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On Becoming a Physicist of Mind

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Abstract

In 1976, the German Max Planck Society established a new research enterprise in psycholinguistics, which became the Max Planck Institute for Psycholinguistics in Nijmegen, the Netherlands. I was fortunate enough to be invited to direct this institute. It enabled me, with my background in visual and auditory psychophysics and the theory of formal grammars and automata, to develop a long-term chronometric endeavor to dissect the process of speaking. It led, among other work, to my book *Speaking* (1989) and to my research team's article in *Brain and Behavioral Sciences* "A Theory of Lexical Access in Speech Production" (1999). When I later became president of the Royal Netherlands Academy of Arts and Sciences, I helped initiate the Women for Science research project of the Inter Academy Council, a project chaired by my physicist sister at the National Institute of Standards and Technology. As an emeritus I published a comprehensive *History of Psycholinguistics* (2013). As will become clear, many people inspired and joined me in these undertakings.

BECOMING A PSYCHOPHYSICIST

Regrettably, the term "mentalist" has been usurped by psychics, clairvoyants, and other illusionists. It should, in analogy to the term "physicist," have meant one who studies mental phenomena through experiment, measurement, and theoretical modeling. That is, in short, what I have had the privilege to do throughout my professional life. It is not for me to judge whether I have been creating illusions.

My mother was a physicist, my father a chemist. The sciences were much around in the large, happy family I grew up in. It was also a poor family, since World War II had ruined the family business and my parents had barely managed to feed the hungry kids with tulip bulbs and sugar beets. Born in Amsterdam in 1938, I went to Leiden University to study psychology.

Louvain and the Perception of Causality

Upon completing my undergraduate studies, I had the good fortune to receive a Belgian–Dutch grant to spend 5 months in Albert Michotte's laboratory in Louvain. Michotte had been a postdoc of Wilhelm Wundt and of Oswald Külpe. The latter, *primus inter pares* of the so-called Würzburg School, had (co)developed the method of "systematic experimental introspection" to study higher mental processes, such as language and problem solving. Like in psychophysics, the subject would (verbally) respond to some experimental stimulus, and the experimenter would analyze how these introspectionist reports ("protocols") systematically covaried with the stimulus conditions. (Daniel Dennett would later call this method heterophenomenology, unaware of the long-existing research tradition.) Michotte had brilliantly applied this method in his classic work *La perception de la causalité* (Michotte 1946). It revealed the exact conditions of visual motion giving rise to the immediate perception of causality, such as when one object launches another one or pushes another one away. I decided to work in his laboratory on the (potential) perception of motion braking, which does not meet Michotte's motion conditions for causality. Would the reported introspections still reveal a causal impression of one object braking the motion of another object? They did. Michotte then worked intensively with me on this apparent disconfirmation of his theory, which led to my first published paper (Levelt 1962).

As a European psychologist, Michotte was a mentalist, but he was also a nativist. His epistemology was neo-Kantian. The major categories of cognition (such as causality, substance, and reality) are innate. His experimental endeavor was to show that they are perceptually innate. The perception of causality should (and does!) even arise in the paradoxical case where the perceptual conditions are fulfilled but in such a way as to contradict experience (e.g., when a moving object diminishes its speed upon being hit by another object moving in the same direction). The later debates between the nativist Noam Chomsky and the empiricist Jean Piaget recapitulated the much earlier disagreements between Michotte and Piaget, as I worked out in my paper "Déjà vu?" (Levelt 1981). Meanwhile, Michotte has been overwhelmingly proven right on the perception of causality, substance, permanence, et cetera, in infants.

The Institute for Perception, Soesterberg

Back in Leiden, I became research assistant to my sublime supervisor, John van de Geer. He took care of my introduction to mathematical psychology, especially the exciting new developments in measurement theory created by Clyde Coombs, Patrick Suppes, Duncan Luce, and others, whose courses and lectures I followed on different occasions. Even more important, he introduced me to a leading laboratory for visual and auditory psychophysics at which he was a consultant: the Institute for Perception in Soesterberg, led by Maarten Bouman. My auditory work there was on the perception of musical consonance and dissonance and had been initiated by Reinier

Plomp, with whom I published a number of papers. I became, in particular, second author on our paper in the *Journal of the Acoustical Society of America* (Plomp & Levelt 1965), in which we demonstrated the relation between consonance perception and the critical bandwidth, another innate perceptual property.

Getting Married

Musical consonance also affected my personal life. I have been an (amateur) baroque flute player all of my life, and in 1963 I married my dear Elisabeth, a baroque musician (harpsichord and organ) and visual artist (textile paintings). We have three children and have shared life's great joys and unavoidable sorrows till the present day.

On Binocular Rivalry

My major project in Soesterberg was in visual psychophysics. It became my dissertation, *On binocular rivalry* (Levelt 1965). In normal binocular vision, the two eyes project roughly the same image on the two retinas. The small differences provide us with a fused stereoscopic image. But when the two images are very different (e.g., vertical stripes in one eye and horizontal stripes in the other eye), fusion breaks down. The two images no longer "add up" but instead compete, alternating every 2 to 5 s. The monograph addressed the role of contours in binocular interaction in two ways. First, to measure how the two eyes average the brightness impressions they receive, I presented fused discs of unequal luminance to the two eyes, as in **Figure 1*a***. The measured curves follow a "law of complementary shares." If an extra contour, a circle, is placed in the disc of just one eye, that eye's contribution to the brightness impression increases drastically, and the other eye's diminishes by the same amount. The effect is easily demonstrated with the brightness paradox in **Figure 1*b***. Fusing the identical black disc in position B yields a black image. Fusing the contoured black and white discs in position A yields a gray image, with both eyes contributing roughly equal amounts. But fusing the left black disc in position C with the same level of now uncontoured white leads to a black image. The contribution of the right eye falls back to zero. In other words, contours "drag" their surrounding luminance into the binocular image.

This also happens when the patterns in the two eyes are in rivalry. I measured the dominance durations of the two eyes' patterns in rivalry and found that they followed a γ-distribution. An unexpected, counterintuitive discovery was that adding an extra contour in one eye did not increase

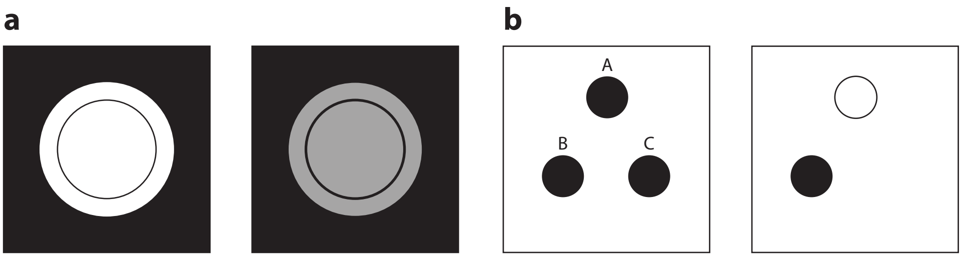

Figure 1

(*a*) Measuring binocular brightness averaging. Discs of unequal luminance are stereoscopically fused, and the resulting brightness impression is judged. If a circle appears in the disc for one eye only, the relative contribution of that eye to brightness increases drastically. (*b*) A brightness paradox. When the two images are stereoscopically fused, the disc in position A looks gray, averaging black and white to equal amounts. The disc in position B looks black, averaging black and black to equal amounts. But the disc in position C looks black, not averaging black and white. Here only the eye with the contoured black disc contributes to the brightness impression. Figure adapted from Levelt (1965).

the average dominance durations for that eye, instead only decreasing the dominance durations for the other eye. In other words, it is the contour strength of the suppressed image that is "doing the work." This became known as "Levelt's second proposition" and led to a long-lasting wave of research in bistable perception, which was reviewed half a century later by Brascamp et al. (2015).

BECOMING A PSYCHOLINGUIST

My background in linguistics so far was zero. But I had read *Syntactic Structures* (Chomsky 1957) and studied Miller & Chomsky's (1963) joint effort to introduce automata as formal models of the language user, and the idea of formal grammars and automata stirred my imagination. I decided to broaden my perspective and go for a postdoc in psycholinguistics.

Harvard's Center for Cognitive Studies

In retrospect, George Miller's letter to me, dated March 22, 1965, became a major determinant of my further scientific career. He invited me to spend the upcoming academic year at Harvard's Center for Cognitive Studies, which he was directing together with Jerome Bruner. Miller was the unchallenged world leader in psycholinguistics; his offer to me was a dream come true.

The Center was an exciting, liberal think tank. Neither Miller nor Bruner ruled; rather, they created wonderful conditions for a critical mass of talented staff and visitors to develop whatever they were after, alone or together. In his reticent, unimposing way, Miller had surrounded himself with "young Turks" who developed experimental procedures for studying the mental processes underlying sentence parsing, memory, and generation. Testing the "psychological reality" of linguistic structure and transformations soon defeated itself, but the experimental toolbox survived, as did the network of psycholinguists—my network, future leaders of the field, among them Bellugi, Bever, Blumenthal, Flores d'Arcais, Fodor, Garrett, McNeill, MacKay, Mehler, Rommetveit, Rubinstein, and Slobin.

The Center was also the brewing place for what was called the cognitive revolution. This is how I earlier described my revolutionary experience (Norman & Levelt 1988, p. 102):

> When I came to the Center, it was quickly made clear to me (but not by George Miller) that, by and large, all psychologists are behaviorists - even if they might deny that themselves - and that the Center was in the business of demolishing behavioristic doctrine and replacing it by a mentalistic approach. The polemic excitement was, of course, largely lost on someone who had been educated on an eclectic mixture of gestalt psychology, phenomenology, psychophysics, and ethology. All of these are either mentalistic or nativistic or both, and behaviorism had been so completely absent from my horizon that I didn't even know the difference between classical and operant conditioning. What had been the unmarked nativist background of most psychology that I had learned at Leyden University and with Michotte in Louvain now became the marked foreground issue. It was somewhat like experiencing the American excitement over Heineken beer, which I had always thought to be just beer.

Behaviorism had been no more than a curious local (United States) scientific aberration that would have disappeared anyhow. What were really revolutionary, however, were the entirely new formal, logical, mathematical, and experimental means to study mental structures and processes, with generative linguistics temporarily in the driving seat. Multidisciplinary cognitive science was born there. I audited Noam Chomsky's and Morris Halle's courses at the Massachusetts Institute of Technology (MIT), my only linguistics courses ever. Noam was open and helpful when I discussed my experimental plans with him.

During my stay I developed a method for deriving syntactic tree structures from subjects' syntactic cohesiveness judgments (in the sentence *The child went to school*, subjects judge the cohesion between the words *the* and *child* to be larger than the ones between *the* and *went* or between *the* and *to*) (Levelt 1969). I also published a two-paper review text in Dutch on generative grammar and its psycholinguistic applications (Levelt 1966a,b).

The Center for Comparative Psycholinguistics, University of Illinois

Charlie Osgood was a generous host for us when Elisabeth and I spent the following winter semester at his center in Urbana, Illinois. Osgood had been a leader of behaviorist psycholinguistics but was now fighting a sad rearguard battle to defend his mediation theory of meaning. Much better were his work on hesitation phenomena in speech (Maclay & Osgood 1959) and his later inventive experiments on sentence production (Osgood 1971).

And dear Elisabeth had the privilege of experiencing Victorian childbirth terror in Urbana's Carle Clinic.

GRONINGEN UNIVERSITY

We returned to the Netherlands in the summer of 1967. I had accepted a tenured associate professorship in experimental psychology at the University of Groningen, which was soon turned into a full professorship and extended with a psycholinguistic teaching assignment in the Language Faculty. I also accepted a similar teaching assignment at Louvain University, where a new psycholinguistics department was being developed. In Groningen I could build up my own laboratory, combining psycholinguistic experimentation on syntactic cohesion (Levelt 1969, 1970) with the psychophysics of binocular color fusion (De Weert & Levelt 1974, 1976) and binaural loudness formation (Levelt et al. 1972). The latter paper (on which my Nijmegen colleague Eddy Roskam should have been made coauthor) was the first application of "conjoint measurement" [a nonmetric scaling procedure introduced by Luce & Tukey (1964)] to binaural loudness. It was my binocular brightness work transferred to binaural loudness.

Ino Flores d'Arcais, then at the University of Padua, was so kind as to involve me with his organization of what was probably the first international psycholinguistics conference in Europe. It took place in the elegant hotel l'Elefante, Bressanone, in July 1969. There were some 35 participants—20 European and 15 American—substantially extending my psycholinguistic network. In particular, I met Herb and Eve Clark there and John Marshall, who were soon going to play important roles in my life. The proceedings, *Advances in Psycholinguistics*, edited by Flores d'Arcais and myself, appeared in 1970 (Flores d'Arcais & Levelt 1970).

My main experience in Groningen, however, was the outbreak of the student revolts; the protesters happened to have chosen for their headquarters the Institute for Psychology, where I had just become director. Revolutionary students took possession of our labs, stamping in wearing their wooden shoes and disrupting our delicate visual equipment. In their self-created new curriculum, they could earn their degrees by cutting sugar cane in Cuba—that sort of thing. But then, one day, a letter arrived that saved my life, my science, and our existence as a family.

THE INSTITUTE FOR ADVANCED STUDY

Duncan Luce, who was the world's leading mathematical psychophysicist, invited me to spend a year at the Institute for Advanced Study in Princeton. He had become a (semi)permanent member of the School of Social Science and was familiar with my psychophysics work. Shortly after receiving his letter, I was offered the experimental psychology chair at Nijmegen University. In a highly complicated maneuver, we packed our research staff with their delicate equipment in a van, leaving the sinking Groningen ship, and headed to the brand new Nijmegen laboratory. From there, Elisabeth and I took the family to the Princeton paradise, where we arrived on September 1, 1971.

And a paradise it was, living with my family on Einstein Drive and sharing my days at the Institute not only with Duncan Luce but also, again, with George Miller, who had moved there, and with Aravind Joshi, Philip Johnson-Laird, and science historian Thomas Kuhn.

My plan for the year was to write a comprehensive text on the theory of formal grammars and automata in their applications to linguistics and psycholinguistics. This had been on my mind since the time I had written my two-paper review text during my postdoc at Harvard. The fates were with me. Nobody was better informed about mathematical linguistics than my next-door neighbor Aravind Joshi. All year, he generously let me pick his brain on everything that was too difficult for me. By the end of the year, the three-volume book was done (in its first Dutch version). Mouton agreed to publish the English translation in its *Janua Linguarum* series, where Chomsky's (1957) *Syntactic Structures* also had appeared. Halfway through the process, my editor left Mouton, letting my book drop into a void from which it never fully escaped. When, after major struggles, it finally appeared in print (Levelt 1974), no marketing whatsoever was done. Eventually I sent my own copies to journals for review.

The three-volume book was the first text to comprehensively introduce formal grammar/automata theory and its applications in (transformational) linguistics and psycholinguistics. It treats formal learnability theory as well as probabilistic grammars and statistical inference. The latter were, at the time, off limits for generative linguists; as Chomsky (1968, p. 57) wrote, "But it must be recognized that the notion 'probability of a sentence' is an entirely useless one, under any known interpretation of this term." Curious dogma! My book also dealt, in Volume 2, with the Peters & Ritchie (1973) proof that the *Aspects* grammar (Chomsky 1965) is undecidable, a type 0 grammar with Turing machine power—a proof with far-reaching consequences.

The best-kept secret of the book is the "interpretation theory" I developed in Volume 3 (Chapter 2). It was never picked up in the (psycho)linguistic world, although it is as valid now as it was 45 years ago. It concerns the use of cohesion judgments: judgments (easily obtained!) of syntactic relation strengths between words in a sentence (as in the above example *The child went to school*) to validate the appropriateness of structural descriptions for that sentence. This can be a powerful method for deciding between alternative structural descriptions within a theory (e.g., within *Aspects*). It can also be a basis for choosing between structural theories—for instance, between phrase structural grammars and dependency grammars. Here I followed good old measurement theory (in particular Suppes & Zinnes 1963) by mathematically mapping a theoretical structure (e.g., a linguistic phrase marker) onto a matrix of measured pairwise cohesion values. This mapping is an empirical interpretation of that structure. I demonstrated the force of this approach by having my experimentally obtained cohesion measures decide between deep and surface structural representations of a sentence and between deep and shallow phrase structural hierarchies, and especially by showing that dependency grammars provide much more faithful representations of cohesion data than do phrase structural representations.

John Benjamins Publishing Company was so kind as to republish my *Formal Grammars* in 2008 as a single volume (Levelt 2008). They asked me to add a postscript on relevant developments since the 1970s. What better could I do than ask Aravind Joshi for a quick tutorial? And again, he generously helped me.

NIJMEGEN UNIVERSITY AND A MAX PLANCK PROJECT GROUP

Chairing the Experimental Psychology Department in Nijmegen was a pleasure. The Psychology Laboratory had just been built; it was the world's largest psychology lab. My experimental department was well equipped, and, more important, I inherited a number of intelligent staff. However, most of them were still working on their dissertations, and I felt responsible for getting all of them to finish as soon as possible. It worked, and I enjoyed all the resulting teamwork.

My *Formal Grammars* had also treated the operators/nucleus theory by Pieter Seuren, whom I had never met. But in 1974 Pieter accepted the chair of philosophy of language in the Nijmegen Philosophy Department. That turned out to be highly intelligent design by the Fates. Slowly but surely, Pieter Seuren became my steady anchor point on the ever more chaotic linguistic waves. Formal syntax was losing its base in semantics and formal semantics its base in cognition, but not for Pieter. The impressive oeuvre of 15 or so books that Pieter Seuren created over the following decades became for me the existence proof of an ecologically valid formal linguistics, indispensable for any viable psycholinguistics. Pieter became a dear friend and an unselfish advisor in matters of personnel and scientific direction during the pioneering phase of my Max Planck Institute and ever since to the present day.

In 1975 I received an invitation from the Max Planck Society to advise them on a new initiative. The Max Planck Society consists of some 80 research institutes in a wide variety of fields, ranging from the natural sciences to the social sciences and the humanities. In 1975 all institutes were located in West Germany, spread over the Bundesländer, the German States, with one exception: the Bibliotheca Hertziana in Rome. It had been donated to the Society in 1912. The Max Planck Society is financed in a 50/50 ratio by the German government and by the joint states. Scientific innovation is high on its agenda. New institutes and institute departments are created in promising, often interdisciplinary research areas. Any scientific member of the Society may come up with new initiatives and involve like-minded colleagues in other fields to prepare a formal proposal to the Society's president. In favorable cases, the president will appoint a committee to prepare a more detailed plan for the Society to consider. This is a wonderful, bottom-up system for scientific innovation.

The new initiative was in the interdisciplinary area of language and psychology. Two members of the preparation committee, biophysicist Werner Reichardt and psychiatrist Detlev Ploog, maintained intensive research cooperation with colleagues from MIT. Reichardt and Ploog were familiar with the Chomskyan revolution and the new developments in cognitive science. The other two members, sociologist Jürgen Habermas and educational philologist Wolfgang Edelstein, were both involved with issues of (linguistic) acculturation. The decision had been made to start a 5-year trial research group in this interdisciplinary field. The committee asked me to advise them on possible leaders, and I did.

Half a year later, they surprisingly returned to check my own interest in such a position. Negotiations followed among the committee, Max Planck president Reimar Lüst, Nijmegen University, and myself with the following outcome: I would establish and lead the temporary Max Planck Project Group in Psycholinguistics, to be located in Nijmegen. The university would "second" me to the Max Planck Society for the 5-year period while I kept my tenured professorship. The university would establish an interdisciplinary peer research group for the same period. We were very lucky to appoint the brilliant John Marshall as head of that research unit. All this officially started on September 1, 1976. This was the first time the German Max Planck Society established a new research institution outside of Germany, in a neighboring country, and this did not go unnoticed. There was immediate wide resonance to this innovative "European move" of the Max Planck Society, helping us to get off to a flying start. There clearly existed a highly receptive climate for this new interdisciplinary field of language sciences. We soon became a European attractor for peers from all over the world.

One of my first moves was to visit Manfred Bierwisch in East Berlin. I was not alone in considering him Europe's leading generative linguist. He should have become the Project Group's leader, but that was impossible. There existed no science treaty between East and West Germany. Manfred was critical about the DDR regime and refused to become a party member. As a consequence, he was kept in a minor position in his Academy Linguistics Institute, couldn't publish, couldn't travel, ended up in jail now and then, and lived in an apartment packed with hidden microphones (as he found out when the Stasi archives opened). But he had a marvelous private library because there was a tacit agreement that any visiting scientist from the West should bring him the latest books and papers. And so I did on October 11, 1976. Manfred received and advised me generously; we became friends on the spot. He took me on a walk through town and to cafés to discuss matters—for good reason. Wolfgang Klein and I edited a Festschrift (Klein & Levelt 1981) for Manfred's 60th birthday in 1981.

Another first move was for me to ask 31-year-old linguistic "Wunderkind" Wolfgang Klein, professor of linguistics at Frankfurt University, to join me in leading the Project Group. He accepted, and from April 1977 he was seconded by his university to our Project Group. From then on we jointly put the Nijmegen Max Planck enterprise on the rails. Klein was doing fundamental work in computer-based syntactic parsing. He also taught mathematical-linguistic approaches to poetry, was the first to apply probabilistic grammars to variation linguistics, and ran a project on untutored second language acquisition in foreign workers. He introduced me to issues of deixis and anaphora, which would become quite central to our research.

The Project Group, eventually consisting of 10 research staff and 6 PhD students, plus liberal technical, library, and administrative support, concentrated its research in two areas: adult and child language. But we avoided mainstream topics. We went for adult language production in context, working on the generation of deictic and anaphoric expressions, on ellipsis, and on linearization. We organized an international conference on this topic, published as Jarvella & Klein (1982). Together with John Marshall's research unit, we obtained a Dutch grant to study "descriptive language," especially the production of referential expressions. The main chosen topic in child language was the child's growing "conception of language," linguistic awareness, and the role of this awareness in language acquisition. We celebrated the official opening of the Project Group on May 3–7, 1977, with a lecture by Jerome Bruner titled "Mother–Child Interaction and Early Linguistic Awareness," immediately followed by an international workshop on "The Child's Conception of Language," published as Sinclair et al. (1978).

Jerry Bruner had agreed to become chair of our Scientific Council. Over many years in that role, he made important contributions to trailblazing the Project Group and then, from 1980 on, the Institute. He modestly described these contributions in his autobiography (Bruner 1983, pp. 174–76). Jerry Bruner died in 2016 at the age of 100. The Institute obtained his personal library, which is now on display and accessible to any interested scholar. It is our tribute to the critical and generous role Jerry Bruner played in the initial phase of our Max Planck research enterprise.

Last but not least, we were happy to appoint three external advisors who were willing to spend considerable time and effort in Nijmegen: Annette Karmiloff-Smith, John Morton, and Ino Flores d'Arcais.

A MAX PLANCK INSTITUTE FOR PSYCHOLINGUISTICS

The new Max Planck Institute for Psycholinguistics came into being on January 1, 1980, with Wolfgang Klein and myself as directors. Its research was to concentrate on three domains: the production, comprehension, and acquisition of language. It exceeds the purpose of these autobiographical notes to comprehensively sketch the developing research in my Institute. I will rather concentrate on the work of my own team in the Language Production Group. However, the

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The Acquisition Group's work on first language acquisition concerned lexical development (Werner Deutsch), grammatical development (Melissa Bowerman), development of discourse skills (Annette Karmiloff-Smith, Maya Hickman, and Jürgen Weissenborn), and the acquisition of real-time comprehension skills (Lorraine Tyler and Angela Friederici). Work in second language acquisition largely concerned Wolfgang Klein's longitudinal cross-linguistic project on the untutored language acquisition of 40 adult immigrants from different language backgrounds in England, Germany, France, the Netherlands, and Sweden. It was coordinated by Clive Perdue. The Comprehension Group's work focused on four issues: theoretical accounts of form/meaning relations (visitors Barbara Partee, Emmon Bach, Marc Steedman, and Heidi Altmann), lexical processing (William Marslen-Wilson, Lorraine Tyler, Pienie Zwitserlood, Ino Flores d'Arcais, and Herbert Schriefers), text comprehension (Wietske Vonk and visitors Uli Glowalla and Dieter Metzing), and language universals (Jack Hawkins). My own Production Group focused on speech repairs (with visitors Ewald Lang and Anne Cutler, and Gerard Kempen from Nijmegen University), intonation (Thomas Pechmann and Anne Cutler), and deictic reference (Veronika Ehrich, Bob Jarvella, and visitors Graham Richardson and Wido Lahey). We had, finally, obtained a major grant from the Netherlands Research Council for a project on aphasia in adults, which would run till 1991 (initially involving Claus Heeschen, Angela Friederici, Marie-Louise Kean, and Paul Eling, with Herman Kolk from Nijmegen University). Our very active and influential Scientific Council consisted of Jerry Bruner, Eve Clark, Merrill Garrett, Bruno Hess, Jacques Mehler, Detlev Ploog, Helmut Schnelle, and Dieter Wunderlich.

sidebar titled The Institute in 1982 gives a feel for the Institute's pioneering state through presenting some data from our 1982 Annual Report.

DISSECTING THE PROCESS OF SPEAKING

My team's research on the process of speaking went through four phases. Though slightly anachronistic, the best way to represent them is with the so-called blueprint of the speaker (**Figure 2**), which appeared in my book *Speaking: From Intention to Articulation* (Levelt 1989). It is a theory under construction, representing the major processing components involved in the generation of utterances. The main partitioning, on the left side, is between processes of conceptualizing, formulating, and articulating. These processes have access to relevant knowledge bases, such as the lexicon, situational knowledge, and the developing discourse model. Also deeply involved with our generation of speech is our speech-comprehension system, depicted on the right side of the diagram. Its functions in speaking include updating the discourse model and self-monitoring. The blueprint was at the same time the crystallization of our evolving research program, which developed in four somewhat overlapping phases.

Phase I: Ins and Outs

My main three topics during the pioneering years of the Project Group and Institute concerned linearization, perspective taking, and self-repair. A major aspect of planning our speech is deciding on the order of mentioning the information we intend to transmit. When you describe your apartment, as Linde & Labov (1975) demonstrated, you must map a three-dimensional configuration onto some linear order. I have coined this "the speaker's linearization problem." This is an "in"-problem for the speaker: how to start top-left in the blueprint? My experimental approach was to have subjects describe spatial networks as in **Figure 3*a***. It turned out they moved or jumped

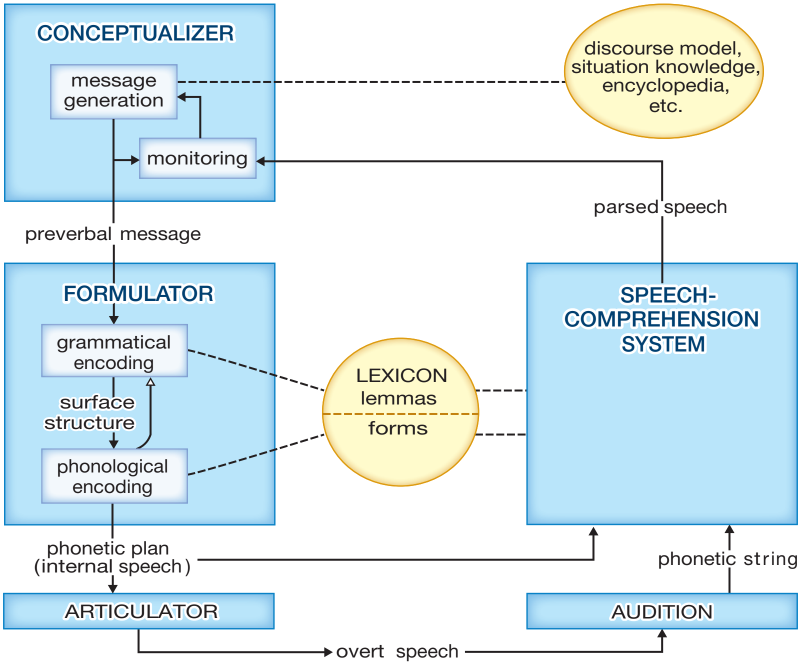

Figure 2

Blueprint of the speaker. Boxes represent processing components; the circle and the ellipse represent knowledge stores. Figure adapted with permission from Levelt (1989).

through such networks in highly systematic ways, following three linearization principles. It was possible to capture these linearization strategies in an augmented transition network, which runs through these and similar networks just like my dear subjects did (Levelt 1982). The principles are of a quite general nature.

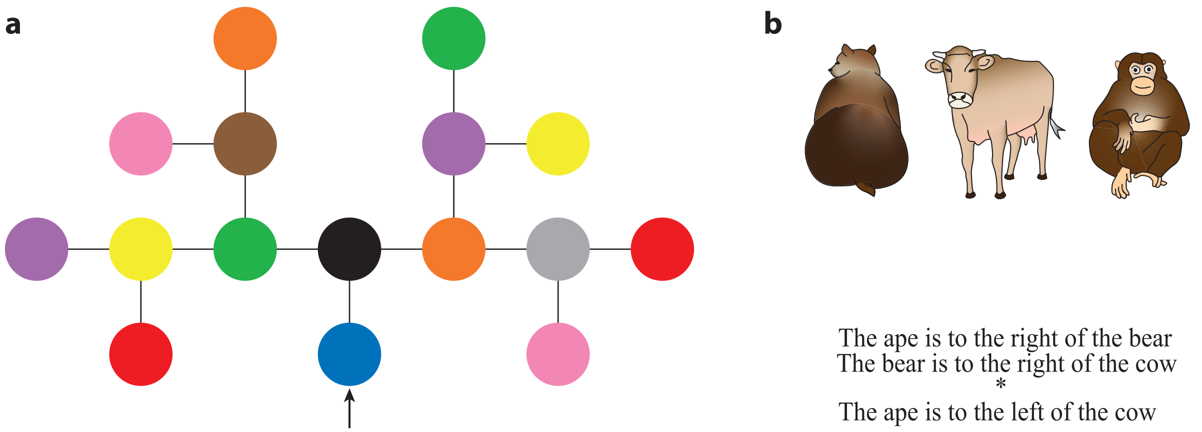

Figure 3

(*a*) Example of a network used in linearization experiments. Subjects were asked to describe the network of colored discs, starting at the arrow. (*b*) The directional term *to the right of* is not transitive when interpreted as *x is to the right of y with respect to y's intrinsic perspective*. Panel *a* adapted with permission from Levelt (1989). Panel *b* adapted with permission from Levelt (1996).

Another "in"-problem for the speaker is perspective taking. The use of directional terms, such as *straight* or *right*, depends on the speaker's perspective. When the speaker makes a "gaze tour" and says, *You go right to*…, the perspective is deictic, *right* meaning right with respect to the speaker's own orientation. But when the speaker makes a "body tour," as it were walking through the network, going right means turning right with respect to the current move. This is the intrinsic perspective. There is a third possibility for spatial perspective, absolutive, as in *uptown* and *downtown*. I spent much effort on the (topo)logical properties of these three perspectives and on their conditions of use, including their roles in the production of elliptic descriptions (for my final overview, see Levelt 1996). **Figure 3*b***, for instance, demonstrates that when using the intrinsic perspective, as opposed to the deictic or absolutive perspective, directional terms such as *to the right of* are not transitive.

Deictic expressions like *this* or *that* often require an accompanying (pointing) gesture. In a series of experiments in which we traced the pointing gesture during speech, we measured to what extent the speech and gesture modes interact during utterance production. We found that the planning of speech and gesture is interactive but that the gesture is ballistic after its initiation (Levelt et al. 1985).

The same pattern descriptions formed an ideal database for approaching an "out"-problem for the speaker. The descriptions contained some 959 spontaneous self-repairs, such as *right to yellow, uh to white*. This initiated a major project for me on self-monitoring and self-repair in speech, reviewed in Levelt (1983) and further in cooperation with Anne Cutler (Levelt & Cutler 1983). The first phase in self-repairing involves the monitoring of your own speech and interrupting speech upon detecting trouble. The perceptual double loop theory proposes that speakers use their comprehension system (right side in the blueprint; see **Figure 2**) to monitor their overt speech and their inner speech. The second phase, upon interruption, is one of hesitation and editing terms, such as *uh* or *rather*. These terms express the character and the urgency of the trouble. The third phase contains the repair proper. I showed that self-repairs follow a well-formedness rule comparable to the well-formedness of coordinations and of question–answer pairs. The paper triggered a flood of research confirming but also modifying the theory.

Phase II: Writing *Speaking: From Intention to Articulation*

When you receive a "grant for life"—becoming a Max Planck director—you had better first think hard about what to do with this very long-term research endowment. During the nineteenth century, research in adult language use had almost exclusively concerned language and speech production, culminating in Wundt's *Die Sprache* (1900). This tradition had largely continued during the twentieth century on both sides of the Atlantic, with Bühler's (1934) work in Europe and the behaviorists' treatises in the United States, especially Kantor's (1936) work and Skinner's (1957) *Verbal Behavior*. This drastically changed with the emergence of information and communication theory and then with the "cognitive revolution." Adult psycholinguistics became the study of language/speech comprehension and memory. That had already drawn me toward topics in language production as a safe research niche during our trial period. I kept that production perspective for my research in the Institute but added a major long-time ambition: to develop a comprehensive theory of the speaker as information processor.

Speaking is our most complex cognitive motor skill. We normally speak at a rate of some two words per second, at that speed retrieving words from a huge, say 50,000-word, mental lexicon. We compose those words' syllables at rates of three to five per second and articulate them at rates of 10 or more speech sounds per second. These utterances have syntactic structure and are usually meaningful and appropriate. The error rate is surprisingly low. Such high-speed performance must be based on full automaticity of the underlying processes. When we speak, we focus our limited attention on content; everything else works by itself, or largely so. The underlying processing components must be relatively autonomous or modular, specialized to do their thing fast and reliably without losing costly time by interacting with one another. The subcomponents must be able to work in parallel in an incremental way, each one working on different bits and pieces of the utterance under construction, being triggered by any fragment of its characteristic input. And all of these processes must run with a minimal amount of looking ahead.

No such real-time processing theory existed when I began writing *Speaking* in 1984. By then I had discovered that the literature on speaking was substantial, though highly scattered over a range of disciplines that normally ignored one another, such as phonetics, anthropology, sociology, linguistics, neuropsychology, artificial intelligence, and, yes, psycholinguistics. Laying out the "blueprint" was a first requirement but not exceedingly hard. Similar, well-argued proposals by Bock, Butterworth [see especially his two-volume anthology on language production (Butterworth 1980, 1983)], Cooper, Dell, Garrett, and Kempen & Hoenkamp had been around in the literature for about a decade. The real work was (*a*) to come up with explicit proposals for the characteristic (triggering) input representations and the output representations for the various processing (sub)components, (*b*) to formulate real-time algorithms mapping these input representations onto output representations, and (*c*) to evaluate these proposals with available empirical evidence.

An essential requirement was that one component's output representation (in fact, any incrementally produced smallest fragment thereof) could function as a characteristic input representation of the next processing component. This dictated my itinerary. I began at the front end, the conception of communicative intentions; made proposals about the structure and the generation of "preverbal" messages; and made these messages the characteristic input to the grammatical encoding algorithm, which delivers surface structures as output. I then used these surface structures as characteristic input to the phonological encoding component, which delivers, by a set of explicit algorithms, phonetic and prosodic plans to the articulatory processing component. Finally, I proposed two feedback loops: The speaker's comprehension system would receive the internally generated phonetic plan and the self-produced overt speech and use these to self-monitor the ongoing speech production.

For each of these in/output representations and for the algorithms, I made reasoned choices, guided by whatever empirical evidence I could find (or obtained myself). Let me give just one example, concerning the core component of grammatical encoding. Its input is a preverbal message, an emerging structure consisting of lexical concepts—that is, concepts for which there are words in the target language. Here I opted for Jackendoff's (1983) conceptual structure format. For the output of grammatical encoding, a "surface structure," Kaplan & Bresnan's (1982) Lexical Functional Grammar offered a natural choice because I was going to argue that grammatical encoding is lexically driven. Here I gratefully adopted the sophisticated algorithm that my Nijmegen University colleagues Gerard Kempen and Ed Hoenkamp had developed. They had published the very first lexically driven algorithm of incremental grammatical encoding, the Incremental Procedural Grammar (IPG) (Kempen & Hoenkamp 1982, 1987). The algorithm was two-stage. During the first stage, the input lexical concepts, fragments of the preverbal message, activated their lemmas in the mental lexicon. Kempen & Huijbers (1983) had introduced the technical term "lemma" to denote the semantic and syntactic properties of a lexical item. Each lemma is, among other things, specified for its grammatical category and for the grammatical functions it governs. During the second stage these lemmas hierarchically combine incrementally, mutually fulfilling their syntactic requirements.

At the time of writing, speech errors were a major empirical source for theories of grammatical encoding. Impressive databases of spontaneous slips of the tongue had been built (by, among others, Berg, Cutler, Cohen & Nooteboom, Dell, Fromkin, Garrett & Shattuck-Hufnagel, MacKay, and Stemberger) or rediscovered (Meringer & Mayer 1895, republished and introduced by Cutler & Fay in 1978). Also, creative experimental procedures had been developed for eliciting speech errors. Important discoveries had emerged from highly sophisticated speech error analyses. I went all-out in *Speaking* to accommodate these findings in my theoretical proposals, not only where grammatical encoding was concerned but also and especially in my treatment of phonological encoding. There were other empirical sources to be integrated in a process theory of grammatical encoding, especially the syntactic priming data obtained by Bock (1986), by way of a paradigm that would become an industry in speaking research.

Speaking became my bestseller. I feel much honored by the proceedings of the biannual International Workshop on Language Production (Meyer & Roelofs 2019), in which *Speaking* became a special theme 30 years after its publication.

Phase III: Toward a Theory of Lexical Access

The chronometric approach. By the time I completed *Speaking*, it had become clear to me that our research team's next research focus should be on the mechanisms of lexical access. How does the speaker, in real time, get from an activated lexical concept, such as SHEEP, to initiate the articulation, that is, |ʃiːp|? Initiating the naming of a presented picture of a sheep takes some 700–800 ms. What exactly happens during that short time window? If grammatical and phonological encoding are lexically driven, then this is the core process to be explained. The speech error–based literature, reviewed in *Speaking*, had provided important insights into these processes, but very little was known about their real-time chronology. A chronometric approach made it possible to focus directly on the normal, undisturbed process. Around 1990, we developed and modified two chronometric paradigms that would serve us over the next decade in hundreds of experiments. We called them explicit and implicit priming. For a description of explicit priming, see the sidebar titled Explicit Priming and **Figure 4**. For a description of implicit priming, see the sidebar titled Implicit Priming. [A third lexical decision paradigm, described in Levelt et al. (1991),

EXPLICIT PRIMING

Glaser & Düngelhoff (1984) presented pictures to be named—for instance, one of a sheep. A printed word (e.g., "goat") could appear in the picture that was semantically related to the picture name. That is the explicit prime (also called a distractor). It could appear at different stimulus onset asynchronies (SOAs) slightly before, simultaneous with, or slightly after picture onset. The participant was instructed to ignore the prime word and to just name the picture. As one of the controls, an unrelated prime word was used, such as "chair." It turned out that the related prime ("goat") slowed down the naming latency more than the unrelated prime ("chair"), especially for SOAs around 0 ms. This version of extrinsic priming is called the picture–word interference paradigm.

Herbert Schriefers and Antje Meyer (Schriefers et al. 1990) extended the paradigm in two ways. They used spoken words as primes, and they added phonological primes—for instance, the spoken word *sheet* when the picture is one of a sheep. There was also an unrelated control condition—for instance, the spoken word *chair*. These three types of prime could be presented at different SOAs, in this case −150, 0, or +150 ms with respect to stimulus onset. The critical measures are the experimental latencies minus the control latency. When there is no interference, this difference score should be zero. This setup was chosen to test the two-stage theory of lexical access: semantic activation followed by phonological activation (see **Figure 4**). The first results supported that theory. At SOA = −150, there is only an effect of the semantic prime (s), interference. During the interval 0–150 ms, there is only an effect of the phonological prime (p), facilitation. These are signatures of the successive stages of semantic selection and phonological encoding.

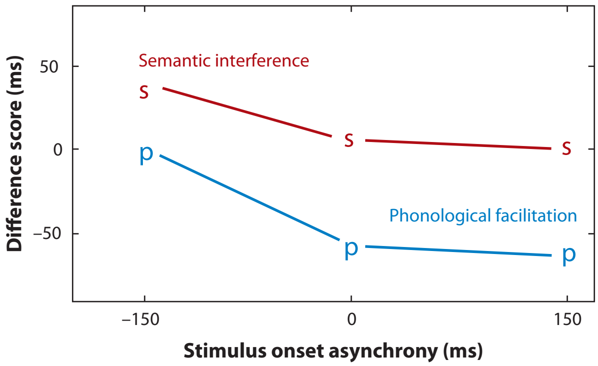

Figure 4

Semantic (s) interference and phonological (p) facilitation at different stimulus onset asynchronies. Data from Schriefers et al. (1990).

IMPLICIT PRIMING

Antje Meyer (1990, 1991) introduced the implicit priming method in her foundational study of phonological encoding. In Meyer's experiments, participants first learned small sets of word pairs, such as the three sets in the columns below:

single–loner	*signal–beacon*	*captain–major*	
place–local	*priest–beadle*	*cards–maker*	[Homogeneous]
fruit–lotus	*glass–beaker*	*tree–maple*	

After learning a set, the participants had to produce, as fast as possible, the second word of a pair (e.g., *loner*) upon the visual presentation of the first word ("single"), the prompt. Thus, the second members of the pairs constitute the response set. The prompts in the set were repeatedly presented in random order, and the subjects' responses and their latencies were recorded. An experiment comprised homogeneous and heterogeneous response sets. In a homogeneous set, such as the three sets above, the response words share part of their form. In the example, the responses share their first syllable: lo*ner*, lo*cal*, lo*tus*; bea*con*, bea*dle*, bea*ker*; ma*jor*, ma*ker*, ma*ple*.

Heterogeneous sets in the experiments were created by regrouping the pairs from the homogeneous sets. For instance, regrouping the above homogeneous first syllable sets can create these three new response sets:

single–loner	*place–local*	*fruit–lotus*	
signal–beacon	*priest–beadle*	*glass–beaker*	[Heterogeneous]
captain–major	*cards–maker*	*tree–maple*	

Here the response sets did not share part of their form.

The responses were faster for the homogeneous sets than for the corresponding heterogeneous ones. Sharing the first syllable facilitated the response. However, no such facilitation occurred when the homogeneous sets shared the second syllable—for instance, with response words *mur*der, *pon*der, and *boul*der. More generally, facilitation occurred only when the response words overlapped from their beginning. Furthermore, facilitation increased with the number of shared segments. This was a major discovery, verified time and again: Phonological encoding is a strictly incremental process. The preparation of unit $n + 1$ strictly follows the preparation of unit n. This principle holds not only for a word's phonemes but also for a word's morphemes.

SOME MAJOR DEVELOPMENTS AT THE MAX PLANCK INSTITUTE SINCE ITS PIONEER PERIOD

1984–1987: William Marslen-Wilson serves as director of the Language Comprehension Department
1985: Manfred Bierwisch appointed external scientific member of the Institute
1986: The new Institute building opens at Nijmegen University campus
1990–1995: Uli Frauenfelder directs Junior Research Group in language comprehension
1991–2017: Stephen Levinson head, then (1998) director of the new Cognitive Anthropology Department
1992–2002: Neurocognition of language project, directed by Colin Brown and Peter Hagoort
1993–2013: Anne Cutler serves as director of the Language Comprehension Department
1997: Major extension of the Institute building
1999: Establishment of the Donders Center for Cognitive Neuroimaging, in which the Institute is a partner, directed by Peter Hagoort
2001: Pieter Muysken appointed external scientific member
2006: Peter Hagoort appointed director of the Neurobiology of Language Department
2009: Max Planck Society authorizes addition of a fifth department, which addresses the genetics of language
2010: Simon Fisher appointed director of the new Language and Genetics Department
2010: Antje Meyer appointed director of the Psychology of Language Department
2014: New wings added to the Institute building that contain the new genetics laboratories and the child research laboratories
2016: Caroline Rowland appointed director of the Language Development Department

Over the years, the Institute was advised and much supported by its Scientific Advisory Board. After its first chair, Jerome Bruner, the Scientific Council chairs were, successively, Eve Clark, Dan Slobin, Herbert Clark, and Ron Mangun.

didn't catch on.] During that decade my team developed our detailed theory of lexical access in steady interaction with the experimentation. And crucially, Ardi Roelofs, all by himself, created the computational model WEAVER++, implementing the theory, again in steady interaction with the ongoing experimentation (Roelofs 1992, 1997). It could make real-time predictions for the outcomes of our experiments. We comprehensively published the theory, its computational implementation, and its body of experimental verification in *Behavioral and Brain Sciences* (W.J.M. Levelt et al. 1999). It became our citation classic.

During the 1990s I was deeply involved with the further development of our Max Planck Institute (see the sidebar titled Some Major Developments at the Max Planck Institute Since Its Pioneer Period).

The theory of lexical access and its computational implementation. Lexical access is a staged process leading from conceptual preparation to initiation of articulation. The flow diagram in **Figure 5*a*** presents the theory of lexical access in outline. Conceptual preparation involves the activation of a lexical concept to be expressed—that is, a concept for which we have a word in our mental lexicon, such as the activity concept ESCORT. In the theory, such concepts are whole; they are not sets of semantic features. However, they do relate to other concepts by way of labeled links, such as *isto* (e.g., ESCORT *isto* ACCOMPANY). Activation spreads over such links, leading to coactivation of other lexical concepts. **Figure 5*b*** represents a fragment of the WEAVER++ network.

During lexical selection, a lemma is selected from the mental lexicon. Adopting the proposal by Roelofs (1992), we had redefined the technical term lemma as the lexical item's syntax (see lemma

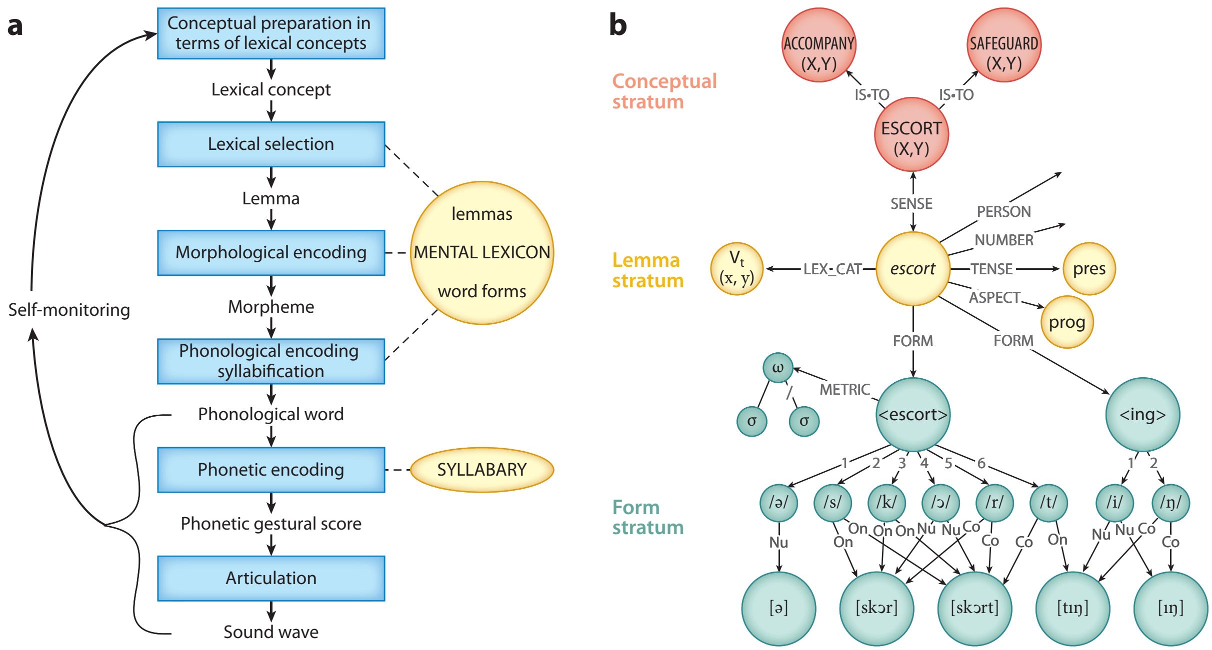

Figure 5

(*a*) Stage theory of lexical access in outline. Preparing a word proceeds through stages of conceptual preparation, lexical selection, morphological and phonological encoding, and phonetic encoding before articulation can be initiated. In parallel there occurs output monitoring involving the speaker's normal speech comprehension mechanism. (*b*) Fragment of the WEAVER++ lexical network. Arrows indicate feedforward spreading activation. Panel *a* adapted with permission from W.J.M. Levelt et al. (1999, p. 3). Panel *b* adapted with permission from W.J.M. Levelt et al. (1999, p. 4).

stratum in **Figure 5*b***). The item's semantics is now specified in the lexical concept. Each lexical concept relates to one specific lemma. In normal processing, the selected lemma will be the one linked to the active target concept. The speed of selecting a lemma is a function of its activation level and the sum activation of all active lemmas (in WEAVER++, following the "Luce rule").

After selection of the target lemma, the lexical system switches to a different mode. The task is now to prepare the articulatory gestures for the expression of that one target word in its prosodic context. The switch is not always easy, as appears from the "tip-of-the-tongue" phenomenon, in which the lemma is available but accessing the word form is hampered. This preparation process runs through three stages. During morphological encoding, a word's morphemes are retrieved from the lexicon. They are incrementally ordered, as shown in our implicit priming experiments (Janssen et al. 2004) (see the sidebar titled Implicit Priming).

During phonological encoding, the phonological segments of each morpheme are retrieved and incrementally ordered (see form stratum in **Figure 5*b***). For a compound word, such as *blackboard*, two phonological codes are accessed—for *black* and for *board*—not one code for *blackboard* as a whole. We showed this to be the case. Accessing such phonological codes is frequency dependent. Bien et al. (2005) showed that the production latency for such compounds is dependent on both morpheme frequencies but not on the full compound frequency. During phonological encoding, a fascinating process takes place: syllabification. Items in the mental lexicon (words, morphemes) are not syllabified. Their form codes are just strings of phonemes. As they are spelled out, they are incrementally grouped into syllables following universal and language-specific rules. These syllables easily stagger lexical boundaries, as in *they escort us*: /'e - skɔ: - təs/. This process is known as "resyllabification," but it is just syllabification.

Finally, during phonetic encoding, composed phonological syllables incrementally activate their syllabic gestures in the "mental syllabary," a repository of much-used syllabic gestures (see bottom nodes in **Figure 5*b***). Speakers of English use no more than 500 syllables to execute 85% of their speech. Those are probably acquired early (C.C. Levelt et al. 1999) and stored as articulatory gestures. Cholin et al. (2006) managed to prove experimentally that high-frequency syllables are accessed faster than low-frequency ones.

WEAVER++'s empirical domain is the chronometry of lexical access. However, minor tampering with its parameters leads to occasional speech errors. These model-generated speech errors nicely match published speech error distributions, as shown by Roelofs (2004).

Cortical activation in word production. The theory of lexical access opened a new window on the spatiotemporal course of cortical activation in word production. In 1996 my team flew to Helsinki, on a plane full of Dutch subjects, to run a picture-naming experiment on the Helsinki University of Technology's 122-channel magnetoencephalographic (MEG) apparatus (with thanks to Riitta Salmelin). From our chronometric experiments, we roughly knew the time windows for the successive processing stages. We could now relate them to the measured neuromagnetic activations. The 270- to 400-ms window for morphonological encoding, for instance, corresponded to activation in Wernicke's area (Levelt et al. 1998). In a further MEG experiment (Maess et al. 2002), in which we used implicit semantic priming (see the sidebar titled Implicit Priming, but now with *homogeneous* meaning same-category pictures, e.g., all animals), we obtained, as expected, left-temporal activation during a "semantic" time window (150–225 ms). Indefrey & Levelt (2000, 2004) used the stage model to perform meta-analyses of word production studies in the neuroimaging literature, leading to a surprisingly clear distribution of cortical regions related to the various processing stages in the theory.

The main bulk of my work from 1989 through 1999 took place in the laboratory. That work has been largely reviewed in the *Behavioral and Brain Sciences* article (W.J.M. Levelt et al. 1999), but see also Levelt (2001). Here, I want to acknowledge my lexical access team during that highly productive decade and beyond—see the sidebar titled Members and Participants of the Lexical Access Team, 1989–2006.

Phase IV: Generating Multiword Utterances

Lexical access is the core process in our blueprint's formulator (**Figure 2**). It is at the base of lexically driven grammatical and phonological encoding. The natural next research step, after clarifying these core processes, was to turn to the generation of multiword utterances. We already

MEMBERS AND PARTICIPANTS OF THE LEXICAL ACCESS TEAM, 1989–2006

A great number of scientists contributed to our project on lexical access. The alphabetical listing hides their different roles, contributions, and backgrounds. They were senior staff, postdocs, many PhD students, research assistants, research visitors, and colleagues from close and far.

These scientists were Harald Baayen, Monika Baumann, Heidrun Bien, Kay Bock, Geert Booij, Colin Brown, Joanna Cholin, Marcus Damian, Jan de Ruiter, Gary Dell, Ger Desserjer, Angela Friederici, Wilhelm Glaser, Peter Hagoort, Alette Haveman, Bert Hoeks, Peter Indefrey, David Irwin, Dirk Janssen, Jörg Jescheniak, Marjolein Korvorst, Koen Kuiper, Aditi Lahiri, Claartje Levelt, Burkhard Maess, Marjolein Meeuwissen, Paul Meijer, Alissa Melinger, Antje Meyer, Rebecca Özdemir, Thomas Pechmann, Peter Praamstra, Rasha Abdel Rahman, Ardi Roelofs, Niels Schiller, Bernadette Schmitt, Herbert Schriefers, Astrid Sleiderink, Simone Sprenger, Jos van Berkum, Femke van der Meulen, Miranda van Turennout, Gabriella Vigliocco, Dirk Vorberg, and Linda Wheeldon.

had started doing so during Phase III and kept going during Phase IV all the way until I became emeritus in 2006. A major experimental problem here is how to trigger someone to "spontaneously" produce the multiword utterance that is the experimental target. Reading the phrase or sentence is not an option because it skips some of the crucial production procedures. We went for three model domains, which were rich enough to be relevant yet restricted enough to allow for such experimental control.

Multiobject scene descriptions and the visual world paradigm. We had already begun using multiple object scene descriptions during the Project Group period to trigger noun phrase and verb phrase coordination (Levelt & Maassen 1981). Bernadette Schmitt and colleagues (1999) used the method to study the generation of anaphoric pronouns, showing that the original noun is reactivated when the pronoun is used. The major development was Antje Meyer's introduction of the "visual world" eye-tracking paradigm (Tanenhaus et al. 1995) in our laboratory in 1997. Meyer et al. (1998) were the first to apply this method to the study of language production. In that first study, the participants viewed a scene containing two objects—for instance, a boat on the left and a flute on the right. They would then say *a boat and a flute* while their eye gaze was tracked. The participants almost always looked first at the left object and then at the right one. How long did they fixate on the first object? Very long—more than 400 ms. That is much longer than needed for recognizing the object. A major discovery was that the fixation duration depended on the frequency of the object name (such as *boat*). Together with later experiments, this showed that a speaker keeps fixating on the object until the word form has been retrieved. This seeming breach of incrementality (it should have been enough to just recognize the left object before moving the gaze to the right one) triggered further work on the management of attention during speech production. This paradigm has meanwhile become prevalent in language production research generally, not only in our lab. Quite complex syntactic planning can now be followed in real time (see, e.g., Konopka & Meyer 2014).

Idiomatic expressions. A substantial part of our mental lexicon consists of fixed expressions (Jackendoff 1995). A comprehensive theory of lexical access should account for their generation as well. We extended our theory to include the production of idioms, fixed expressions that are not (fully) compositional. Cutting & Bock (1997) were the first to propose such a production theory, with important experimental support. Our theory (Sprenger et al. 2006) is a modification thereof. Like Cutting & Bock, we propose that idioms, such as *kick the bucket*, have their own lexical concepts. They are activated just like other lexical concepts. In our theory they also have their own lemma. We called it a "superlemma" because an idiom's lemma will spread its activation to the "normal" lemmas involved (i.e., for *kick* and for *bucket*). The superlemma represents the (mostly) limited syntax of the idiom. You cannot say, *Bill and Mary kicked their buckets*, or *The bucket was kicked by Bill*, without losing the idiomatic meaning. It was a challenge to have participants produce specific idioms in chronometric experiments, but we managed and could show, as the theory predicts, that the normal lemmas plus their lexical concepts are indeed coactivated when an idiom is prepared for production.

Clock times and number names. Clock times and number names form rich, generative domains. Kay Bock initiated the study of clock time naming as a model domain. Soon after, she came to spend her sabbatical year in our Institute, which allowed us to make this a joint cross-linguistic (English–Dutch) project (Bock et al. 2003) that also involved the naming of numbers. In both languages, two systems of clock time naming are used. When your digital clock displays 12:45, you can read it either as *twelve forty-five* or as *a quarter to one*. These are, respectively, absolute and relative time expressions. Americans have a preference for absolute expressions; the Dutch prefer relative expressions. Grammars for such expressions were nowhere to be found in the linguistic literature, so we wrote them ourselves. Relative expressions (and hence their grammars) differ substantially between languages. Two issues became central in our research. The first one was "thinking-for-speaking," a notion Slobin (1996) introduced to denote the speaker's retrieval of conceptual features that require obligatory marking during grammatical encoding. When you describe 12:45 as *a quarter to one*, you mention none of its numbers. In fact, you perform a rather time-consuming computation (Meeuwissen et al. 2003, Korvorst et al. 2006) to conceptualize the displayed time in order to map it onto a preverbal message that can be grammatically encoded as a relative expression. These studies and their detailed follow-up by Sprenger & van Rijn (2013) are the first in the literature in which thinking-for-speaking was precisely quantified and measured.

The second issue was "seeing-for-saying" (which facilitates thinking-for-speaking). Where does the speaker look when reading a digital or analog clock time? The main outcome of our experiments was that you scan the display in the order of mention. Reading 12:45 as *twelve forty-five*, your gaze jumps from 12 to 45. The Dutch gaze is similar when they say *twaalf-uur-vijf-en-veertig*. But if you say *a quarter to one* (or in Dutch *kwart-voor-een*), you focus first on 45 and then on 12. The process is similar for the reading of analog clock times. Saying *twelve forty-five*, you fixate first on the small hand and then on the large one. The fixation order is the reverse when you are going to say *a quarter to twelve*. The important conclusion is that the target syntax of your time expression guides your scanning pattern, seeing-for-saying.

This completed the fourth and last phase of my evolving research program. For me it also completed almost half a century of laboratory work.

THE ROYAL NETHERLANDS ACADEMY OF ARTS AND SCIENCES

I became president of the Royal Netherlands Academy of Arts and Sciences in 2002 for a 3-year term. It was an almost full-time job, hence somewhat interfering with my laboratory work. The Academy is sort of a small Max Planck Society; it was at the time running some 19 research institutes in the humanities and medical-biological sciences. It also advises the Dutch government on policy-for-science and science-for-policy matters. One major feat of our Board was the establishment of The Young Academy, a wonderful step up for brilliant young scientists. I also took special pleasure in contributing to the international network of (almost 100) academies, the Inter Academy Panel. When I began as president, it had just established its own research council, the Inter Academy Council, of which Bruce Alberts, at the time National Academy of Sciences president, was cochair. The Council's headquarters resided in our Academy in Amsterdam.

One of my initiatives was to launch a project called Women for Science. This had been suggested to me by my American sister Anneke Levelt-Sengers, a physicist at the National Institute of Standards and Technology and, like me, a member of the National Academy of Sciences. She had convinced me of the tragic gender imbalance, almost all over the world, in academia. It needed little effort on my side to convince Bruce Alberts. He skillfully initiated this research project, appointing my sister as cochair of the research committee. The committee's report (Levelt-Sengers & Sharma 2006) triggered worldwide implementation programs, especially in the InterAmerican Network of Academies of Sciences, the network of American academies.

A HISTORY OF PSYCHOLINGUISTICS: THE PRE-CHOMSKYAN ERA

I became Max Planck Emeritus in 2006, which is a recommendable status to acquire. My Institute generously has provided me with an office as well as administrative, technical, and library facilities till the present day. I decided to take on a major new project that had been long on my mind, namely, to write a history of psycholinguistics. No such comprehensive work existed. Rather, textbooks in the field usually suggested that psycholinguistics had emerged after World War II, especially during the "cognitive revolution." Nothing, however, is less true. Empirical psycholinguistics has its origins in the late 1700s. It became an established science during its first golden age, 1865–1915 (cf. Levelt 2016), with Wilhelm Wundt's (1900) two-volume *Die Sprache* as its foundational work.

It took me six wonderful years to write this book (Levelt 2013) on "psycholinguistics BC"—that is, psycholinguistics before Chomsky. I did not extend it beyond about 1960 because I had become a player in the field myself—I did not want the book to become (partly) autobiographical.

One general experience in writing this book was my noticing, time and again, that brilliant theories, discoveries, tools, and ideas had fallen into oblivion and then were reinvented in modern psycholinguistics, usually without any reference to the original. I called them "sleeping beauties" (Levelt 2015). Here are just two examples: Who invented the phrase marker? Wilhelm Wundt in 1880. He used it extensively in *Die Sprache* (1900). I have not seen *any* use of phrase markers in the (psycho)linguistic literature after Wundt until 1949. Only then did Nida (1949) reintroduce them in the second edition of his morphology textbook, though without any reference to Wundt. And mind you: Leonard Bloomfield, the grand master of immediate constituent analysis, had been a devoted postdoc with Wundt.

Here is another example: Who was the brilliant creator of speech error analysis, establishing the first solid method of collecting such errors; creating the first reliable corpus; and providing the first, still valid, methods of analyzing them? Rudolf Meringer did in his 1895 book with Carl Mayer (Meringer & Mayer 1895). This approach to speech error analysis remained fully ignored in the literature until it was resuscitated during the 1960s, finally becoming a booming field of research. Here we do know the cause: Sigmund Freud, the Donald Trump of psycholinguistics. His first edition of *Zur Psychopathologie des Alltagslebens* (Freud 1901) makes reference to Meringer's work, bluntly rejecting Meringer's now time-honored analyses and proposing a hilarious set of "alternative facts"—namely, deep psychoanalytic causes of speech errors. Every new edition contained more of them, not self-observed but provided to Freud by his admirers. After the sixth German-language edition of Freud's monograph, Meringer (1923) exploded and published a devastating review, deconstructing and ridiculing Freud's analyses case by case, but to no avail. The review was in German; Freud's eleventh English edition appeared the same year. The monograph had conquered the world. It had become the ultimate "scientific" source on speech errors. Meringer was forgotten till Cutler & Fay's 1978 edition (Meringer & Mayer 1978) saved his foundational work from oblivion. Studying the history of your field can be a humbling experience.

WHAT'S NEXT?

Gratitude. I have been a Sunday's child, surrounded by golden people both in my family and in my science. The Max Planck Society's establishment of an Institute for Psycholinguistics has deeply affected the field and created research opportunities and a research future for hundreds of young scientists from all over the world. I myself have had the privilege of (co)supervising 58 PhD students. The Institute is booming and so, no doubt, will psycholinguistics be for a long time to come.

DISCLOSURE STATEMENT

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