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Problems of linguistic semantics from the standpoint of the philosophy and methodology of language

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The same as many of my colleagues, I was and am accustomed to make rather free use of the nominal strings "logical semantics" and "linguistic semantics", without giving much thought to the question: what lies behind the difference between them? - although we were probably always aware of its importance. When in 1934 Rudolf Carnap introduced the term "logical syntax", he was fully conscious of the fact that this adjective "logical" gave rise to a mode of speech which sharply deviated from the current use made by linguists at that time of the word "syntax", even though he himself did not use the complementary term 'linguistic syntax'. Neither do I know who was the first to use the string "logical semantics" and in which language. And this, of course, is of a certain systematic importance. When in 1954 I published my essay "Logical syntax and semantics" in the American journal *Language*, I used the string "logical semantics" neither in the title nor in the text: although the principal aim of the essay was to draw the attention of my fellow-linguists to the fact that logicians such as Carnap and Tarski had developed methods for dealing with artificial languages without themselves applying them to natural languages, but which the linguists could disregard only to their own loss. Carnap used the adjective "logical" with "syntax" in two different senses:

- (1) more or less synonymous with "formal", i.e., expressed negatively, without any reference to meaning and use, and, expressed positively, with sole reference to the kind and sequence of the signs of which the larger strings are composed,
- (2) to characterize a conception of syntax which would enable us to deal with the logical properties of sentences and with the logical relations between them, something, that for the linguists had until then been absolutely unusual and strange.

The first-mentioned sense, both contextually and historically, is not particularly apposite, and it is not worth while going into it. The second one, too, has since become obsolete, especially through Carnap's own later works. Instead, another pair of concepts has emerged in recent decades. This opposes a "syntactic" to a "semantic" treatment of logic, whether it is a matter of constructed or natural languages, and in which such highly interesting problems as the possibilities and limitations of the replacement of semantic methods of dealing with logic by syntactic ones have been formulated and partly solved, with such pertinent theorems as those of Gödel and Tarski.

In 1933 Carnap still strove for a *syntactization* of *logical semantics*, to put it in slogan terms, with all the dangers connected with such simplifying formulations. Shortly afterwards, however, he realized that this endeavour was superfluous, after he had learned from Tarski that semantics can be treated in a metaphysics- and in particular ontology-free manner. Nowadays logicians are concerned with the problem to what purpose, and how far, semantic logic is replaceable by a syntactic logic, again in slogans: with the purposes and limitations of a *syntactization of semantic logic* rather than of logical semantics.

What, then, is logical semantics? It will surprise nobody when, after brief reflection, he will discover that the nominal string is equivocal. I believe the contrary would surprise us nowadays. In one sense, logical semantics is nothing else than semantics, whether of artificial or natural languages, as it is practised by logicians; linguistic semantics, correspondingly, is semantics as practised by linguists; and philosophical semantics - the third element in this triad and which I do not wish to go into today - is, of course, semantics as practised by philosophers. This is an important, though not very instructive, analysis, for as such it does not say much about the methods of treatment.

When, sitting at my desk, I contemplated in what way the logical treatment of semantics could be distinguished from the linguistic treatment of semantics, as to its contents, I soon was led

to the following five dichotomies: The first is "normative" versus "descriptive"; the 2nd "artificial" vs. "natural"; the 3rd "universal" vs. "particular"; the 4th "pure" vs. "applied"; and the 5th "with particular regard to logical relations" versus "without particular regard to logical relations". The first element of each pair seemed to have something to do with "logical" and the second with "linguistic". Here my imagination gave out.

I am, of course, unable to undertake a serious analysis of these dichotomies within the limited time at my disposal. Such an analysis will be carried out with some thoroughness in a chapter entitled "Logic and theoretical linguistics" in vol. XII of *Current Trends in Linguistics* (J.A. Sebeok, ed.), to be published in 1973. I would like to devote the remainder of my time to a short exploration of the last-mentioned dichotomy, i.e., "with particular regard to logical relations" versus "without particular regard to logical relations". This dichotomy is discussed much less frequently than the other four. On my panel you see Professor Staal who, in 1967, participated in a symposium on "Formal logic and linguistics" which I prepared and introduced within the framework of the 3rd International Congress on Logic, Methodology and Philosophy of Sciences, and who subsequently published an excellent report on this symposium in *Foundations of Language*, 5 (1969), pp. 256-284. I presume that some of those present here have attended that symposium. There it was already clear that many linguists - at first in America, but soon also in Europe - had come to realize that no serious description of natural languages is feasible without somehow dealing with the logical form of the sentences in those languages, after which the logical properties and relations of these sentences can be formulated. The fifth dichotomy was recognized already at that time as a pseudo-dichotomy, though I would not claim that all linguists share this view. Neither do I have the time to go into the interesting historical developments which led to this realization. In place of this pseudo-question there arose another, much more serious, question which from the very beginning transcended the borderlines between logicians and linguists. The question now was whether logical form, which determines the logical properties of and the relations between the sentences, should be attributed to the sentences as such in their surface structure - as was the current term already at that time - or to the deep structures underlying them; perhaps even whether logical form should be identified with deep structure. At the outset, not every linguist was aware of the fact that this same differentiation, though not in exactly the same terminology, had already been made by Russell and Wittgenstein, both philosophical logicians or logical philosophers, half a century earlier. One of the numerous questions I asked those gathered at the Amsterdam symposium was whether this taking into account of the logical properties and relations of the sentences in natural languages should be direct or indirect, meaning by this pair of adjectives precisely the opposition just formulated - though I did not then formulate it in this way and, therefore was not properly understood by all the participants in the discussion - and rightly so. Of course, you will see at once that this dichotomy overlaps several of the previously mentioned dichotomies so that, for instance, one can be of the opinion that the deep structure of sentences in natural languages should best be represented in a suitable artificial language, that this artificial language, then, should be the same for all natural languages, i.e. that it should be universal that this artificial language could be discovered by transcendental reflexion and that, in this sense, it would be both normative and pure, etc. Those of you who were in Amsterdam will recall that Montague (of whom only few people knew until then that he had been exploring for years, very intensively, the logic, semantics, and pragmatics of natural languages) pleaded in favour of the direct method and gave the first hints of his researches which, shortly thereafter, started to appear in print in quick succession and which are about to influence decisively the attitudes of many professional mathematical logicians towards the logic of natural languages and the attitudes of many linguists towards the use of the sophisticated methods of mathematical logicians and model theorists for their own purposes. But the mode of expression I just employed is not sufficiently precise. Montague objected to the deep structure syndrome only, or mainly, because it originated from the M.I.T. school of linguistics (the so-called MIT-niks) whose work appeared to him as lacking in precision to such a degree as to be ruled out of serious competition. But in his own semantics, Montague by no means dealt with sentences of natural languages as such but rather with ordered pairs of such sentences and so-called indexes, which themselves were ordered quadruples of points of reference indicating the speaker, the audience, the time, and the place of the various utterances - in short, describing the situation of the utterance, and all this relative to possible worlds. In

addition, he had no objections to representing these constructs in a language of intensional logic, i.e. to a typically indirect treatment. Among others, Katz was in favour of the direct method at that time, pleading for the method of semantic representations in his own specific sense. I am mentioning all this only in order to deal finally with the decisive question: How does all this look five years after Amsterdam? Although the chaos in linguistic semantics has rather increased since then and will probably increase even more over the coming years, and dozens of new systems, or at least of new variants of old systems, are making and will make their debut each year - some of which might even show up during this panel discussion and perhaps also in the other discussions - this much seems to be clear to me: The opposition between logical and linguistic methods of dealing with the semantics of natural languages is progressively evaporating. And when one reads two contributions by unknown authors to the problem of, say, the presuppositions, the number of logical inaccuracies on the one hand and of naive sounding remarks on linguistic usage on the other, this might provide a clue to their different backgrounds; but this can hardly be deduced from the contents of their remarks.

With regard to the subject of deep structures, surprisingly enough hardly anything decisive has happened in the course of the past four years. Whereas Montague officially declined to work with transformations, though he did, of course, work with other means which perform some of their functions, David Lewis, for instance, no longer has any scruples of this sort. The variety of opinions as to how the deep structures should look, which types of transformations are strong enough but not too strong, has even increased, and it is anything but clear whether something like deep structures are at all necessary for the logical form, although, as far as I know, no seriously competitive conception has been put forth as yet. Neither has anything definitely new been said about the best form of semantic rules. Today it ought to be rather generally recognized that dictionaries and rules of projection are far from sufficient (though Katz, in his new book *Semantic Theory* (1972), continues to make do with such means); but it cannot be said that there has been a consensus or even a rapprochement on the question of the format of the needed additional rules. Likewise, there has been rather a sharpening of the differences of opinions as to which type of logic should form the basis of the semantics of natural languages, whether this is the correct mode of expression at all, and if so, where the line should be drawn between semantics and the basic logic, and whether, e.g., logic can be identified with universal semantics in any serious sense - as I once proposed - which would then also require an explication of the term "universal semantics" which has not been seriously undertaken so far. This task, by the way, is very urgent anyway, since the prefix "universal" has come into general use lately, not only in connection with universal languages and universal grammars, but also with universal syntax and recently even with universal pragmatics. Chomsky has already pointed out the double meaning of "universal" in this context, viz., firstly, as *common to all languages* - in which case one could further differentiate between "universal" as necessarily common and "general" as accidentally common if this suits the author's philosophical position, as I also tentatively hinted at the time: and secondly, as *determining the conditions* that any adequate semantics must fulfil, thus *transcendental* in a quasi-Kantian sense. It ought to be clear what a hornets' nest one would thereby stir up, and this is probably the main reason why I have not yet seen any further elaboration of Chomsky's ideas in this connection although I am convinced that we shall hear more about this in the course of our meeting. Another reason for this neglect may lie in the fact that the expression "semantic universals" is much more attractive than "universal semantics" since the former expression evokes associations with profound perennial philosophical problems, whereas the latter reminds us, at most, of Leibniz' *characteristica universalis* which, since the early 19th century, was no longer able to stir up the interest of linguists, at least not until the publication of Chomsky's *Cartesian Linguistics*.

I hope that what I have said will suffice to indicate that I am not prepared to recognize substantive barriers of any kind though I would not go so far as to accept the formula of Montague and many others to the effect that there are no differences whatsoever between natural and constructed languages - as this formula seems to me far too extreme and only liable to revive unnecessary controversies.

The breach of the last psychological barriers between logicians and linguists with regard to the treatment of the semantics of natural languages came from a quite unexpected direction, namely from syntax syntax. An interesting development emerged in recent years, of which I am not sure

whether it is known to all interested linguists, although we have amongst us some active participants in this development. What I mean is the renaissance of the *categorial grammars*. It is assumed by many that Chomsky's phrase structure grammars (regular, simple, context-free, linear, context-sensitive, etc.) are adequate at least for the basis of the natural languages, perhaps even for the generation of the set of *all* sentences of natural languages as well, but at least for the generation of the sentences needed in computer languages; thus an immense literature has appeared within a short period, dealing with these and similar grammars in their innumerable axiomatic details, examining the scope of their application, ascertaining their weak and strong equivalences and non-equivalences, etc. I myself have been occupied with such things for some years. However, it is known to a minority only that the first attempts at an algebraic linguistics were concerned with grammars of an at least superficially different structure, that is, with the categorial grammars developed mostly by Ajdukiewicz on the basis of beginnings made by Kotarbiński and Leśniewski, and developed some more by myself and my collaborators. These grammars have considerable pedagogical, but not less substantive advantages of the phrase structure grammars, as was recently pointed out by, for instance, Mr. Potts here and by Mr. Lehrberger who is a pupil of Mr. Hiż here though not yet published. Therefore it is rather a pity that I myself interrupted the development of these grammars at the time, i.e. in 1953 (although I still published something about them from time to time), for reasons of which I already gave a hint elsewhere but which I now wish to describe at some greater length since they seem to me important for the revival of these grammars. Apart from the banal fact that in that year I returned from the United States to Israel and my new academic duties occupied me fully over that period, leaving no time for further serious research, the principal reason was that I had come up against barriers which, in 1953, I did not know how to overcome in a simple manner although I had some ideas. Of these numerous barriers I shall mention, for the sake of brevity, only two: 1. *Discontinuous expressions*, such as "called...up" as in John called his girl friend up where the length and sometimes also the syntactic structure of the strings between the parts are indeterminate, or at least appeared thus to me then, and could not, therefore, be dealt with by means of the classical categorial grammars nor, for that matter, of the classical phrase structure grammars. 2. The fact that certain expressions, as for instance ad-sentences (sentential adverbs) like "unfortunately" can be interpolated in simple sentences in almost any place, which again could not be dealt with by the means current at that time. I was not clever enough to notice that all this, and much more, could be done by means of Chomskyan transformations, for the simple reason that at that time there existed no transformation grammars. When Chomsky discovered or invented them in the following year, he unfortunately based them on phrase structure grammars, probably because this type of grammar was closely related to the constituent grammars which were common in America at the time so that he could avoid introducing two essential innovations simultaneously and could save his strength for the one revolutionary innovation.

Mr. Hiż here is doubtlessly acquainted with the categorial grammars since his student years in Poland, and he is still working on their further development with important results. I have just mentioned a new Technical Report by one of his students. Montague probably heard of these categorial grammars from his teacher Tarski, who being a pupil of Leśniewski and Kotarbiński, knew of them as a matter of course though he did not himself engage in their development. Then David Lewis probably heard about categorial grammars from Montague. Peter Geach, who is not with us, knows Polish, was always Interested in Polish logic, and probably became interested in the categorial grammars through this source. I presume that my colleague, Mr. Potts here, got Interested through him. I am not saying all this because the biographical details are of particular importance, but only in order to emphasize my belief that the revival of the interest of eminent philosophical logicians in categorial grammars would probably have died down again had not men like David Lewis, Potts and others, independently from each other and from me (though I made occasional mention thereof in lectures and seminars, I had not published anything important on the subject in years), hit upon the decisive, though almost obvious, idea to develop a *categorial-transformational grammar*, analogous to the phrase-structure-transformational grammar, but superior to the latter in essential points. For us it might be of decisive importance that there is every indication that it will be feasible to create for categorial grammars a semantics which would be adequate for logical purposes, by means of rules with a clearer structure, than for other grammars current today. Should it turn out that, additionally, one can make do with

transformations which do not alter the semantic contents of the transformands (and, despite Chomsky, there is a certain chance for this although it is not yet possible to predict what price will have to be paid for this extremely useful property), then I foresee that categorial grammars will gain in popularity, both with logicians and with linguists - and this, as already adumbrated, would eliminate the last barriers to an understanding between them. And since the elimination of these barriers is the *Leitmotiv* of my crusade of nearly twenty years' duration, let me close my discourse on this optimistic note.