll (1960) or Leopold (1953/54) selections in the reader, which touch only fleetingly upon the acquisition of reference.

Linguistically oriented studies specifically devoted to the problem of the acquisition of the reference system are few. Berko and Brown's (1960) study contains a section on the methodological problems involved, and offers some possible testing procedures for assessment of the child's acquisition of general rules of reference. Riess's (1946) investigation of the associative generalizations of conditioned responses to homonyms and synonyms is interesting, although it is Berko and Brown's evaluation of the findings which are important. Commenting of Riess's discovery that older children generalized responses to synonyms, in contrast to the younger children who generalized most readily to homonyms, they conclude: "Here is evidence that responses trained to one word will be extended to others linked with it through the reference system" (Berko and Brown 1960:539).
Werner and Kaplan's "The acquisition of word meanings: a developmental study" (1950) was one of the earlier examples of a series of experiments in which, typically, referential acquisition is examined by having subjects factor out or accrete meanings to nonsense words (which are usually imbedded in larger meaningful contexts). Another example of this type of study is that of Spiker, "Experiments with children on the hypothesis of acquired distinctiveness and equivalence of cues" (1956).

A paper by Lenneberg (1957) carries these experiments further and attempts to construct a labelling experiment for adults, using color classification learning tasks which would be an analogue to the child's. This study prompted Rubenstein and Aborn to conclude that

...the task faced by the child in learning a language is considerably different from the usual adult language-learning task, where the classification of referents may require only a shift in labelling, or acquisition of classification schemes that are parallel to already conceptualized schemes. To the child, language learning involves the acquisition of classification schemes where dissimilar stimuli are sometimes given the same name and where similar stimuli are sometimes given different names (1960:307).

An interesting developmental study of children's reference is reported on by Ervin and Foster (1960).

The most elaborate psychological discussion of the acquisition of meaning is found in the writings of Osgood and his commentators, where it forms a necessary foundation to his behavioristic semantic theory. More extended discussion of Osgood's theory is contained in Parts 2 and 5 of this survey, but a recent observation by Lambert is relevant here since it points up a major gap and block to the study of the acquisition of meaning:

For Osgood, the meaning of a sign or symbol is the mental or neurological counterpart, in attenuated form, of the actual emotional and behavioral responses which have habitually been made to the referent for which the symbol stands. That is, linguistic symbols are originally learned in a context where they are repeatedly paired with their appropriate referents (Lambert, 1962:5).

The obvious problem is that work in this area has either proceeded without reference to concurrent linguistic development (e.g., Piaget), or has had to rely on purportedly analogous experimental conditions involving subjects, often adult, who have already acquired their primary language (e.g., Lenneberg's study mentioned above). The sad fact remains that little experimental psycholinguistic work has been directed to that key integrative development in the child which Lounsbury succinctly characterized as "a very extensive naming vocabulary based on recognition of both static and dynamic qualities of things in the environment, with the vocabulary in part already morphologically differentiated" (1963:556). As we can see, most acquisition studies have concentrated "on mastery of the code for referential function" (Hymes, 1962:39).
When can the child be said to have acquired his language? There is clearly a difference to be drawn between the continued learning of new vocabulary, derivational patterns, and peripheral optional syntactic structures that will occupy the individual throughout his life-span, on the one hand, and the acquisition of phonology and the basic grammatical apparatus (and its functionings) which are the key issues in the child's acquisition, on the other. The articles included in Saporta's reader do not discuss these qualitative differences between early learning and later accretions to the basic schemata (or later learning of secondary languages).

Menyuk, in two papers (1963a, 1963b), is concerned with adapting Chomsky's partial generative grammar of English (1957, 1962b) so that it can account for some of the closed-system competence of young children's speech as well as developmental processes in later continuing acquisition. Her recent investigations have led her to conclude that "the data obtained seem to indicate that the children in this population, at age 8, have incorporated most of the basic generative rules of grammar that we have thus far been able to describe and are using these rules to understand and produce sentences" (1963b:438).

More recent research has yielded additional conclusions about developmental trends in child language learning. Brown and Fraser (1963) and Brown and Bellugi (1964) provide many insights about the developmental aspects of sentence formation (see below). Brown and Berko (1960), Ervin (1961), and Entwisle, Forsyth, and Muuss (1964) all offer mutually corroborative data suggesting that children's word associations are markedly different from those of adults. Ervin (1961) concluded that this difference involves a simple shift from the child's syntactically contiguous associations to the adult's predominant use of paradigmatic replacement associations. Apparently association performances are marked by age differences in much later life as well (see Korchin and Basowitz, 1957).

Nor is the question of developmental stages, and earlier as opposed to later learning, of purely psychological import. Several writers have suggested that many of the processes of historical change in language can be understood only in terms of a psychologically determined structural stability. One clear case is the several times offered hypothesis that the differential persistence of "peripheral" as opposed to "core" elements in language structure is directly related to early learning. This hypothesis is offered in both of the larger studies from which the Weinreich (1954) and Haugen (1956) papers were taken, and found earlier expression by Tesnière (1939) and Whitney (1881), among others. The notion that the hierarchical structure of language is differentially susceptible to the pressures which produce linguistic change is too important psycholinguistically to be left dangling.

If the studies available have not come to grips with these basic ques-
tions, they have at least made great progress in the descriptive characterization of what is involved in early learning. It is with the tasks posed by this characterization that the Carroll (1960), Jakobson and Halle (1956), Leopold (1953/54), and Berko (1958) papers in the reader are concerned.

Carroll’s paper contains a good bibliography, which includes references to a number of excellent survey and bibliographic studies. Especially to be noted are W. F. Leopold, Bibliography of Child Language (1952); M. M. Lewis, Infant Speech: A Study of the Beginnings of Language (1951); and D. McCarthy, “Language development in children” (1954). It touches upon various problems in research methodology and discusses work directed toward the description of various pre-language stages of development, such as the constitutionally determined “cooing” and “babbling” states. Unfortunately there is little said (except by way of acknowledging that it exists) about the process of differential reinforcement which fixes the child’s language-learning propensities onto the code of the particular language spoken in his community.

The Jakobson and Halle paper, well known in its original larger context, The Fundamentals of Language (1956), offers an interesting but as yet untested hypothesis for the sequential development of phonology in the child, which purportedly obtains (at least in certain fundamental developments in feature contrasts) in all languages. Although certain general propositions of this hypothesis can be accepted, the more detailed developmental sequence, which is also assumed to be universal, has never been attested empirically, nor do the authors offer any suggestions for carrying out the necessary experimentation. The Leopold paper in Saporta’s reader is in some respects repetitious of Carroll’s, and one of his other papers (e.g., 1939-47, 1948, 1952) would have been a more valuable inclusion in the reader.

With respect to the child’s acquisition of grammar, most earlier studies agreed that children tend to generalize on the basis of the most productive morphophonemic, inflectional, and derivational rules, to the extent that they “over-generalize.” Berko and Brown summarize these findings to say “that small children produce new forms by analogy with words they already know, and the direction of the new forms is toward eliminating the irregularities and inconsistencies of the language . . . the equivalent of saying I ringed and two mice” (Berko and Brown, 1960:542).

To Brown and his associates goes much of the credit for devising tests of the child’s processes of acquisition. The language-acquiring child being somewhat less than an ideal experimental subject, some of these tests have been rather ingenious. This section of Saporta’s reader might have been enhanced by a paper discussing the research problems involved in studying child language, since the problems are many. For example, is the “obtained/not obtained” criterion in children’s speech an accurate analogue of “grammatical/nongrammatical” in the adult’s? How does one decide
when a novel combination occurs, and more important, whether it is the result of imitation or the extension by analogy of some already acquired implicit rule? Berko and Brown's (1960) study of research methods would have been the ideal inclusion. However, Berko's 1958 paper, "The child's learning of English morphology," is typical of the experimental approach advocated, and it is very gratifying to find it included. Irwin’s study (1960), appearing in the same volume as the Berko and Brown paper (Mussen, 1960), offers complementary information on experimental work, and is useful in addition for its references on childhood language disorders.

It is surprising to find that there are few corresponding studies concerned with the child's acquisition of phonology. The Albrights' (1956) study "The phonology of a two-year old child" would have made an interesting complement to Berko's paper, and so also Irwin and Chen's (1946) study of the appearance of phoneme (contrasts) in child speech, if accompanied with needed editorial cautions. Evidence is accumulating (and this does not emerge from either the Albrights' or the Irwin and Chen study) that the acquisition of phonology is inextricably bound up with the learning not of the important paradigmatic contrasts and their detailed environmental expressions, but rather of psychologically larger units, words and phrases. Thus it appears "that discrimination of critical phonemic cues are developed 'incidentally' in the course of learning to behave differentially to meaningful speech units" (Osgood, 1963a:279).

Evidence is also accumulating that the child achieves a receptive prowess which is often significantly ahead of his productive control. The fact that receptive control can exist even without production has been recognized, as in the case of such abnormal conditions as anarthria, in which the child (because of congenitally conferred motor defects) never acquires a speaking ability in his language. Lenneberg (1962b) has reported in detail on one such case, typical of many others, in which a patient without productive skills nevertheless had acquired language in the sense that he possessed the grammatical schemata necessary for decoding, i.e., for understanding. This study in particular strikes deep at those motor theories of language acquisition which attach primary importance to the roles of imitation and the developing motor speech skills, and it lends support to the view that decoding as well as encoding skills are "dependent upon the acquisition of a single set of organizing principles" (Lenneberg, 1962b:424).

Brown and his associates have been very active in further pursuing the child's acquisition of language and in experimenting with many of the variables such as "imitation," so-called "telegraphic speech," etc., which have traditionally posed problems to its investigation. It is interesting to note that Brown and Fraser (1963) and Brown and Bellugi (1964) are currently entertaining the hypothesis that child speech is a systematic reduction of adult speech, in which the telegraphic style of the child is
just one part of "an interaction cycle between the mother and child that amounts to a pair of reciprocal transformations: the child reduces its mother's speech and the mother expands the child's" (Brown, 1962:4). They have been busy experimenting with tests to control "imitation" as opposed to "production" (as an encoding event). Not the least of the questions involved is whether or not the child can engage in accurate repetitions of sentences without first having gained receptive mastery over the syntactic structurings involved (Fraser, Bellugi, and Brown, 1963).

The surveyor would like to recommend some topics for consideration in future investigations in this area of the child's developing linguistic competence. One possibility would be to examine the extensive descriptive information which is already available documenting "baby talk" in various languages. The problem with much of this descriptive material, however, is that it does not describe actual conversational interaction between the adult and child, although evidence from work with retarded children suggests that there are hitherto unappreciated regularities in the adult speaker's conversational reductions (e.g., see Spradlin and Rosenberg, 1964). A related matter is the as yet unanalyzed conversational interactions between children. Again, evidence from the study of retarded children suggests that there is much still to be learned about the structure of such dialogue (e.g., see Rosenberg, Spradlin, and Sanford, 1961). In view of the fact that little is known about the characteristics of adult conversational speech, this lacuna is less surprising. The investigation of child-adult nonverbal interaction has hardly begun, although the role of experimenter-bias (using both verbal and gestural cues) in children's word associations has been briefly examined by Rowley and Keller (1962).

It is apparently true that children go through a developmental sequence characterized by certain universally occurring maturational sequences, and in the process of socialization, begin to speak the language of the community wherein they live, sometimes on the basis of relatively brief exposure to it. This has the ring of a truism. Nevertheless, we are forced to say "apparently" because we lack good cross-cultural data on children's acquisition of language, although Lenneberg and others have recently addressed their attention to this question. But such evidence as can be presently mustered strongly suggests several hypotheses which are crucial to a comprehensive theory of language and verbal behavior.

The first hypothesis is that there is a critical age for language acquisition, a conclusion drawn from the study of various speech disorders, from the study of interrupted acquisition, and from the study of second language learning. (An irrefutable case for the existence of a critical age for language acquisition will be embodied in Lenneberg's forthcoming book, The Biological Foundation of Language.) A second hypothesis comes from appreciating that the child, with a finite memory and within a very brief time-span, acquires the sorts of linguistic competence which
enable him (as either hearer or speaker) to cope with an *infinitude* of utterances in his language, to either understand them or to produce them in the appropriate contexts. The realization follows that language competence develops without the reinforcing contingencies which Skinner and others would have us believe are crucial. This is because language acquisition now appears to be more critically bound to constitutionally determined maturation and to mere exposure to the language, than to a learning situation characterized by impossibly large numbers of grammatical tokens and corrections by adults. Some of the consequences of this realization for a theory of language have already been discussed in Part 4. The study of language acquisition is an area within psycholinguistics from which we can expect many exciting developments.

There are many interests affiliated with the study of language contact and bilingualism. The full range of these interests is better caught by Weinreich and Haugen in their respective reports delivered at the Eighth International Congress of Linguists (Weinreich, 1958b; Haugen, 1958) than in the papers of theirs which are included in Saporta's reader. The main fields of interest have been, first and foremost, the linguistic analysis of the results of bilingualism (i.e., linguistic borrowings) and only secondly, the sociological setting in which language contact and bilingual behavior occur. Neither of these areas are immediately relevant here.

There are, however, a number of problems involved in bilingual behavior which are of crucial concern for psycholinguistics. And as a general introduction to bilingualism, Saporta could not have assembled a better group of papers for his reader. Weinreich's *Languages in Contact* (1954), the book from which his two papers in the reader are taken, and Haugen's *Bilingualism in the Americas* (1956), from which the Haugen selection is taken, regardless of their increasing outdatedness, are the two most authoritative surveys of bibliography and research problems available. Vildomé's new book, *Multilingualism* (1963), offers insightful theoretical discussion of the topic and includes a bibliography useful for its up-to-dateness. A recent study of linguistic borrowing of note is that of Deroy (1956). Numerous recent papers dealing with the sociological aspects of bilingualism are enumerated in several of the survey studies mentioned in footnote 4, especially Bright (1963) and Lounsbury (1961), and older references are included in Weinreich (1954) and Haugen (1956).

Lambert's is among the most authoritative work currently being conducted on psycholinguistically interesting problems in bilingualism. The Lambert, Havelka, and Crosby (1958) paper in Saporta's reader is only one of several very competent studies which have been undertaken by Lambert. Some of the other research which he has conducted or participated in is indicated in the brief bibliography appended to the 1958 paper. A far better review of Lambert's work, however, is contained in his own recent survey (Lambert, 1962). In this survey Lambert suggests that,
as a central frame of reference, "it is of psychological interest to understand how bilinguals can learn two symbols for each referent and yet manage to use each language system with a minimum of inter-lingual interference" (p. 27). This paper is primarily devoted to turning up psycholinguistic insights for second language teaching. But actually, the two sections of relevance here ("A psychology of bilingualism" and "A social-psychology of second language learning") are anything but pedagogical in their orientation, although the implications for education are certainly present. For example, in discussing the age of learning of a second language, and having mentioned the traditional fears of deleterious effects of the process on personality and on intelligence, Lambert makes the startling disclosure that the bilingual subjects in his experiments "are far superior to monolinguals on both verbal and nonverbal tests of intelligence" (p. 30); this is attributed to a possibly greater "cognitive flexibility." An important dimension in successful second language learning is "an appropriate attitudinal orientation toward the other language" (p. 37).

These and many other observations provide a wealth of new insights into second language learning in both its applied and theoretical aspects. Further and more detailed results of the Montreal research with second language learning are contained in Lambert, Gardner, Barik, and Turnstall (1963) and in the important report by Peal and Lambert (1962). This group has also done several studies on attitudinal dimensions of second language learning (e.g., Anisfeld, Bogo, and Lambert, 1962). Ervin and Osgood's distinction between "coordinate" and "compound" bilingualism, which they proposed in the 1954 survey, has been widely used in experimental work dealing with bilingual behavior. This distinction is based on variation in the mediation model of acquisition, viz., whether referents are separate or merged. Osgood indicates elsewhere (Osgood, 1963a:285 f) that he believes the psycholinguistically important issues in bilingualism relate to the determination of the "degree" and "type" of bilingualism in the individual, "type" to be expressed in the coordinate/compound dichotomy. Attempts to measure language dominance in multilingual persons have encountered some difficulties for the experimenters; see, for example, Lambert, Havelka, and Gardner (1959) and Diebold (1963). More information on dominance is provided by recent experiments with interlingual word associations (Kolers, 1963) and with bilinguals' projective content (Ervin, 1964).

We can thus see that the central concern is how the bilingual speaker becomes "inputted" for two language codes and then how the codes are used differentially. "How the codes are kept separate" loads the question, for empirical evidence suggests that two (or more) codes are rarely employed by the individual without interferences of various sorts from one to the other. Overt manifestation of interference is relative, of course, ranging from hardly perceptible (e.g., in the case of the coordinate
bilingual who is "code-switching") to very obvious (e.g., in the strong "foreign accent" and grammatical errors of the subordinately bilingual speaker in his secondary language). But Lambert, Ervin, and others, have been less interested in the linguistic quantification of interlingual interference than in dominance, associative interferences, recall, and biculturality. A case in point is the Lambert, Havelka, and Crosby paper, in which associative interference is found to be a function of the social learning situation, in either "separated" or "fused" social contexts.

The omission of the role of psycholinguistics in second language teaching from Saporta's reader can be defended on the grounds that there is just too immense and specialized a literature on foreign language teaching. But no thoroughgoing coverage of psycholinguistics should fail to mention its potential contribution to those applied ventures. Foreign language teaching has already experienced various palace revolts and Copernican revolutions. But there still remain key issues in language teaching which are more than matters of efficacy of teaching methods and which depend on the skills of the psycholinguist for their ultimate resolution. Although the alert reader will be able to conjure up many more, let me mention here only one issue with which psycholinguists have become involved: the student's aptitude for second language learning. John Carroll has been especially active in this area, having devised a language aptitude test of great promise. The psycholinguistic import can be grasped immediately from some of the factors involved, such as associative memory, inductive language-learning capacity, etc. (see Carroll, 1958a, 1962a).

"Linguistic change" is related to "bilingualism," if the latter is viewed as one possible mechanism responsible for the former. There are no papers in the reader specifically devoted to linguistic change. The important contributions of psycholinguistics to the study of linguistic change probably lie ahead and perhaps outside the area of bilingualism. Any theory of linguistic change will have to account for the other principle types of change involved, particularly sound changes and the various types of analogic change. And almost certainly, such a theory will have to consider sociolinguistic factors which are beyond the scope of this discussion.

There are several important diachronic processes which the linguist relies upon in historical work, the etiology of which will ultimately demand a psycholinguistic explanation. Here I am thinking of the observations (embodied as some sort of ill-defined premises or rules-of-thumb in historical linguistics): (1) that sound change is structurally regular, and (2) that the hierarchical structure of language is differentially susceptible to change. The comparative method operates on just these assumptions. A proposal for the relatability of two languages have been traditionally based on the discovery of systematic sound correspondences between certain of their forms which can be regarded as "core" elements
(such as basic vocabulary and basic inflectional apparatus), relatively resistant to replacement by borrowings. Such sound correspondences are usually the result of systematic divergent descent from a common ancestral unit.

One property of sound change has discouraged attempts to explain its workings: that is its "unconscious" quality, the fact that the changes operate systematically in a language "without exception" and beyond the awareness and control of the speakers. The "sound laws" proposed during the last century were, of course, particularistic statements descriptive of phonological change in the language under study. They were universal only in the sense of their systematic operation within one language. These "laws" were thus never deduced from a general theory of linguistic change. Rather they involved a primitive induction by which the linguist merely indicated the necessary and sufficient conditions for the emergence of the particular sound change.

Attempts to deal with the causes of sound change and its regularity have met with little encouragement. Sapir, in his book *Language* (1921), proposed that linguistic change exhibited a predictable directional tendency, a vaguely stated notion of structurally predetermined change which he called "drift." Sapir's concept of drift has been widely misunderstood, especially by anthropologists who interpreted it as invoking mystical evolutionary doctrines. As late as 1953, Birdly could admonish that "no one insisted more strongly than he [Sapir] upon the individual nature of 'genuine' culture. Yet, in essential agreement with the prevalent climate of opinion, he maintained that language manifested a 'drift,' or long-range historical trend, which transcended the conscious efforts of the individuals who utilized a given language...." (1953:93). Recently much interest has come to be focused on drift, especially in regard to phonological change. Martinet's important paper of 1952 has developed into a theory of sound change. This paper, and Martinet's book *Économie des changements phonétiques* (1955) which in part resulted from it, have exerted much influence in certain circles.

The immediate relevance of analogic change for psycholinguistics needs no comment. Nor is the interest recent. The philologist Albert Thumb, in several papers and finally in a co-authored volume with the psychologist Marbe (Thumb and Marbe, 1901), was very concerned with analogic changes and their expression as lapses in language acquisition and in word and idiom formation. And Hermann Paul (1960) before Thumb offered a very coherent picture of the workings of *Analogiebildung* and its important role as a mechanism of linguistic change.

In Chapter 6 of the *Psycholinguistics* monograph, there is an excellent section on "Language change" by Greenberg, Osgood, and Saporta. The subsection on "Formal change" contains a good discussion of the principal types of linguistic change, which makes it a valuable introductory paper
on the topic. The discussions of "entropy balance" and numerous other matters with tantalizing applicability to historical problems, make the present republication of the monograph all the more appreciated.

7. Pathologies of Linguistic Behavior


In 1960 Rubenstein and Aborn reported, after their survey of the studies known to them, that "the research in the area of language disturbances has been quite unsatisfying from a psycholinguistic point of view. . . . The fact remains that we have little by way of systematic knowledge of disturbed language and that very little more has accrued during the past five years" (1960:308). I hope to show that these authors' conclusions were not entirely valid, and certainly the advances since their time of writing have been appreciable. At any rate, the papers included in this section of the reader reveal the progress in linguists' thinking about language disorders even before 1960.

One of the traps in dealing with a topic like "language disturbance" is its rather uncertain bounds. Rubenstein and Aborn, for instance, evince very catholic criteria, treating within this one category those aspects of stylistic variation which have been discussed elsewhere as "paralanguage" as well as physiologically-based disorders such as aphasia. Thus, a well-known functionally-oriented study by Lorenz and Cobb (1954) is discussed within the same framework as a well-known organically-oriented study by Luria (1958), concerned with the correlations between the site of lesions in the cortex and the recognized "types" of aphasic disorders.

The Flanagan, Goldiamond, and Azrin (1958) paper, the only one in this section that does not deal with aphasia, discusses the relationship of stuttering to the patient's anxiety. It reports on an experiment in which stuttering was experimentally conditioned to aversive stimuli. The suppression of the stuttering response was observed when the onset of the unpleasant stimulus was made contingent upon the stuttering response itself. A more general introduction to this class of disorders can be found in Johnson and Leutenegger's treatise on stuttering (1956). Current bibliographic information appears in the Journal of Speech and Hearing Disorders, and a wide range of material is discussed in Travis (1957).

Among the most dramatic effects of brain injury are the various adventitious language disorders collectively called aphasia. Two world wars
have left us with an incredible number of brain lesions, representing a
gamut of injuries and attendant impairments of behavior. Some of these
conditions could never occur naturally in a population, and most of them
for ethical reasons could never be experimentally induced. In spite of the
prevalence of these language pathologies in our population and in spite
of the availability of subjects so afflicted, the very fact that the traumata
have not been experimentally induced has limited what we have been
able to learn about the nature of the putative correlations between locali-
ization of the lesion and the extent of the damage.

Nevertheless, comparative descriptive studies are still yielding impor-
tant information. The work of Luria (e.g., 1958, 1959b) and his collabor-
ators, for instance, promises an early comprehensive picture of the
physiological correlates of various aphasias. Complementary data have
come from the study of localization of various functions in the cortex by
direct experimental stimulation. Relevant here is the earlier-mentioned
research of Penfield and his collaborators (e.g., Penfield and Roberts,
1959). Cursory comparison of Brain’s (1961) or Russell’s (1961) con-
temporary surveys with Goldstein’s (1948) of a decade past will convince
the reader that, at least as it concerns the physiological factors under-
lying aphasia, Rubenstein and Aborn’s evaluation of research in this field
is indeed questionable.

Moreover, the past decade has seen some felicitous rethinking about
the typologizing of aphasia and about the linguistic importance of aphasia
for a general theory of language. There have been some advances in the
descriptive characterization of aphasia, as in Fillenbaum and Jones’
(1962) use of the “cloze technique.” Characteristically, Saporta’s reader
has avoided any strongly physiologically-oriented papers, although the
introduction to Schuell and Jenkins (1959) contains a valuable thumbnail
history of the neurological theorizing from de Broca through the work
of Hughlings Jackson and Henry Head, down through contemporary
research.

Jakobson has been responsible for much of the rethinking about aphasia
on the part of linguists. The paper included in the reader is only one of
several from this author’s hand. The papers he has written during the
past decade all derive from ideas first made known in his *Kinder-
sprache, Aphasie und allgemeine Lautgesetze* (1941). His interest is principally
in deriving laws of implication for a general theory of language, through
examining the process and states of aphasic disintegration.

Jakobson claims that the phonological disintegration characteristic of
the aphasic’s linguistic regressions is a mirror-image of the child’s acquisi-
tion of its sound-pattern. The theoretical import of this observation
has been spelled out in more detail in a better-known study by Jakobson
and Halle (1956), which has attracted some adverse comment (Joos,
1957a).

A second observation reported by Jakobson is that there are two
fundamental types of aphasic disturbance, representing respective impairment of the two basic linguistic processes implied in speech: selection and combination. Impairment of the selection process is reflected in so-called "similarity disorders" while impairment of the combinatory process produces "contiguity disorders." Jakobson considers this basic dichotomy between similarity and contiguity disorders to override the older distinctions between "motor-productive" and "sensory-receptive" types.

That impairment in the sensory modality is rarely independent of corresponding damage to the motor modality (and vice versa) has been demonstrated clinically, and it is the purpose of the Schuell and Jenkins (1959) paper to show an even more striking unity underlying all aphasic disorders. Their study is valuable for its introductory sketch of available theories of language deficit. The running commentary on these theories is often critical, as when Schuell and Jenkins reject Jakobson’s model of aphasia on the grounds of its failure to utilize careful longitudinal studies. The main conclusion of the paper is that there is "a single dimension of language deficit rather than the multiple dimensions or typologies suggested in the past." This unidimensionality is adduced through a carefully executed Guttman Scale analysis.

Goodglass and Hunt (1958), in the next paper in Saporta’s reader, focus on the differential impairment of grammatical performance, attempting to elucidate the significance of the older distinction between "agrammatism" and "paragrammatism," the former viewed as an important variety of Jakobson’s contiguity disorder. A simple test involving control of English morphophonemics suggests to the authors conclusions markedly divergent from those offered by Schuell and Jenkins: "Our limited sample gives no support to the hypothesis that a common factor explains disturbances both in grammatical expression and in auditory discrimination of grammatical forms."

Lambert and Fillenbaum’s paper (1959) reports on a survey which tenuously confirms Weinreich’s theoretical distinction between coordinate and compound bilingualism. The distinction is based on the functional independence (in coordinate bilingualism), as opposed to functional dependence (in compound bilingualism), between the speaker’s two or more languages. If functional independence is regarded as determined by the social contexts in which the languages are acquired (and of such additional factors as the speaker’s age, the affective values attached to the languages, and the speaker’s overall competence in his secondary language), it might be expected that coordinate bilinguals would have their languages differentially affected by aphasia-producing damage while the compound would have his languages equally affected. The cases examined by Lambert and Fillenbaum, where information is provided about the extent of deficit and/or about recovery after aphasic insult, confirm the expectation deduced from Weinreich’s typology of bilinguals.
An interesting recent discussion of aphasia not included in the reader (nor mentioned in the bibliographies of those that are included) is Roger Brown's observations on the opposition "abstract-concrete" as it is customarily used to characterize aphasic speech. Chapter 8 of his *Words and Things* (1958) includes a very insightful discussion of the opposition in terms of superordinate and subordinate categories within lexical hierarchies. So considered, the opposition becomes a very meaningful one for describing the various deficits such as "categorical rigidity" and other referential difficulties with polysemy to which aphasics are usually subject.

The "pathologies of linguistic behavior" certainly do not stop with stuttering and aphasia. Some indication of the total range of phenomena which fall into this category is available in the previously mentioned works by Brain (1961), Russell (1961), and Travis (1957). Another available source is a recent synopsis by Lenneberg (1964) of language disorders clinically observed in children. Although phrased in terms of symptomatology and prognosis, this study is especially important because of its reliance on carefully controlled longitudinal studies. Lenneberg's forthcoming *The Biological Foundation of Language* will contain much additional material of relevance.

It is noteworthy that this is the one topical category represented in the Saporta reader which was not given consideration at the 1953 Summer Seminar on Psycholinguistics. The most likely reason was that the conference was held at a time before widespread familiarity with Jakobson's and Luria's writings had attracted linguists' attention to the importance which speech pathologies might hold for a general theory of language. Before this acquaintance was developed, in spite of the interest in aphasia which Bloomfield revealed in the second chapter of his influential *Language*, the study of speech pathology was no doubt also avoided by linguists because it was regarded as an applied discipline.

It is my impression that speech therapists and other specialists in speech pathology unknowingly have much to offer for psycholinguistics and for the theory of verbal behavior in general. Both Goldstein (1948) and Jakobson (1956) stress this point, and this opinion is certain to be supported by Lenneberg's forthcoming book. In the meantime, one notes with satisfaction that the study of speech pathologies is attracting ever more attention by linguists and psychologists, who are discovering—even in the therapist's rules for treatment—hitherto overlooked insights into the nature of language. Nor is this looking asfield one-sided; see in this connection a recent article written for linguists by a therapist (Taylor, 1964).

8. *Linguistic Relativity and the Relation of Linguistic Processes to Perception and Cognition*

Part VIII of Saporta's reader offers an interesting assortment: B. L. Whorf, "Science and linguistics" (1940); J. H. Greenberg, "Concerning
inferences from linguistic to nonlinguistic data” (1954), a paper from
the symposium proceedings entitled Language and Culture (Hoijer,
1954); R. W. Brown and E. H. Lenneberg, “A study in language and
cognition” (1954); E. H. Lenneberg and J. M. Roberts, “The language
of experience” (1956), part of the original of the same title; R. W. Brown,
“Linguistic determinism and the part of speech” (1957); L. S. Vygotsky,
“Thought and speech” (1939), Chapter Seven of his Thought and Lan-
in the effect of language on the reproduction of visually perceived forms”
(1957).
C. W. Morris’ different dimensions of semiosis (“the process in which
something functions as a sign,” 1939:81) were mentioned in Part 5 of
this survey. One of the dimensions distinguished was the “pragmatic,”
the relations of the sign system to its interpreters. It is this pragmatic
dimension, with all the diverse sociological and psychological phenomena
associated with it, which offers a target area for the papers included in
this final section of Saporta’s reader. The topic itself has eluded consistent
labelling. “Ethnolinguistics” enjoyed wider currency than its coinage and
amorphous reference deserved, and now seems less prevalent, although
there have been numerous attempts to revitalize the term (e.g., Fernández
term its early currency; the term’s curiously disjunctive reference is shown in
Hoijer’s (1953) review of this subfield. “Language and Culture” is a frequent title for American university courses designed to concen-
trate on this topic, and it is regarded by American anthropologists as a
core area of “anthropological linguistics.” Several reviews indicate the
prominence accorded this area in American anthropology and linguistics
(e.g., Goodenough, 1956; Hoijer, 1961; Lévi-Strauss, Jakobson, Voegelin,
and Sebeok, 1950; and Voegelin and Harris, 1947). It is interesting to note
in passing that anthropological linguistics now boasts a formidable book
of readings of its own, edited by Hymes (1964).
While not as neat as “ethnolinguistics” or “language and culture,” Sa-
porta’s descriptive chapter-heading has the merit of leaving less doubt
about the purview of its contents. The central problem dealt with in this
section is the simple and historically old proposition—constantly resur-
rected and reformulated—that language and thought are interdependent,
and that the structural particulars of one are necessarily replicated in
the other. It is significant that in recent times the proposition has been
reiterated overwhelmingly in one direction, namely, that the mental func-
tions of a group are in some way dependent upon the structure and con-
tent of the language they speak. We can subsume such propositions under
a heading called “theories of linguistic relativity” in accordance with
usage now familiar. But it must be stressed again that linguistic rela-
tivity implies a one-way directionality in the relationship between lan-
guage and thought, which has never been conclusively demonstrated to
exist. Many studies in concept formation imply such a directionality (see Lenneberg, 1962c, and Parts 5 and 6 of the present survey).

Of the published critiques of linguistic relativity, however, only one (Fearing, 1954) is at all explicit about the unexplored reverse possibility. It seems likely that the most important interdependencies will soon be shown to be in the reverse direction: that the striking universals in language structure, which are only now being fully appreciated, are dependent on cognitive schemata which are part of the human constitution. If this is true, a theory of cognitive determinism will displace further inquiry concerning linguistic relativity. We will then return to a view of the relationship between language and thought not unlike that entertained by the Greek philosophers, who saw a basic unity in human cognitive faculties such as logic and reason. It is evident that all branches of psycholinguistics are deeply involved in the resolution of this problem. Particularly important for its resolution are continued advances by psychologists in the fields of cognition and neurophysiology; the continued development of the linguist's interest in semantic theory, linguistic typology, and language universals; and the collection by anthropologists of an adequate range of cross-cultural data bearing on the relationship between "language and culture."

John Carroll, who edited a collection of Whorf's papers, poses the question as to the reasons behind the perennial interest which linguistic relativity holds. His answer is interesting: "Perhaps it is the suggestion that all one's life one has been tricked, all unaware, by the structure of language into a certain way of perceiving reality, with the implication that awareness of this trickery will enable one to see the world with fresh insight" (Carroll, 1956:7). To judge from the selections in Saporta's reader, and from the emphasis which American linguists and other social scientists place on Whorf's ideas, one could believe that this interest stemmed from his and Sapir's writing alone and was, in effect, a peculiarly New World development. This is certainly not the case.

To realize how hoary a tradition linguistic relativity really boasts, one has only to turn to any number of recent German publications devoted to that subject (e.g., Gipper, 1959; Jost, 1960) or to more accessible commentaries on the movement which Basilius (1952) has aptly termed "Neo-Humboldtian ethnolinguistics" (e.g., Fernández Guizzetti, 1961; Öhman, 1953; Shetter, 1962; and Trager, 1959). These studies show clearly how Weisgerber, Porzig, Jost, Ipsen, and others, have built various superficially similar theories of semantics and linguistic relativity on a common foundation propounded by Wilhelm von Humboldt during the mid-nineteenth century.

A proper setting would have to mention the successive modifications of von Humboldt's position and the successive contributions of these scholars, not merely in order to spell out a nice intellectual history, but to point out how this European approach has attempted to solve the
theoretical and analytical problems common to all theories of linguistic relativity. Contemporary concern with semantic theory and componential analysis should include, for instance, an examination of the development of the concept of "semantic field," originally proposed by Ipsen and subsequently expanded and modified first by Trier and then by Porzig. While the psychological underpinnings of the European studies are weak, the problems encountered by the Neo-Humboldtians are very much the same as those encountered in the American examinations of Whorf's ideas and as those currently being discovered in ethnoscience. In this respect European criticism of Neo-Humboldtian research is as important to review as the research itself. This criticism is quite abundant, see, for example, Betz (1954); Kainz (1956-62); and Konradt-Hicking (1956).

The provincialism of American ventures in linguistic relativity is the more remarkable because of the expression of the Neo-Humboldtian position in the writings of Ernst Cassirer, who has been widely read by philosophers and anthropologists in this country. It is significant, although not widely appreciated, that Cassirer changed his views on linguistic relativity, and in his later writings played down the notion that the substance of a particular language exerted specific constraints on its speakers' cognition. Cassirer actually published a self-reinterpretation (1945), and it is interesting to compare this with his earlier summary statements (see, for example, Cassirer, 1933). Lenneberg (1955) has published a penetrating analysis of this change in Cassirer's philosophy in which he compares Cassirer's well-known Die Philosophie der symbolischen Formen (1923-9) with a later essay (1944). The American provincialism is surprising also because Whorf is well-known and quoted in Germany and because one American study, a paper by Waterman (1957), offered a comparison between Whorf's idea and semantic field theory.

Whorf's writings (especially his 1949 book) had an immediate effect in this country, especially in anthropological circles, even before his writings were widely distributed. Their reception in anthropology was perhaps encouraged by independently conceived research on linguistic relativity by Dorothy Lee (e.g., 1938, 1944). A decade after the publication of the paper included in Saporta's reader, Whorf's ideas were collectively (and loosely) referred to as his "hypothesis" and the topic of linguistic relativity itself was briefly known as "metalinguistics" (Trager, 1949). There followed at least one program conceived to systematically examine the Whorfian hypothesis, the Conference on the Interrelations of Language and Other Aspects of Culture; Greenberg's paper in the reader is from the published results of this Conference (Hoijer, 1954). This was followed by the publication of a collection of Whorf's papers (Carroll, 1956).

The most recent phase of research activity in linguistic relativity began (partly as a result of the Conference and partly as individual efforts) with serious attempts to formalize the Whorfian hypothesis. Noteworthy
attempts include those of Carroll (1958b, 1963), Feuer (1953), Fishman (1960), Gastil (1959), Lenneberg (1953), and the Lenneberg and Roberts paper (1956) contained in Saporta’s reader. Fortunately, these attempts at formalization were coordinated with attempts to subject the hypothesis to experimental verification. In addition to the Brown and Lenneberg (1954), Lenneberg and Roberts (1956), and Brown (1957) papers contained in the reader, experimentally-oriented papers include Carroll and Casagrande (1958) and Flavell (1958). This most recent spate of experimental activity includes the Social Science Research Council’s Southwest Project in Comparative Psycholinguistics.

Carroll offers a null hypothesis for linguistic relativity, namely, its converse—a theory of “linguistic neutrality which would assert that mental operations and other behaviors are independent of the language in which they are carried out” (1963:2). This carries with it important implications about translatability between languages. The papers in this reader are not concerned with experimentation involving the null hypothesis, nor have there been any serious attempts in the literature to maintain such a position. This is because, on one very low level, languages do differ widely in their hierarchical lexical groupings, specifically in the ways in which superordinate categories are composed. Carroll has aptly described these differences with reference to second language learning:

Convergent phenomena occur when the referents of two or more symbols in the native language are represented by a smaller number of symbols in the second language. Divergent phenomena occur... when the second language contains a larger number of symbols and corresponding semantic distinctions than the first language (Carroll, 1963:2).

Similarly, examining the classificatory principles underlying such groupings, Lounsbury uses the “term ‘first-line distinctions’ to designate distinctions of reference which are made, in usage, by the naming vocabulary of a language; and the term ‘first-line ambiguities’ to refer to the ambiguities of reference which are present in that same usage” (1963:569). The existence of such category differences, and the attendant problems created for translation, render the null hypothesis void.

But, as most of the papers in the reader point out, this is the weakest of the claims made by Whorf. As his paper “Science and linguistics” indicates, he adduces not only lexical evidence of this sort but also structural-typological differences in grammar to support his claim that it is because of such linguistic differences that “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.”

Even on this lexical level, Whorf has been subjected to criticism for his naïve use of translation and the implicit assumption he makes that such convergent-divergent differences are somehow replicated by cognitive differences in the speakers. Lenneberg has been particularly critical of
the translation issue, in both of the reader papers on which he collaborated and elsewhere (e.g., 1953). Fishman graphically describes the research on this level of the problem as "anecdotal" and resulting merely in "an enchanting catalog of codifiability differences" (1960:327). Beyond this very trivial level (which Fishman calls "Linguistic codifiability and cultural reflections"), there are the "behavioral concomitants" to codifiability; and beyond this lexical level, the cultural and behavioral concomitants to linguistic (grammatical) structure.

Greenberg's study in the reader offers a programmatic statement of the possibilities for making inferences from linguistic to nonlinguistic behavior. It is made unwieldy, however, by incorporation of a cumbersome typology of semantic units, and it does not really come to grips with formalizing the Whorfian hypothesis itself in such a way that it could be tested. Several papers which appeared subsequently, by Carroll (1958b, 1963) and by Fishman (1960), pose the hypothesis better.

The paper by Brown and Lenneberg (1954) represents the first carefully controlled experimental attempt to verify the Whorfian hypothesis, on its lexical level. In this study, color recognition is shown to be dependent on codability factors. The paper is particularly interesting for its ingenious operationalizing of the concepts of category codability and availability. It is interesting that a later experiment by another investigator appeared to completely contradict this finding, but Lenneberg (1961) has recently demonstrated that the original correlation is still valid. The complete Lenneberg and Roberts paper (1956) adds cross-cultural and cross-language perspective to the investigation of color codability, showing the same interdependency among Zuñi speakers. The section included in the Saporta reader gives a very clear theoretical introduction to the problem of experimental verification of linguistic relativity. Brown's paper describes children's allocation of nonsense words to English parts-of-speech categories, as manifest in correct morphophonemic mastery. Thereafter, the children "take the part-of-speech membership of a new word as a clue to the meaning of the word."

A much diluted version of the Whorfian hypothesis has now been offered by Carroll: "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" (1963:12). And here the matter still rests.

None of the papers in the reader treat a related topic which is of widespread philosophical and anthropological interest. It was customary, in the days before sophistication in language universals permitted otherwise, for social scientists to take firm stands on the issue of whether the languages of preliterate speakers were in any specifiable way "primitive" as compared with the languages spoken by so-called civilized peoples. Majority opinion at the turn of the century was that a very real difference
did indeed exist, and this difference was said to lie in the poorly developed capacity for abstraction which was thought to characterize primitive languages and their speakers’ intellect; such is the argument, for example, proposed by Lévy-Bruhl (1922).

Brown (1958) has offered an original analysis of the opposition “concrete-abstract” which is assumed to differentiate primitive from civilized languages. If his account does not conclusively settle the question, he at least succeeds in giving precise definitions to the concepts involved, and this will no doubt have a beneficial effect on future discussions. Since the “concrete-abstract” opposition is usually viewed as an evolutionary progression from dominance of the former mode to the latter, the evolutionary question arises. Are there languages which in some way can be said to be more “highly evolved” than others? The only recent non-sentimental attempt to weigh the issue is an unfortunately little-read paper by Hymes (1961c).

The Vygotsky paper on “Thought and speech” is one of the most fortunate inclusions in Saporta’s reader. This chapter, selected from his Thought and Language (1934), gives some indication not only by Vygotsky’s control over continental literature on psychology and linguistics, but also over the contemporary ventures with language and thought in the United States. Vygotsky’s non-provincialism is reflected in his anxiety about a compartmentalized approach to a theory of language, in which semantics, grammar, phonetics, etc., would each become encysted disciplines—a fear which was certainly realized in subsequent developments in American linguistics. In his introduction to a 1962 M.I.T. translation of Vygotsky’s book (1962), Bruner argues quite convincingly that “Vygotsky is an original” whose approach and interpretations are difficult to place within a history of psychological thought. The advent of his 1934 Thought and Language in complete translation into English will no doubt exert appreciable effect on current thought on concept formation and other developmental approaches to cognition, and on ethnosience.

The first chapter of Thought and Language (not included in the reader), in effortless prose, proposes many of the ideas about cognitive processes which are today pretentiously presented as conceptual innovations.

The most important role the paper plays in the reader, in contrast to the other papers contained in Part VIII, is to confront the student with the proposition that speech and thought are independent, at least ontogenetically, having “different genetic roots.” The essence of his developmental theory is that

... in the speech development of the child, we can with certainty establish a preintellectual stage, and in his thought development, a prelinguistic stage. ... Up to a certain point in time, the two follow different lines, independently of each other. ... At a certain point these lines meet, whereupon thought becomes verbal and speech rational (Vygotsky, 1962:44).
Vygotsky's interest is focused mainly on those developmental phases in children during which speech and thought are, according to his scheme, independent. In this respect, his interests and experimental approaches closely parallel Piaget's, whose earlier work Vygotsky was well acquainted with. Their differences are somewhat less than Vygotsky avers, as a recent commentary by Piaget (1962) confirms. However, Vygotsky is more linguistically oriented, and my surmise is that he has been more impressed than Piaget with the later (adult) correspondences between particular language structures and the cognitive schemata of the speakers. The putative difference is easily caught in Vygotsky's words:

Piaget believes that egocentric speech stems from the insufficient socialization of speech and that its only development is decrease and eventual death. Its culmination lies in the past. Inner speech is something new brought in from the outside along with socialization. We [Vygotsky] believe that egocentric speech stems from the insufficient individualization of primary social speech. Its culmination lies in the future. It develops into inner speech (Vygotsky, 1962:135,6).

The "Thought and word" chapter of the book contains a wealth of ideas about research designs for testing various aspects of egocentric speech and concept formation, which deserve following up. The book as a whole has much to recommend it, including Vygotsky's insightful criticism of associational theory, and his sophisticated distinction between substance (analysis into "elements") and form (analysis into "distinctive units").

The paper by Herman, Lawless, and Marshall, as its descriptive title suggests, describes an experiment in which language-influenced reproductions of visual forms were elicited under various control situations involving the subjects' awareness of the impending task of reproducing the visual forms, and the exposure time for the stimulus figures. Like a number of similar studies, the experiment revolves around the subjects' tendency to reproduce amorphous or ambiguous visual forms as more similar to the referent experimentally associated with them than the original unlabelled forms actually were.

This is the place to register the absence of an old psycholinguistic chestnut: there is no paper devoted to sound symbolism or to other sound-based synesthesias like "color-hearing," which have long been observed to have communicative value. Brown has an excellent chapter in his Words and Things (1958) devoted to a review of the topic with extensive references to earlier work. Subsequent research includes papers by Brown and Nuttall (1959), Miron (1961), and Taylor (1963); visual-verbal synesthesia is discussed by Osgood (1960). The topic is lent additional appeal by Bloomfield's discussion of sound symbolism in the chapter (from his book Language) included in Part V of Saporta's reader. Since the time of Bloomfield's writing (1933), linguists have tended to ignore the problem as uninteresting, but it is obvious that recent interest in language universals will oblige them to reexamine the data, since evidence suggests
that at least one type of sound symbolism is universal. Moreover, the relevance of sound symbolism to the descriptive analysis of at least one language (English) has been amply demonstrated; see, for example: Marchand (1959); Markel and Hamp (1960); and Wells and Keyser (1961).

The Saporta reader does not document one very sensible development which has resulted from a revived interest in "language and culture"—the collection of more linguistic and cultural materials for testing some of the premature Whorfian hypotheses. This task seems to have fallen to social anthropologists, who have accepted the duties with surprising fervor. As a matter of fact, collective concern with ethnosience, with its assemblage of concepts and techniques derived from linguistics, represents one of the most productive fads in American anthropology. This review will not attempt to survey the pertinent research in ethnosience since it is not (in its present still very descriptive state) of immediate concern to the psycholinguist with his more experimental bent; moreover, a number of surveys are available.5

The anthropologists' output in this area will be of significance for psycholinguistics, although it involves several difficulties. One of these is that, without concern for the universal comparative aspects of ethnosience, research in this area will become bogged down in particularistic interpretations of individual cases, just when the trend in psycholinguistics (and linguistics in general) is swinging toward concern for language universals and a general theory of language. The need for concerted effort is caught in recent remarks by Casagrande:

...[psycholinguistics] most characteristically deals with the generic function in shaping cognitive processes, while [ethnolinguistics] is typically concerned with the comparative problem of how structural differences among languages, in both their lexical and grammatical aspects, systematically relate to differences in the cognitive processes or other behavior of their speakers. The two approaches are complementary (1963: 231).

Another problem is that much of semantic field analysis has dealt with semantic domains where the associated lexical sets have yielded easily to componential analysis. We need think here only of the many studies of kinship terminology and native biotic taxonomies to realize how loaded these fields are in favor of rigorous structural analysis. The control situation is almost ideal: the referents, as objects named, are physically discrete and can be relatively easily manipulated, linguistically if not physically. It will be interesting to see if componential analysis can be successfully extended outside of the highly structured lexical domains wherein it has been so effective.

The key problem revealed by ethnosience and componential analysis has been neatly described by Frake as that of "demonstrating the cognitive saliency of componential solutions...and of relating terminological attributes to actual perceptual discriminations" (1962:83). This poses
much the same problems concerning psychological reality (here cognitive) that the existence of linguistic primes does (there perceptual). One solution is to give a strictly formal account of the componential analysis without regard to its behavioral correlates. But this clearly removes the analysis in question from the realm of pragmatics, placing it rather into semantics. Better known examples of such semantic analyses include Haugen (1957), Lotz (1949), and Wonderly (1952). These studies may be contrasted with the formal analyses contained in Brown and Ford (1961), Brown and Gilman (1960), Burling (1963), De Soto and Bosley (1962), Frake (1961), and Lounsbury (1956), in which there is appreciably more concern with the psychological and sociological correlates of the terminological systems under analysis.

The attempt to elucidate the native speaker's cognitive awareness of the classifying principles underlying his folk taxonomy will involve the anthropologist with some knotty problems in cognition psychology. Not the least will be his behaviorist heritage, which will tempt him to apply simple learning theory concepts to induce the cognitive structure underlying the classification. Barring this regression, it will be necessary for him to understand the rapidly developing trends in cognitive theory, and it is in this respect that research in ethnoscience will become increasingly dependent upon psycholinguistics.

9. Other Topics

Many of the references cited in Footnotes 2 and 3 will convey the impression that there are hardly any bounds which can be effectively placed on the field of focus for psycholinguistics, as long as one's investigation somehow treats language with reference to the speaker. George Miller's "The psycholinguists" (1964), an evaluation of the ideas which prompt the increasing flock of psycholinguists to their seemingly disparate endeavors, is perhaps the most thoughtful. (This article is reprinted as an appendix to the present volume.)

In defense of the discipline's progress, we can take his view that semantic "accepting and interpreting is just now coming into scientific focus" (p. 31)—by which he refers to much of the research discussed in Part 5 of this survey—and let this prognosis be compared with that set forth by Bloomfield in 1933. In spite of his optimism, however, Miller views many topics and interests, which logically deserve psycholinguistic attention, as being still too much terra incognita to be explored with our present theoretical stance and available experimental and analytic techniques. He insists, for instance, that "pragmatic questions involving belief systems are presently so vague as to be hardly worth asking" (p. 31).

Similarly, there are several other topics which, no matter how early recognized, must still wait for another generation of behavioral scientists. An example is the interrelationships of social and cultural contexts to the linguistic events which occur within them, of so much concern to the
anthropologist Malinowski with his interest in language as a "mode of action" (see Malinowski, 1923, and Firth, 1957). It would be an easy task to extend this list of topics which are still beyond the pale of psycholinguistics. The reader who has carefully digested the sections of the 1954 Psycholinguistics monograph reprinted herein may even come to wonder if such a list can have an end. Like those motivated to make a personal investment in any developing behavioral science, the psycholinguist is at once both embarrassed and excited by the many unanswered questions which lie ahead.

Leaving the matter of future developments, we shall now mention a few topical areas, omitted from the preceding survey, which already possess associated research and bodies of literature. The first topic, which was not included either in the Saporta reader or in the 1954 Psycholinguistics monograph, is the area of "public opinion and mass communication." This is surely of interest to many sociologists and social psychologists. But although this area logically belongs within a general psycholinguistics, its omission from the above-mentioned treatises is a defensible one. For unlike most of the other subfields included in our topical purviews, this area boasts a developed body of literature, complete with several books of readings of its own, not to mention several research organizations (such as the University of Illinois Institute of Communications Research) devoted to its nurture. Some glimpses of the developments in this area are provided in Brown (1958) and Pool (1959), but the topic will not receive further attention here.

In contrast, the two remaining topics boast only a very scattered literature. In the remarks which follow, it will be my purpose not only to sponsor their admission to psycholinguistics (where they now belong only by default, if at all), but also to offer a very brief survey of some of the available literature of note. Earlier publications will receive more extended treatment here than in other sections above, because of the isolated nature of the relevant research activity.

In my introductory section, I called these topics "non-verbal communication" and "zoosemiotics." As we shall see, these terms were chosen because they happen to label dominant interests within the more inclusive areas of inquiry under discussion here. As might be expected, clarity of reference is sacrificed by naming a superordinate category after one of its constituent subordinates.

The surveyor feels justified in making this digression on several grounds: (i) The most important reason is that the new psycholinguistics could provide the integrative force which would reduce to order the diversity manifest in the research to be mentioned below. (ii) It would be most parochial to continue to examine human communication only in the light of that aspect of it (language) which linguists have deemed worthy of investigation. (iii) Many of the questions from these marginal areas, which earlier encouraged speculation (through want of adequate empiri-
cal data), can now be answered on the basis of new discoveries. (iv) The 1954 Psycholinguistics monograph mentions frontier areas (those below being some principal examples), and it would be useful to establish the foresight demonstrated at the 1953 Summer Seminar by documenting some of the developmental trends which its participants foresaw.

One way to approach the area of inquiry which includes non-verbal behavior is to first state that the persons most active in its investigation have been anthropologists and psychiatrists. Many commentators have been quick to auger an increasingly important role within linguistics for the study of paralanguage, of gestural systems, various speech surrogates, and the like. But there is always a limbo-like air surrounding the mention of such research. Rubenstein and Aborn (1960), for instance, mention slow development in this area, but they discuss most of the paralinguistic studies they mention within a general category of "language disturbances." Their observations are independently echoed by Hymes (1961a), who recounts how the individual or quantitatively varying attributes in speech (of central interest in paralinguistics) were pruned off by descriptive linguistic procedures as unnecessary and complicating, indeed forbidden, factors in the formal analysis of the language. Nevertheless, an awareness of such quantitative variations has occasionally forced itself to the fore in descriptive analysis.

An interesting attempt to distinguish between these and other distinctive prosodic features is found in Stockwell, Bowen, and Silva-Fuenzalida (1956). Following the reprinted version of their study in Readings in Linguistics (Joos, 1957b), an editorial comment by Joos predicts that the study of "vocalizations" constituted the (then) next frontier area for descriptive linguistics. Jakobson and Halle (1956) make a distinction between "expressive" and other (linguistic) distinctive features but fail to extend further attention to the former units.

Why is it that paralanguage has for so long escaped scrutiny? One reason may be that we tend to speak through the paralinguistic system in a manner which correctly suggests that we are much less conscious about the paralinguistic than about the linguistic channel. But as a matter of fact, it appears that whereas the messages conveyed by the two channels are in some as yet unstudied way highly corroboratory and complementary, the two must be regarded as functionally independent, and that on occasion (and according to the dictates of our particular cultural conditioning) we can use the two systems simultaneously to deliver two quite different messages.

There are many examples of the speaker's paralinguistic competence which clearly argue for its functional independence and psychological reality: the child's ability to interpret adult vocalizations before he is able to recognize or himself produce the linguistic units involved is one such instance; the proficient actor's ability to convey emotions by somehow mimicking the distinctive aspects of paralanguage which are associated
with various emotional states in off-stage life; the psychiatrist's ability to identify certain emotional states or attitudes from his patient's "manner of speaking"; all of these examples of paralinguistic competence and performance highlight the communicative relevance of non-verbal vocalizations which co-occur with verbal behavior. It is important to note that the distinction between the two channels used to be phrased as "verbal" (linguistic) and "vocal" (paralinguistic).

Paralanguage has nevertheless been of long-standing interest to individual investigators. Sapir's article, "Speech as a personality trait" (1927), was a pioneer study on which much later work has been based, particularly the work of Newman (1944) and Herzog (1949). Henry (1936) and Sanford (1942a, 1942b) also contributed valuable early papers. Hymes' review (1961a) lists and discusses several of these early studies.

In the mid-1950s there were again numerous individual studies of paralanguage. At first, a number of papers appeared from the psychiatrists' camp. Goldman-Eisler (e.g., 1956) offered interesting insights into the relationship between the rate of speech and the respiration cycle, and the correlation of these to factors of tension in neurotic patients. Like many other workers who became interested in non-verbal communication problems at this point, Goldman-Eisler has continued to conduct research in this area; many of her earlier papers are listed and discussed in a more recent study (1961). Matarazzo, Saslow, and Matarazzo (1956) investigated the duration of periods of silence and of speech in patient interviews, suggesting a technique for measuring the duration (by the "interaction chronograph") and offering proposals to account for some of the observed correlations. Kasl and Mahl (1956) suggested their own measures for verbal interaction. Mahl reported on experiments which demonstrated that certain types of "speech disturbances and silence seem to be expressive attributes that are useful as anxiety indices" (1956). The interest of psychiatrists in speech silences during patients' discourses was further manifest in Starkweather's important study (1956b), which demonstrated the agreement of a group of psychiatrists on the subtle speech modifications and silences which in prior practice they had intuitively associated with hypertension. More articles in this vein from this period are mentioned in Rubenstein and Aborn (1960).

It is unfortunate that the important contributions from the linguists' camp did not appear until after this first flurry of psychiatric articles. Pittenger and Smith's (1957) and McQuown's (1957) articles in the journal *Psychiatry* could have greatly enhanced the value of the above-mentioned psychiatrists' reports through contribution of more precise descriptive techniques. (Subsequent psychiatrists' reports on non-verbal communication have shown considerable more linguistic sophistication; see below.)
There early emerged three basic problems in connection with the study of paralanguage which required cooperation by linguists (these problems apply to the study of the gestural communication channel as well): (i) the analytic problems of how to segment the behavior observed into recurring units, to devise a notation for these, and to isolate their distinctive components; (ii) the development of a meaningful taxonomy; and (iii) the determination of the constitutional limits on variability in the behavior manifest in these channels, as evidenced by cross-cultural differences in such behavior.

Pittenger and Smith (1957) and McQuown (1957) laid important groundwork for the resolution of the first and second problems, and a recent study (Pittenger, Hockett, and Danehy, 1960) has developed and applied this groundwork to a detailed descriptive specification of the non-verbal (as well as linguistic) phenomena in a psychiatric interview. Trager (1958) outlines a taxonomic system based on this same tradition, and has since published papers (1960, 1961) showing the application of this descriptive scheme to the Tao language and culture. In consonance with many linguists’ views, Trager regards paralanguage as an independent communication system. Its exclusion from a “theory of language in the strict sense” is further suggested by Joos in a recent prognosis of trends in linguistics (1961).

While considerable progress has thus been made toward the resolution of the first two of the basic problems posed above, the third question about constitutional determinants of paralinguistic (and gestural) behavior still awaits comparable groundwork. The groundwork will involve asking many basic questions. Are some of the vocal contours (and part of the gestural repertoire) a part of human biology, basically alike for all humans and permitting only limited culturally imposed variation? Much cross-cultural work needs to be undertaken to determine this constitutional element, and along with that, much comparative primatology. But the climate is again favorable for this sort of investigation, as recent papers on the evolution of language indicate.

It might be argued that some of the published results of paralinguistic investigations are too recent and weakly inductive to appraise, and just such criticism is accumulating. For example, Hymes has asserted: “At present the focus of this work is chiefly on identifying and describing the relevant features. How these features occur relative to each other, how their distribution of occurrences interrelates with such things as situation, role, personality . . . most of this is yet to be determined” (1961a: 322). Lenneberg evinces similar disappointment when, in his review of Pittenger, Hockett, and Danehy (1960), he complains about missing “a discussion of at least the theoretical possibility of discovering a range of cues” (1962:73).

Nevertheless, when one collates the investigations carried out to date
(and there have been many more than this review has room to mention), it does seem that there are distinctive affective features which are susceptible of componential analysis. This fact emerges very clearly from the many studies in which judges are given some diagnostic task to be based on listening to recorded texts. Starkweather (1964) has recently reviewed a large number of these studies and concludes:

The studies of speech without meaningful content clearly indicate that information about the speaker is carried in the voice and is not dependent on an understanding of the verbal content. Judges of this material show considerable agreement when they are asked to have opinions about the emotion being expressed, either to identify it or to indicate the strength of the particular feeling. We have the clear impression that judgments are related to significant changes in pitch, rate and volume among other physical characteristics of the voice, but untrained judges are unable to describe these characteristics consistently.

The recency and taxonomic character of non-verbal communication and the terminological difficulties are not sufficient reasons for its neglect. Fortunately, relevant research seems to be gathering momentum, as evidenced by the recent Indiana University Conference on Paralanguage and Kinesics, from which a number of papers are now available (see Sebeok, Hayes and Bateson, 1964). The distinctive role of paralanguage has been investigated with reference to spontaneous discourse (Maclay and Osgood, 1959) and dialogue (Livant, 1963). Several very sophisticated discussions about the experimental control of linguistic and paralinguistic variables are available, of which one of the outstanding is that of Kramer (1964). Paralinguistic phenomena have entered into many conditioning experiments involving experimenter bias (see Page 2).

Related interest in kinesics has been in evidence for a decade. Better-known earlier studies include La Barre (1947) and Birdwhistell (1952). Despite the severe limitations of negligible progress in this area, a number of interesting experiments have been reported in which gesture and body position have been used as the crucial variable in studies of experimenter bias (e.g., Reece and Whitman, 1962; Ekman, 1964).

Terminological differences, not unexpectedly, plague this area. "Paralinguistics" and "kinesics" have both experienced attempts at precise definition and delineation, but there remain overlappings of conceptual categories, and the terms themselves are still not widely used. Particularly in the earlier writings of the 1950s, there existed a welter of terms for phenomena which linguists could only agree to call extra- and non-linguistic.

"Content-free speech" is only one example of such terms. To the psychiatrists who coined it, "content-free speech" referred to the second of two co-occurring communication channels inherent in speech production. The first is what linguists study as "expression," or rather the part of
expression which is the meat of descriptive linguistics, viz., the phonological and grammatical primes and their syntactic structure. Starkweather (e.g., 1956a, 1956b, 1964), Davitz (e.g., Davitz and Davitz, 1959) and others see this channel as conducting semantic information, that is, the content of speech, as linguists use the term "content." Simultaneous with content-speech is the second content-free channel which conducts "affective" as opposed to "semantic" information, by "voice quality." A sample passage from a 1956 article by Starkweather is enough to boggle the unprepared linguist-reader: "The voice quality is also thought to be under less conscious control than the content and to contain information that may be at variance with the content of the message" (1956a:121). "Content-free speech" has also come to refer to recorded bodies of speech which have been run through low-pass filters such that features conveying affective qualities are retained while linguistic intelligibility is suppressed.

By the late 1950s, one begins to encounter "nonverbal communication" as a technical term for content-free speech, as well as a cover-term for what is involved in the combined purview of paralinguistics and kinesics. The encounter could only bring dismay to the communications engineer, social psychologist, and ethologist, in whose fields that term is employed with quite different references. Investigators such as E. T. Hall (1959) have given the term wide currency in their contrastive cultural studies, in approximately the same meaning as used by Starkweather, Trager, Birdwhistell, and others.

This discussion was not intended to cover other forms of (human) nonverbal communication, such as various speech surrogate systems (written languages, so-called drum and whistle languages, and other cultural inventions), however fertile investigations of these phenomena might be for psycholinguistic theory. The relevance of such surrogate systems for the analysis of psychological reality, or what the engineer calls the "fidelity criterion," is obvious. The reader is directed to Cherry (1957) for some general considerations and to Stern (1957) for an insightful discussion of several types of speech surrogates.

Those with a psychiatric bent may be disappointed to discover that functionally-based speech "disorders" will receive only this nodding recognition. The Lorenz and Cobb (1954) study cited in Part 7 is one of only a very few references dealing with this topic that will be found in the bibliography which follows. There is a large literature dealing with such subjects as the content analysis of "schizophrenic speech," to mention only one example, and there is a pressing need for someone conversant with this literature to review it and comment on its relevance for psycholinguistics.

The third and last topic of omission, to be discussed only briefly, is that referred to earlier as "zoosemiotics" (Sebeok, 1962). Again, as with nonverbal behavior, this labels only one dominant interest within a much
larger category of pursuits ("semiotics") which logically includes it. Perhaps this topic can be best introduced by enumerating the key questions which research in this area is expected to answer for psycholinguists: (1) What is the general nature of animal communication systems?; and (2) What are the (evolutionary) origins of human language?

Those who have contributed most to the literature in this field are anthropologists, zoologists, and psychologists. Many substantive contributions of significance have emanated particularly from specialists within these fields who are best qualified to address these problems, namely, ethologists, primatologists, physical anthropologists, and those psycholinguists concerned with the biological bases of language.

It is surprising to discover, just at the time when significant substantive insights are being achieved by these specialists, that theoreticians among anthropologists tend to be adopting the same speculative stance which, before the turn of the century, enjoined the intellectual obloquy of such questions as the origin of language (see Diebold, 1964a). The major difficulty lies in the tradition in anthropological theory which (first assuming that language is the sine qua non of culture) argues on purely logico-typological grounds for various purportedly crucial differences between human and non-human communicative behavior.

This tradition has produced several widely read studies which have become as legitimately influential as they are incisive, including those of White (1949) and of Hockett (1960). The specialist can take little exception to the general arguments embodied in these papers, although there are certainly more comprehensive and psychologically palatable overviews, such as that of Sebeok (1962). Extensive bibliographic coverage of many of the more psycholinguistically relevant studies is provided in Hockett and Ascher (1964), Kainz (1961), Marler (1959, 1961), and Sebeok (1962, 1963).

The problems involving speculation arise when these typological discussions are augmented by considerations of the origin of language and its phylogenetic links with other primate communication systems. The surveyor chooses to leave prosecution of these charges to detailed, authoritative studies, such as those of Kainz (1961) and Lenneberg (forthcoming).

NOTES

1. This survey is based on an earlier study, my review of Sol Saporta, ed., 1961, Psycholinguistics: A Book of Readings, New York (see Diebold, 1964b). While many passages remain unchanged, the original study has been considerably revised and expanded, and contains a separate and enlarged bibliography. My earlier survey of psycholinguistics contained fairly extensive bibliographic coverage and a discursive (and, at times, too personal) commentary on developmental trends. A preprint version of that review was widely circulated, and I was gratified by the many critical responses elicited from the recipients. Many of their suggested changes have been incorporated
into the present survey, including bibliographic addenda and corrigenda, and for these I wish to express my appreciation.

With no offense to many others too numerous to mention, I want to acknowledge particular indebtedness to Robert B. Lees, Eric H. Lenneberg, and Thomas A. Sebeok for their comments and help; to Lois A. Levin for her research activities on my behalf; and to Bernard B. Perry, Director of the Indiana University Press, for his enthusiastic encouragement. Special thanks are hereby extended Glenn H. Matthews, of Prentice-Hall Inc., and John W. Parker, of Holt, Rinehart and Winston, Inc., for generous purveyance of their firms’ produce.

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2. In its earlier volumes, The Psychological Bulletin contains a number of surveys which are now mainly of historical interest. These include Mead (1904), Espe (1921), Adams and Powers (1929), and McLean (1936). The only other American journal which regularly surveys psycholinguistic activity is the International Journal of American Linguistics, which in its recently instituted Abstracts section, has printed collections of abstracts prepared by Susan M. Ervin. See I.J.A.L. 28. 205–9 (1962) and 30. 184–93 (1964).

3. In the pages that follow, this monograph appears cited in various ways. Sometimes the usual form for bibliographic citations is followed, viz., “Osgood and Sebeok, 1954.” Often, however, this work is referred to simply as “the 1954 survey” or “the 1954 monograph.” Reference to the “1953 conference” will be taken to mean the (1953) Summer Seminar on Psycholinguistics, sponsored by the Social Science Research Council. “SSRC,” according to prevalent convention, will be used as an abbreviation for the latter organization. Another departure from standard bibliographic citation occurs in the case of Sol Saporta, ed., 1961, Psycholinguistics: A Book of Readings, New York, which is usually referred to as “Saporta’s reader.”

4. In preparing this review, the following sources (alphabetically arranged) were found to be particularly useful for their own coverage of psycholinguistic publications and research, or for their insightful commentaries on psycholinguistics and its various subfields: Berko and Brown (1960); Bright (1963); Brown (1958); Carroll (1955, 1958c); Cherry (1957); Chomsky (1959); Cofer (1961); Cera and Musgrave (1963); Delacroix (1933); Greenberg (1963); Hall (1951, 1952); Henle (1958); Hamp (1961); Hymes (1961a, 1962); Irwin (1960); Kainz (1946, 1956–62); Lambert (1962); Loubsbury (1959, 1962, 1963); Miller (1951a, 1951b, 1954, 1962a, 1964); Miller and Chomsky (1963); Mowrer (1954, 1960); Olmsted (1955); Olmsted and Moore (1952); Osgood (1963a, 1963b, 1963c); Osgood and Sebeok (1954); Pronko (1946); Rubenstein and Aborn (1960); Saporta (1961); and Sebeok (1962). The reviewer was privileged to peruse preliminary chapters of Lenneberg’s forthcoming book, The Biological Foundation of Language, and found it to be extremely useful.

Some earlier European research (which we would now call “psycholinguistic”) is cited in Carroll, 1955, Delacroix, 1933, and Kainz, 1946. The present survey, however, concentrates on North American research activities. With certain noteworthy exceptions, this is the region where the most intensive psycholinguistic research has evolved and is being currently pursued. European efforts have been most outstanding in the domains of concept formation, physiological psycholinguistics, and general semantics. Relevant European contributions are mentioned in this survey at appropriate points.