

## HOW THE COGNITIVE REVOLUTION PASSED LINGUISTICS BY

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### ***1. Some history***

In order to understand what was involved in the cognitive revolution that took place around 1960 we must go back to the origins of psychology as an academic discipline in the late 19<sup>th</sup> century, and in particular to the various concepts of ‘mind’ that were current in that period. Let us first look at a concept of mind typically found in the metaphysics of that period (and other periods as well). In the very popular handbook on metaphysics by Bowne, first published in 1882 but reprinted many times after that, we read:

In spontaneous consciousness the mental subject is given as active and abiding; and the race has constructed various names for it, as mind, soul, spirit, and their equivalents, to indicate its reality. The structure of all thought and language concerning the inner life also implies it. This general conviction of the race we believe to be correct. Nevertheless it is disputed on various grounds; and the soul is declared by many to be only a name for a group of states of consciousness, more or less complex, which are produced in some way or other, but which inhere in no substantial or active subject. [...] The question concerning the reality of the soul is commonly called the question of materialism or spiritualism. [...] The true question is whether the soul be a proper agent acting out of itself, or whether it is only a name for a set of states of consciousness produced and brought together from without, by physical organization or otherwise. The view which maintains the former position we call spiritualism. (Bowne 1926: 300)

Bowne was professor of philosophy at Boston University. The man whose views he was opposing was William James, professor of psychology at nearby Harvard, who, in his epoch-making *Principles of Psychology* of 1890, defended an incipient materialist concept of mind. Harking back to predecessors like Herbart, Hume and Mill, who had proposed theories of the mind where the mind was seen as a causally determined (though not yet unambiguously physical) mechanism, he proposes, not without a great deal of hesitation, a cautious form of physicalism:

Bodily experiences, therefore, and more particularly brain-experiences, must take a place amongst those conditions of the mental life of which Psychology need take account. *The spiritualist and the associationist must both be 'cerebralists,'* to the extent at least of admitting that certain peculiarities in the way of working of their own favorite principles are explicable only by the fact that the brain laws are a codeterminant of the result. (James 1901: 4, italics original)

We shall see that some seventy years later Harvard will again be the scene of controversy in psychological theory.

But before we come to that we must review the momentous development of BEHAVIORISM in America. The year of its birth is usually given as 1913, when John Broadus Watson, professor of psychology at Johns Hopkins University in Baltimore, published his famous article 'Psychology as a behaviorist views it'. Watson, who specialized in animal psychology, concluded that empathy or introspection as a method for the discovery of psychological processes and structures cannot work for animals, and that the only way of, so to speak, getting into the animal's mind is to apply the method of the physical sciences. This view he then applied to human psychology as well.

First he distinguished the observable DATA, which can only consist in the BEHAVIOR of living organisms. Not all forms of behavior, however, inform us about the mind, but only those that cannot be explained by direct physical causation as studied in physics. Bleeding after a cut, for example, is explained by direct physical causation, but angry shouting after an insult is not. Bleeding and similar physical effects, therefore, need not concern the psychologist, but anger and similar effects will have to: they require the assumption of an intervening mechanism. It is these indirectly caused forms of behavior that are the subject matter of psychology. A SPECIAL CAUSAL HYPOTHESIS is needed for their explanation. The hypothesis adopted by behaviorism consists in the assumption of a relatively simple stimulus transfer mechanism ('conditioning'), much after the fashion of the theory of conditioned reflexes developed since around 1900 by the Russian physiologist Ivan Petrovich Pavlov. It is this CONDITIONING HYPOTHESIS that characterizes behaviorism and was, by and large, abandoned around 1960. The other methodological premises, namely the nature of the data and the causal questions asked, have continued to characterize the whole of cognitive psychology till the present day. It is only in the answers that modern psychology differs from behaviorism.

Much more so than Jamesian psychology, behaviorism was thoroughly materialist. This, of course, fitted in perfectly well with the then powerful philosophical movement of positivism, which expressed a virtually unlimited faith in modern science as the salvation of mankind. People like the 19<sup>th</sup> cen-

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ture French writer and philosopher Hippolyte Taine had already promoted the belief that the humanities should adopt the methods of the natural sciences (see his book *De l'intelligence* of 1870, reprinted many times after)<sup>1</sup>.

The period between 1920 and 1960 is characterized by a brash optimism with regard to the possibilities of the natural sciences, including the new mathematics and the new logic. Scientific ingenuity was seen as the one and only effective remedy for all social, political, economic and psychological problems that were so obviously besetting mankind all over the world. Now, of course, half a century later, we have become a little wiser and a great deal sadder. We realize that the problems are much deeper and more complex than what was thought then, and that present-day scientific understanding, whether in terms of natural or of human sciences, barely probes beyond the surface.

One may well regard behaviorism as one of the most powerful expressions of this general attitude of positivism. It meant a total rejection of the old spiritualist or idealist views of the mind. All so-called 'mentalist' talk was stigmatized as being unscientific. People had no emotions, desires, intentions or thoughts, only conditioned responses, based on stimulus associations that had taken place in their life histories. Behaviorism was, in actual fact, a psychology without the mind.

Nowadays we agree that the behaviorist dogma (because that is what it soon became) of conditioned responses was an extremely superficial theory, even though, from a purely methodological point of view, it was perhaps the right start. One begins with a minimal hypothesis as a possible explanation of the observed data, and enriches the initial simple theory only if forced by the facts. This is nothing but the principle of simplicity of scientific theories, or Ockham's razor. And the theory of conditioned responses was indeed just about the rock bottom minimal theory for the data in question. But the theory became an ideology, a revolt against 'mystic' views of man and mankind, the embodiment of the new scientific approach to all human phenomena. For about forty years many psychologists, especially in America, were mentally unable to give it up, or even relativize it.

Dogmatic behaviorism was widely applied. It was thought that any form of undesirable or 'deviant' behavior could be corrected by reconditioning those individuals who were guilty of it. In 1968 I heard the famous Harvard behaviorist Burrhus F. Skinner boast about his treatment of homosexuals,

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<sup>1</sup> Ferdinand de Saussure was a personal friend of Taine's during his Paris years, and was profoundly influenced by him (see Aarsleff 1982: 356–71).

who were given electrical shocks when viewing a projected picture of an attractive naked man, but sweets when viewing a picture of an attractive naked woman. He did not mention the many suicides that resulted from this kind of ‘therapy’. Many other examples can, of course, be mentioned.

But not all manifestations of the general positivistic attitude were as sinister as the excesses of behaviorism. We also see, especially in Europe, a spate of high-spirited movements that tried to improve the world and mankind by the application of the new mathematical logic introduced by men like Frege and Russell. If only people could be taught to speak and think logically, the world would be a better place. In the Netherlands, for example, we had the progressive idealistic ‘Significa’ movement, founded in the early 20<sup>th</sup> century by the writer Frederik van Eeden (who was directly inspired by Victoria Lady Welby who led a similar movement in Britain) and the great mathematician Luitzen E. J. Brouwer (see Schmitz 1990). This movement affected all young mathematicians and logicians of the period in the Netherlands, and many others as well, and it was not until well into the 1970s that it gradually faded away.

Natural language, too, was suspect in the eyes of the positivists, as it was regarded as ambiguous, vague and logically unreliable. A fairly large number of artificial languages were developed in the hope that they would supplant the existing national and regional languages of the world. These artificial languages, not only Esperanto but also Volapük, Interlingua and many others, mostly developed by linguistic amateurs (some of them crackpots), were thought to be logically sound and semantically unambiguous. If only the world could be persuaded to adopt these languages as the only official means of communication, there would no longer be any wars or other forms of human misunderstanding. I clearly remember, from the days I went to school and to university, schoolmates and fellow students who came from progressive, mostly socialist, families and carried with them the scientific and linguistic ideals of those idealistic but naïve modernist movements.

The linguistic professionals did not, on the whole, go for international artificial languages. Instead, they went for structuralism. Structuralism is the view that the object of enquiry can be studied as if it were some kind of machine, consisting of functional parts that make it work. In Europe, linguistic structuralism started in the late 19<sup>th</sup> century with the Polish linguist Jan Baudouin de Courtenay and, a little later, with Ferdinand de Saussure in Genève. Much has been written about the origins of European structuralism, but what is of direct interest to us in the present context is the fact that it did not, or not explicitly, adhere to a strictly materialist theory of the mind and even less to

behaviorism. As a consequence, introspection or empathy as a method for the testing of analyses was considered legitimate, and so were appeals to semantic phenomena. The Europeans, in short, were much more traditional and much less modernistic as regards questions of the mind than their American colleagues.

In America, linguistic structuralism turned behaviorist and thus materialist. The leading American structuralist linguist was Leonard Bloomfield (1887–1949), who had become a staunch supporter of behaviorism around 1920. Through his teaching and his influence, but above all through his book *Language* of 1933, he dominated American linguistics from 1925 till his death in 1949 and foisted behaviorism on it. This meant that, at least until the cognitive revolution of 1960, semantic phenomena were regarded with distrust and linguistic theories were set up without any appeal to semantic notions. Outsiders did find this curious, and they were right, because a linguistics without meaning can only be an abomination. But that is how it was for quite some time. (Even so, American structuralism has been extremely fruitful and has led to real insights, but in hindsight we regard it rather as a mere passing rite than as a great breakthrough in and of itself.)

## **2. *The Cognitive Revolution***

Then, around 1960, the cognitive revolution happened, ushering in the new era of COGNITIVE SCIENCE, which continues up to the present day (see Gardner 1985 for a well-informed survey). The revolution happened in psychology and amounted to a rejection of behaviorism and the introduction of the computational view of the mind.

Just after World War II a few independent minds were beginning to realize that the hypothesis of conditioned responses was basically insufficient for a rational explanation of the relevant behavioral data. The first to speak about this openly was Karl Lashley, who vented his doubts and arguments in a paper read at the Hixon Symposium of 1948 (Lashley 1951). He emphasized the fact that repetitive behavior cannot be explained behavioristically, as every new instance is necessarily physically distinct from all preceding instances. There must, therefore, be a mechanism of identification, which, however, is not provided by behaviorist theory.

These ideas were soon picked up and further elaborated by a group of ‘angry young psychologists’ at Harvard, working in the close vicinity of B. F. Skinner, the leading behaviorist in the world. The group included George A. Miller, Jerome S. Bruner, Roger Brown, but also, from 1951 till 1955, the young Noam Chomsky, who had been awarded a junior research fellowship

at Harvard and quickly became a prominent member of the group. They found that humans are capable of acquiring skills that far transgress the boundaries of what can possibly be learned through conditioning. The acquisition of competence in a language is a case in point, but other forms of behavior prove the same. For example, the repeated catching of a rebounding ball by a child requires very precise reactions to the trajectory of the ball, which is different each time the ball is caught. As a matter of principle conditioning cannot produce that skill. Or take the behavior of civilians in a town where shooting is heard from rooftops. These people run away to seek shelter, lie down behind whatever can serve as protection, etc. Clearly, such behavior cannot be explained by an appeal to previous experience. Rational thinking seems a better explanatory basis. Thus there grew a climate of strong resistance to the behaviorist dogma.

The alternative theory that the angry young psychologists came up with was a direct result of recent developments in mathematics and computer science, in particular the theory of algorithms, as developed during the 1920s by mathematicians like Emil Post. An algorithm is a computational structure allowing for the formal derivation of output strings of symbols from given input strings of symbols or from a start command, which may be either random or statistically weighted. Among those who were in the know it was clear, by the mid-1950s, that the notion of algorithm was absolutely central to computer science. Computer programs and their machine implementations are instantiations of algorithms.

When the mind is imagined to be a vast computer plant, driven by a large number of algorithmic programs producing all sorts of coded outputs that regulate bodily processes, including the production and comprehension of linguistic symbols, we have a powerful alternative to the abysmally poor and restricted mechanism postulated by behaviorism. Conditioned responses were replaced with computer programs. This not only meant an enormous increase in the power of psychological explanations, it also left room for specific hypotheses about possible innate patterns of computational 'prewiring'. This would help explain the otherwise curious fact that some specific abilities, such as the competent use of language, are normally acquired quickly and according to what appears to be a fixed acquisition program.

As has been said, behaviorism had permeated linguistics through Bloomfield and his followers. But Skinner felt that he could do better. In 1957 he published his book *Verbal Behavior*, meant to be the bible of behaviorist linguistics. Chomsky's review of this book in *Language* of 1959 is generally considered to have been crucial in the sudden downfall of behaviorism,

though it was hardly original, as most of its central ideas had been taken from Lashley (1951), and in spite of the fact that it contained many crucial inadequacies, especially in the way Skinner's ideas were misrepresented<sup>2</sup>.

It so happened that the cognitive revolution, and in particular Chomsky's attack on behaviorism, coincided with another revolution, this time in linguistics, much less important but attracting a great deal more publicity. The linguistic revolution was likewise based on the notion of algorithm. Zellig S. Harris (1909–1992), a close follower of Bloomfield and a keen reader of mathematics, was familiar with algorithmic notions, and at the end of his magnum opus (1951) he actually proposes, though still in rather clumsy terminology, an algorithmic structure for natural language grammars (see Seuren 1998: 213–215). That is, a grammar of a language  $L$  was considered to be an algorithm, driven by a random start command and specifying in a formally exact way what sequences of what constituents will generate precisely the set of proper, grammatical sentences of  $L$ . This, of course, was the origin of what we have come to know as GENERATIVE GRAMMAR. In his famous (1957) and later writings, Noam Chomsky developed this notion further, and managed to take all the credit.

### 3. *The Unsatisfactory Sequel*

One would naturally expect the two revolutions, the cognitive and the linguistic, to merge, since the algorithms postulated by linguists for the grammars of natural languages would naturally be regarded as instances of the kind of mental algorithms envisaged by psychologists. It was inevitable that the question of the psychological reality of grammatical algorithms should be raised. And so it was, but it was never answered satisfactorily. The new school of generative grammar never stopped being half-hearted, ambiguous and noncommittal regarding questions of psychological reality. While the cognitive revolution brought the mind back into psychology, the linguistic revolution saw the mind but left it at the doorstep. Why and how this happened is the question that occupies us in the present paper.

The question is especially interesting because during the 1960s a new development in generative grammar, under the name of GENERATIVE SEMANTICS, made serious and highly promising advances in the direction of cognitive science. Generative Semantics, which had been initiated by some of

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<sup>2</sup> See in particular MacCorquodale (1970). Murray (1994: 232) speaks of a "blatant misrepresentation".

Chomsky's followers, notably John Ross, Jim McCawley, Paul Postal and George Lakoff, rejected the random generator view of grammar and saw the grammar of a language as a formal, algorithmic transducing device turning thoughts into sentences and vice versa. Such a view is obviously more natural, and more cognitivist, than the random generator concept, which hardly stands a chance of being psychologically real. But it involves the notion of (propositional) thought and requires a good knowledge of logic, semantics and a fair amount of philosophy. And that was, apparently, too much for Chomsky and his followers, who mounted a violent campaign against Generative Semantics in the early 1970s and caused its virtual disappearance from the linguistics market place.

The role played by Chomsky, in this respect, deserves special scrutiny. It is remarkable, to say the least, that the man who was so prominently instrumental in demolishing behaviorism and who subsequently became the most influential figure in linguistics shirked from drawing the consequences of his anti-behaviorism and continued to be distrustful of all things mental, as if behaviorism had never ceased to exist. Chomsky never developed a feel for or interest in semantics. His theory of grammar, in the various guises it assumed over the years, has not so far managed to establish an organic link with a proper theory of meaning. Nor has it ever changed its representation of grammar as a random sentence generator. The notion of a grammar as a formal mediating device between thoughts and sentences was steadfastly rejected, although that is the notion one expects if the mental, cognitive character of grammar is taken seriously. Moreover, Chomsky's ideas regarding the question of the psychological plausibility of such a randomly generating grammatical model never reached any degree of clarity or precision but kept vacillating in inconsistent ways (see Botha 1989: 159–164 for a trenchant discussion).

During the 1970s and later, he used his full weight to attack and demolish the burgeoning development of Generative Semantics. One would expect him to support this new development with great enthusiasm, as it was a direct continuation of the cognitive revolution which he himself had helped initiate. But he did the opposite and used all means in his power to crush that development. In doing so he relied on heavy rhetoric and power politics but never on academic argument<sup>3</sup>. One naturally wonders why. Many feel inclined to

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<sup>3</sup> See R. Harris (1993), Huck & Goldsmith (1995) or Seuren (1998: 493–527) for thorough discussions and analyses of this shameful episode in the history of linguistics. Newmeyer (1980) is factually unreliable and shows a strong pro-Chomskyan bias, as shown in McCawley (1980).

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seek the answer in his character and his personal ambitions and idiosyncrasies. The new theory of Generative Semantics had not been initiated by him but by some of his followers. Had he gone along with them, he would have lost the initiative and would have had to play second fiddle to them. Other, even more dubious, motives have been suggested, but we shall refrain from any speculations in that regard.

It is clear anyway that Chomsky's towering position in the field made enough of an impression on large numbers of mediocre young linguists for them to follow the Master uncritically and reject Generative Semantics merely on the strength of his authority. But Chomsky's excessively dominant position still does not explain why so many other, more clear-minded linguists, who could have shown some independence, were so quick at uncritically rejecting a development that was conquering the world even more rapidly than the random generator model had done a decade earlier and whose research program was unusually clear and promising.

When tackling this question it is perhaps best to see it in a wider perspective. Why did the cognitive revolution not sweep the world as behaviorism had done? Cognitive science replaced behaviorism, but its effects remained largely limited to psychology and its applications, especially artificial intelligence and natural language processing. And its influence was also felt in certain sections of the philosophy of mind, where mental modeling and the mind as a human phenomenon were taken more seriously. Outside these areas, however, the situation did not change much, even though it became unfashionable to mention behaviorism overtly. Cognitive science had brought the mind back into psychology, but it was, apparently, not strong enough to remove the widespread feelings of awe and fear with regard to the mysteries of things mental in the world at large.

One factor may have played a role in this respect. Although the computer metaphor turned out to work well for an analysis of many specific areas of mental activity, certain central facts about the human mind remained unexplained in this new approach, and have remained so till the present day. One thinks in particular of human decision making processes, i.e. the action of free will, and of the difference between mental phenomena that are open to awareness or consciousness and those that take place 'underground' as fast and automatic routines and are screened off from introspection. In general, consciousness phenomena have remained unexplained. They seem to require a conscious inner subject of the processes at hand, a 'homunculus' living inside the mind or brain, reading off the interfaces and taking the decisions. But that 'homunculus' must again have a mind with a second order homunculus,

and so forth ad infinitum. Problems of this nature have been discussed in the literature of the philosophy of mind, but they have not been solved in cognitive science. In fact, they are hardly ever discussed there. It is quite possible that this striking inadequacy is one of the causes of the widespread lingering hesitation with regard to a scientific treatment of mental phenomena.

Another factor may have been the lack of support from the new discipline of FORMAL or MODEL-THEORETIC SEMANTICS that came about after 1970. During the '60s the young Californian logician Richard Montague began investigating the possibility of considering natural language, or more precisely English, as some sort of formal logical language. His idea was that English, and probably other languages as well, offered a challenge to the logician in that it would be interesting to see to what extent it could be treated as a well-defined formal language of the sort developed by logicians. It so happened that at that time there was a new trend in logical proof theory, the so-called 'semantic movement', which aimed at facilitating logical proofs with the help of 'models' with respect to which the logical formulae are true or false. Montague applied this logical (but hardly linguistic) notion of 'semantics' to natural language, to find out how far he could carry it through. As has been said, the development that resulted, after 1970, is called 'formal' or 'model-theoretic' semantics.

This form of semantics has no linguistic roots. It is directly derived from modern mathematical logic, which, one should realize, had been explicitly anti-psychological since Bertrand Russell's book *The Principles of Mathematics* of 1903. Since then till the present day, logicians have generally taken it for granted that logic has nothing to do with correct thinking but is, instead, a formal calculus for 'entailments', i.e. necessary consequences of the kind mathematicians work with.

Yet it was this explicitly noncognitive form of model-theoretic semantics that became established as the standard theory of meaning in linguistics, the reason being simply that linguistics had nothing better to offer, after all those semantically barren years of behaviorism. Many linguists, from all quarters, felt profoundly uncomfortable with the new formal semantics. Those who were engaged in studies of lexical meaning were especially unhappy, but they were not part of mainstream linguistics. And those linguists who were or thought they were at center stage, were forced by the circumstances to seek ways of bridging the gap and come to an arrangement. The Chomskyan generativists have always felt uncomfortable in this respect. Others sought refuge in a different framework of grammatical theory, in particular CATEGORIAL GRAMMAR or HEAD-DRIVEN PHRASE STRUCTURE GRAMMAR (HPSG). This

is not the place to evaluate the merits of these and similar theories, which often show great intellectual acumen combined with formal prowess. The point here is, rather, that the semantics they incorporate is of the mathematical, model-theoretic, and thus noncognitive type.

Meanwhile, model-theoretic semantics has started to make attempts at becoming more cognitively realistic. Since the early 1980s formal semanticists have been working on formal systems that aim at taking into account the fact that uttered sentences are, as a rule, interpreted in terms of a given context or discourse. But although these discourse-dependent systems of semantic analysis and representation often show considerable technical proficiency, it is painfully obvious that their designers are neither linguists nor psychologists, nor indeed philosophers.

Linguistics and formal semantics have thus remained strange bedfellows. This is regrettable because a more cognitively oriented theory of meaning might well have enabled linguistics to integrate better with the new cognitive science. But the opposite happened. The combined prestige of the new theories of grammar and the new model-theoretic semantics drove linguistics away from cognitive science.

One might have expected that the 1970s development of Generative Semantics, mentioned above, could or should have stepped in and offered a cognitively serious theory of meaning. But this did not happen, mainly because the group of young generative semanticists simply lacked the necessary training in philosophy and logic even to make a beginning with a proper cognitive theory of meaning. And by the time they, or some of them, reached the stage of expertise and maturity where they could do something about this in a serious and professional way, it was too late, as the semantic gap had in actual fact been filled by model-theoretic semantics.

Some linguists, however, did deplore the lack of cognitivism in linguistics. During the 1980s the Californian linguist Ronald Langacker and some associates started a new movement in theoretical grammar called COGNITIVE GRAMMAR (Langacker 1987, 1991), closely related with CONSTRUCTION GRAMMAR (e.g. Croft 2001). Their aim is to bring language and cognition together again. But in trying to do so they take the a priori position that language is not or hardly 'modular', i.e. there are no specific psychological functions for the acquisition and use of language, which are both taken to be fully explained by general human cognitive functions and communicative strategies. In other words, there is nothing specifically innate or 'prewired' about language. This new school of thought has meanwhile attracted a fair number of followers. Unfortunately, however, it is characterized by an ex-

treme naiveté regarding the logical and philosophical foundations of semantics and even more so regarding questions of cognitive modeling. Their psychology has not proceeded beyond the simplest possible kind of pre-Wundtian folk-psychology. They also appear to be totally unaware of well-known and basically important philosophical questions of truth and meaning, to say nothing of their lack of familiarity with logic and its foundations. This, of course, hardly puts them in a position to achieve the badly needed integration of linguistics with cognitive science, which requires a judicious synthesis and rethinking of results and insights current in the disciplines concerned.

#### ***4. The main cause and a possible solution***

Here we come to what is perhaps the main cause of the limited effect of the cognitive revolution: the magnitude of the enterprise and the lack of expertise of those who should be in a position to bring about a fruitful integration. Not that the current or dominant views in the disciplines concerned are all sound and only need to be tossed together to produce a satisfactory integrated doctrinal salad. On the contrary, most ideas current in these disciplines are probably misguided, incomplete, uninformed and parochial. But it is mandatory that those who feel called upon to bring about the necessary corrections should themselves have intimate knowledge of what goes on in the separate disciplines, so that their corrections are serious and professional.

This has so far not or hardly been the case. The linguists, whether they are of the Chomskyan or any other denomination, are too often badly informed about cognitive science, philosophy and logic. The pragmatists tend to know too little about philosophy, logic and sound methodology. The philosophers of language hardly know any linguistics or psychology, and their knowledge of logic is, on the whole, superficial. Many of them, moreover, are only part-time philosophers of language, as they devote equal amounts of time to social philosophy, ethics, philosophy of law, philosophy of science, etc. The logical semanticists are, on the whole, strangers in linguistics and psychology, and mostly also in philosophy. And among all of those mentioned there are many who feel free to dabble in evolution theory, genetics and the prehistory of mankind.

It is appropriate, in this context, to quote Karl Bühler, who saw this difficulty coming, at least as far as linguists and psychologists were concerned. In 1934 he wrote:

Wenn die immer wieder geforderte gegenseitige wissenschaftliche Handreichung zwischen Psychologie und Linguistik Früchte zeitigen soll, so müssen die Fachmänner auf beiden Seiten den Mut aufbringen, jeder dem anderen ins Konzept zu spre-

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chen. Keiner vermag sich selbst dem Gesetz von der beschränkten menschlichen Fassungskraft zu entziehen. (Bühler 1934: 111)

For a serious and up-to-date reintroduction of the cognitive factor in the human sciences it is necessary, first, that a group of psychologists, linguists, pragmatists, logicians, philosophers, and possibly also geneticists, anthropologists and prehistorians come together and commit themselves to the study of the FOUNDATIONS OF LANGUAGE AND COGNITION. They will have to make themselves familiar with each other's methods, results, ways of thinking, techniques and terminology. But that is merely a necessary condition. What is needed is also uncluttered, original thinking and communal brainstorming regarding the foundations of language and cognition. Only then can the problems be isolated in a fully professional and up-to-date fashion and can a consistent set of new perspectives and theories be developed.

If ever there was a need for interdisciplinary action it is now, in these fields. Some well-meaning linguistics departments start at the bottom end, treating their students to courses not only in the traditional linguistic subdisciplines of phonology, phonetics, morphology, syntax, sociolinguistics, etc. but also in foreign language teaching, semantics and sometimes even in natural language processing (but usually very little cognitive science, logic or philosophy). How useful such curricula are is hard to say. They certainly carry the risk of creating an unintegrated hodge-podge in the heads of the students, who are taught by staff that are themselves insufficiently informed about the other subjects taught to the same students. On the whole, it does not seem realistic to expect that a process of productive integration of the various results, insights and research traditions will get off the ground in this way.

The action should start at the very top, and as a group effort. The requirements are too heavy to be met by single individuals. The action should take the form of a research institute with places for postdocs and PhD students. If the results obtained are valuable, they will automatically trickle down into undergraduate teaching. What is needed is a think tank of original and open-minded specialists who are forced to interact intensively and on a daily basis, and whose publications will carry the necessary weight. I have some experience with institutes of the German Max Planck Gesellschaft. These come close to the ideal just sketched. Yet as institutes they tend to be too large for the purpose at hand, which requires a new initiative, based on a stricter formula of interaction. But who will have the capacity and the will to set such a thing into motion? This is a question I have no answer to.

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