Déjà vu?

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A decade of *Cognition* has made it a very established journal. The editor and associate editors presumably have mixed feelings about this: they will not be insensitive to success, I guess, but surely they have a problem with establishment. The original motivation for the journal was, in part, to get away from the masquerade of the standard paper format, which tends to inhibit the expression of the larger theoretical framework or the scientific philosophy of the author. For another part, and not unrelated to this, the journal was intended to serve as a forum to discuss the utility of cognitive science for society and its (ab)uses in changing society. In the first editorial one can read: 'Thus it is our duty to discuss not only the practical value of our scientific conceptions in light of the problems faced by people and societies but also to evaluate possible applications in the light of what we know about ourselves'. Rereading this editorial I couldn't help being reminded of a similar statement: 'This peace we will not find before we have changed ourselves; and in order to change ourselves we will first have to know ourselves. This knowledge psychology tries to provide us, by patient work; and we will have to wait also patiently for the time that this knowledge can be utilized in practice'. This was written in the year 1909 by G. Heymans, the well-known Dutch psychologist, in a paper called 'The coming century of psychology'. Admittedly, *Cognition's* editorials show less patience, but this should be understandable given that three quarters of Heymans' century have passed by without noticeable results for either peace or society.

The same fact, however, makes one wary to express opinions about the utility of one's own work for the coming decade (as I was asked to do). It is not going to be more than that of our scientific predecessors for present society: very limited, if any. Still, the historical perspective is less a source of pessimism for me than one of inspiration. Whatever the research I will do in the coming decade, it will stay centered around some fundamental notions and issues which are classical in psychology. My scientific roots are in a continental tradition which is dominantly mentalistic and nativistic. The intellectual climate at Leyden University during my education was eclectic, but with clear overtones of phenomenology, Gestalt psychology and ethology. I

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vividly remember my surprise at listening to Chomsky's attacks on psychology during my year in Cambridge, Mass. 1965-1966. Psychology apparently was Skinnerian psychology, as it still was in Chomsky's article 'Psychology and ideology' which opens the first issue of *Cognition*. Honestly, I didn't even know the difference between classical and instrumental conditioning at the time, in spite of my Ph. D. in psychology.

In subsequent years, I have come to realize that a major influence on my thinking stemmed from A. Michotte, the Belgian psychologist, who in 1959 had given me a semester's hospitality in his laboratory at Louvain university. During this stay he had worked intensively with me, in spite of the 57 years difference in age. Michotte had been trained by Wundt, and especially by Külpe, the founder of the so-called 'Würzburg school'. This school had departed from Wundt by studying 'higher' mental processes by means of experimentation. The approach remained characteristic of all of Michotte's research. (Below we will turn to another member of this school, Karl Bühler, who was the first to apply the school's 'method of systematic introspection' to psycholinguistic issues). Michotte's epistemology was neo-Kantian. He believed that the major categories of cognition (substance, reality, causality) were innate, and the dominant direction of his work was to show that in origin these are innate *perceptual* categories. The immediate and compulsive impression of causality, for instance, arises under precisely definable and quite restricted perceptual conditions. By inventive experimentation, Michotte could construct so-called 'negative' cases' where these perceptual conditions were not fulfilled, but where experience would suggest causality; still, no impression of causality resulted. And even more convincingly, he set up 'paradoxical cases' where the perceptual conditions for causality were fulfilled, but in such a way as to contradict experience (so, for instance, if an object *diminishes* its speed at being hit by another object moving in the same direction); here he found his subjects spontaneously and systematically reporting an impression of causality. As Michotte (1963, p. 220–221) put it, these cases provide a clear demonstration of the uselessness of any psychological theory which suggests that it is past experience which plays the crucial part in setting up causal links'. Not surprisingly, Michotte had deep disagreements with Piaget, though they used to address one another as 'le Maître de Genève and 'le Maître de Louvain', and to understate their disagreements in highly polished language. When I read 'The debate between Jean Piaget and Noam Chomsky' (1980), so beautifully edited by Piatelli-Palmarini, I had a strong experience of déjà vu. Exactly the same arguments had been going on between Piaget and Michotte more than twenty years earlier. When Chomsky writes 'The natural way to proceed, if we are trying to determine the nature of S_0 [the genetically determined initial state—W. L.], is to try to find some property of the steady

state that is minimally affected by experience, a property for which E (experience) is reduced as close to zero as possible', he expresses exactly the logic of Michotte's experimentation with negative and paradoxical cases.

The notion that important aspects of our behavior and experience are based on pre-given structure, over which we have little control, has been and still is a Leitmotiv in my research. Additionally, there is the conviction that these basic structures have a modular organization with a maximum of organization within a module, and a minimum of interaction between modules. This conviction I surely owe to my thesis supervisor John van de Geer who expressed it as the notion of 'relative autonomy' of subsystems. As a consequence, I dislike heterarchical theories in which all modules can talk to all other modules; I prefer hierarchical organization in cognitive theory.

Some examples can illustrate these theoretical starting points. In my work on binocular vision (Levelt, 1968), I have tried to determine the eyes' interaction in brightness and rivalry. The findings leave no doubt that higher processes such as Gestalt formation or attention do not interfere to any substantial degree with the system's activity: the cyclopean system is relatively autonomous, and the interaction between the eyes is fixed and rather simple, in full agreement with Hering's (1862) nativist views on the visual system, and in contrast to Helmholtz's empiricist position. In the same vein, we have shown that loudness interaction between the two ears is completely additive, i.e., that there is no interaction term at all: the two ears deliver their output without being affected by one another (Levelt, Riemersma and Bunt, 1972). Another example can be found in work on the perception of musical consonance I was involved in (Plomp and Levelt, 1965; Levelt, van de Geer and Plomp, 1966). There is no doubt that culture and experience are major determinants of consonance perception. Still, we could show that, whatever culture has built, it is rooted in the given psychophysical structure of the ear, more specifically in the so-called 'critical band' of pitch/loudness interaction. The situation is very similar to that of the perceptual origin of causality mentioned above: In both cases there is an immediate impression over which we have no control, but which may develop into an abstract cognitive category.

The issue of relative autonomy is especially intriguing where perception and language interact. My interest was raised by George Miller's work on verbs of motion which I came to know during a year at the Institute in Princeton. The semantic components Miller suggested, such as 'causative', 'permissive', 'instrumental reminded me of Michotte's perceptual impressions of causation, triggering (*déclenchement*), and instrument (*effet outil*). Would there be anything like component-by-component matching between perception and language in verification and naming of visual motions, and how do the systems interact? The main finding of the subsequent research (Levelt, Schreuder and

Hoenkamp, 1978, Schreuder, 1978) was that in verification no semantic decomposition takes place (for 'true' cases), and that the process is driven by the perceptual system without much feedback from the linguistic system: the most salient perceptual feature 'looks for' the most salient meaning component of the verb, and not inversely.

Processing relations between perception and language has also become a major theme in our newly founded Max-Planck-Institute for Psycholinguistics. More specifically we study how speakers operate on spatial representations in producing descriptions of spatial arrangements (such as living rooms, routes, or more abstract networks). A central concern here is what I called the speaker's linearization problem (cf. Levelt, 1981a, b): a speaker will normally have to 'linearize' a spatial or other knowledge structure for expression. This requirement to determine an order of mention, and how to deal with it has been studied by rhetoricians for millenia, but the issue is by and large ignored in modern psycholinguistics. We found that there is a coherent set of linearization principles. They are, in part, determined by what is mutual knowledge in the speech situation, and in part by the speakers's working memory requirements. Neither of these are linguistic in nature, and we have growing evidence that linearization decisions are unaffected by formulation processes such as lexicalization and syntactic choice (Levelt and Maassen, 1981). Linearization, therefore, may have to be considered as a relatively autonomous procedure in the speaker's formulation process. The same principles of linearization should hold whatever the modality of language (e.g., signed *versus* spoken, cf. Levelt, 1980).

Given a chosen linearization, however, the speaker provides the listener very systematically with linguistic cues which facilitate the reconstruction of the intended spatial or other complex representation They comprise, among others, anaphoric devices, modal expressions and deictical devices of various sorts. The Institute is deeply involved with the study of how such devices are used in both perception and production, and how they develop in first and second language learners. Some of the work is reported in Jarvella and Klein (1981).

Mentioning our research on deixis gives me the occasion to return to Karl Bühler, who spent the last 23 years of his life in total oblivion in America. The Jarvella and Klien book opens with their translation of Bühler's highly original analysis of deixis in part II of his *Sprachtheorie* (1934). Something else which seems to have been completely forgotten about Bühler is the fact that he moved psycholinguistics into the laboratory, something George Miller had to accomplish again half a century later. In 1908(a) Bühler published a study in which he measured comprehension latencies for complex sentences, and where subjects had to give introspections on their process of understan-

ding these sentences. Even Ericsson and Simon (1980) show no awareness that this was the first major study with verbal reports as experimental data. The systematic analysis of linguistic introspections, which is thus nothing new, has fascinated me, for both methodological and theoretical reasons ever since my first contacts with generative linguistics.

As far as method is concerned the sharp dispute between Bühler (1980b) and Wundt (1907) had made it clear that, whatever one's theoretical position, one should take intuitive reports as data just as any other behavioral data. Michotte (1954) clearly took this position to consider 'les réponses verbales comme des réactifs differentiels', as do Ericsson and Simon. What I have tried to add to this notion is that the interpretation of such introspective data requires (a) a theory of the subject matter under study; in case of judgments of syntactic cohesion or grammaticality this can, for instance, be a generative grammar of the language, and (b) a measurement or interpretation theory, i.e., a theory about how the (intuitive) data relate to the entities in the theory on the subject matter. In Levelt, 1974, Vol. III, I developed formal interpretation theories for judgments of syntactic cohesion, and used them to study different generative accounts of English and Dutch. The coming decade may produce some more work along these lines.

As far as a theory of linguistic intuitions is concerned the question is: where do they come from? It is often argued that grammaticality judgements require semantic interpretation of the test sentence. A reaction time study (Levelt *et al.,* 1976) shows that this is not so: syntactic judgment can be a relatively autonomous process. We have, furthermore, started research on the causes and functions of linguistic awareness in children (Levelt, Sinclair and Jarvella, 1978). Nothing would have to be changed in current theories of language acquisition if children were to show no linguistic awareness at all. Are occurrences of linguistic awareness in the child indeed mere epiphenomena, or is the child's ability to reflect on language one of the innate predispositions which are indispensable for attaining coherent interaction between independently developed functioning procedures (cf. Karmiloff-Smith, 1981)?

My feet are in the Old World's psychology, my hands grope around in the New World's cognitive science. The Max-Planck Society's establishment of an Institute for Psycholinguistics will surely contribute to making this tingling tension productive, not only for myself, but also for large numbers of young scientists who will be shaping our field for the decades to come.

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