

REVIEW ARTICLE

Danny D. Steinberg and Leon A. Jakobovits (eds.), *Semantics. An Interdisciplinary Reader in Philosophy, Linguistics and Psychology.* Cambridge University Press, London, 1971, x + 603 pp. Cloth £ 6.80.

Semantics has been through a bad period in linguistics. Whereas, over the past fifty years, great progress has been made in the understanding of linguistic structure, first in structuralist linguistics, then in transformational grammar, little has been achieved in relating linguistic structures to their meanings. Indeed, the very notion of meaning has, by and large, remained obscure.

Much more progress has been made in philosophical semantics. Although here, too, the fundamental concept "meaning" has not been clarified significantly beyond 19th Century and earlier notions, radical new developments in the understanding of logical structures went hand in hand with a careful and subtle analyses of their semantic properties. Without necessarily committing themselves as to the nature of the phenomenon "meaning", logicians operate with it in terms of truth conditions and entailments associated with logical structures. Especially the notion of reference has been investigated in great depth, in connection with the extension and the intension of terms. Great progress has been made in the study of quantification, of modalities, of propositional connectives, to mention just a few areas.

Mainly under the influence of the Oxford school of "ordinary language" philosophers, persistent attempts were made at recognising, one way or another, the semantics of logical structures in "ordinary", i.e., natural, language. (An outstanding example of this tendency is found in P. Strawson, *Introduction to Logical Theory*, London 1952.) In fact, two main streams seem to emerge in modern logic. On the one hand there is pure mathematical logic, a highly technical affair, closely connected with the study of mathematical foundations. Mathematical logicians tend to look upon natural language as vague, unfit for precise logical operations, and often even as just uninteresting. On the other hand we have so-called philosophical logic, which is less interested in systems and their properties than in the nature of logic, its value for and function in our understanding of the world, its relation with the mind and its relation with language. It is this latter tradition which is now beginning to inject new life into linguistic semantics.

Linguistics, however, is not simply the passive receiver. Slowly but noticeably, linguists have been recovering from their semantic "neurosis", the result of a period of barren behaviourism pervading virtually the whole of structuralist linguistics. With the advent of transformational grammar linguists dared again to speak openly about meaning, even

though it was felt, in the late fifties, that one was largely groping in the dark. That Chomsky, in 1957, used ambiguity as an argument for syntactic analysis was more an act of courage and confidence than the result of careful thought. Nowadays such matters have been sorted out a little better, so that we now tend to separate semantic from syntactic arguments. The re-appearance of the concept "mind" in linguistic theory has made linguists rather eager to make up for their semantic arrears.

Meanwhile, in the process of developing syntactic arguments and theories, linguists were often struck by what could only be seen as intriguing parallels between syntactic and semantic properties. It is clear that in this situation some flirtation was bound to arise between philosophers and linguists. The former had at their disposal a host of subtle and acute, often very puzzling, observations of semantic properties of terms and sentences in natural language, as well as some experience in dealing with semantic properties of logical structures, but they lacked a clear insight into the structural properties of sentences. The latter were not nearly as sophisticated in their semantic observations or theory, but had considerable knowledge of linguistic structures. The two together might give birth to a great synthesis.

In fact, some flirtation has been going on for the past five years or so. Certain observations (such as Quine's cases of opaque reference, or Geach's *Only Satan pities himself* versus *Only Satan pities Satan*) began to trickle into linguistic papers, notably McCawley's, suitably adapted to a more trendy transformational sociolect (*Only Lyndon Johnson voted for himself/Lyndon Johnson*). The notion of presupposition has gained prominence in transformational literature, whereby, invariably, due reference is made to Stiawson's and other philosophers' writings on the subject. Austin's notion of performative utterance has been discovered by linguists, and attempts have been made, mainly, but not only, by Ross, to detect performative verbs in syntactic deep structure. The question of the relation between syntactic mappings from deep structure to surface structure and semantic mapping relations holding between surface structures and their semantic analyses (presumably in the form of some logical analysis) began to be mooted, – first in a rather confused way but soon in more and more crystallised form. Philosophers and linguists are seen, these days, reading each other's books and using each other's arguments.

With the appearance of the Steinberg-Jakobovits volume on *Semantics* things seem to have developed into a regular affair. Yet the book does not consist of two, but of three parts, as the title indicates. The section on Linguistics, well over 300 pages and by far the largest, finds itself sandwiched between a part I, Philosophy, of just over 150 pages, and a part III, Psychology, of just over 100 pages. This is, perhaps, slightly symbolic. The linguistic contributions are almost all clearly oriented towards philosophy; the psychological papers tend to discuss the feasibil-

ity of interpreting meaning and semantic properties in terms of psychological structures. In so far as the papers in part III do this, they represent the uneasy bond which existed in the days of structuralism between whatever there was in the way of linguistic semantics and behaviourist psychology. It is clear that this engagement has now come to an end, and is being replaced by a new partnership.

This appears most clearly from Charles Osgood's article in the Psychology section, "Where do sentences come from?" (pp. 497–529). Osgood is well known from the heyday of behaviourist psycholinguistics: he was one of the two editors of the 1954 volume *Psycholinguistics: A Survey of Theory and Research Problems*. In this paper he makes an attempt at deriving all sorts of grammatical and semantic properties, observed in recent literature, from basic elements figuring in behaviourist theories. He tries to establish the "psychological" nature of presuppositions, of some of Fillmore's cases, of determiners and pronouns. Although he discusses recent developments in linguistic theory at length, he remains a disapproving onlooker and essentially an outsider. This is borne out not only by little things, such as his referring to the author of the 1969 article in *Language*, "Some reasons why there can't be any *some-any* rule", as "he", whereas the author is in fact, although called Lakoff, a "she" (p. 500). There is also his superficial, even tendentious, interpretation of various assertions made by transformationalists when they commit themselves to a mentalistic rather than a behaviouristic position. This is interpreted as though these authors commit themselves to a psychological, rather than a linguistic, point of view. (Examples are found on pp. 499–502, and elsewhere.) Where he speaks of transformational grammar (pp. 519–522) he betrays a serious lack of understanding: deep structures are not, and were never thought to be, "kernel sentences"; transformations do not turn one sentence into another; a sequence of rules is not meant to represent the steps a speaker goes through in producing an utterance.

Then, without warning, Osgood proceeds to present a theory of meaning in behaviouristic terms; he calls it "Representational Mediation Theory", which is, he says (p. 522), "the only learning approach that has seriously attempted to incorporate the *symbolic processes* in general and *meaning* in particular within an S-R associationistic model". Meaning is said to be an " r_m ", or: mediating reaction to signs. If a perceptible object regularly and reliably provokes a reaction which is a significant part of what is otherwise provoked by something else, then the object or event is a sign of that something else, the "significate".

Leaving all other possible objections aside, it is at least clear that no distinction can be made, in this theory, between cognitive and non-cognitive meanings: both would have to fall under the general heading of "behaviour produced". But then, one wonders, why does Osgood stress the "cognitive nature" of presuppositions (p. 500), of case (pp. 507/8),

etc.? But, more significantly in this context, one also wonders what prompts him, first, to relabel mentalistic notions of meaning and presupposition as "cognitive" or "psychological", and then to cast meaning into a behaviouristic mould as part of a particular tradition in psychology. It looks like a last attempt to win the linguists' hearts for a lost cause.

The cause is considered lost by J. Fodor, in his article "Could meaning be an r_m ?" (pp. 558–568), where he maintains that the Mediation theory appears to differ from Single Stage behaviourist theories of meaning only in that some *part* of the gross response provoked by the signifiante is taken to be provoked by the sign, rather than the gross total response. Since the particular part of the gross total response which is to be associated with the sign is not specified, the claim that Mediation theory differs from Single Stage theory is either empty, or, if it can be filled, untrue. For either there is a one-to-one relation between the part and the whole, in which case all the well-known and generally accepted objections to the Single Stage theory carry over to the Mediation theory, – or there is a many-to-one relation; but then every sign would be inherently and essentially ambiguous in many ways, which is not so.

I shall be brief about the remaining papers in the Psychology section. In a short but excellent paper David McNeill raises the question of the relation of linguistic universals vis-à-vis other species-bound mental properties (pp. 530–535). The problem, interesting though it is, seems to have little direct bearing on problems of semantics. Then there is a much longer contribution by Eric Lenneberg, "Language and cognition" (pp. 536–557). Lenneberg attempts to establish, on the basis of psychological experiments, that "there is little evidence of the tyrannical grip of words on cognition" (p. 551). He thus rejects the Sapir-Whorf hypothesis, according to which cognition is guided by the vocabulary of the speaker's language. A less inspired article by George A. Miller, "Empirical methods in the study of semantics" (pp. 569–585) concentrates on the question of how to measure semantic similarities among lexical items. The results of his experiments do not seem to elucidate any empirical problem raised by semantic data. It may be a little unfair to judge the article by the first sentence of its summary; yet this is what it says: "A too short summary of this paper might be that language is what it is because we use it to say things" (p. 584). The remainder of the summary does not help much, either. Finally there is an article by Thomas Bever and Peter Rosenbaum "Some lexical structures and their empirical validity" (pp. 586–599). They use intuitions of selectional restrictions to establish hierarchies of lexical features and of lexical items. These intuitions are notoriously unreliable and idiosyncratic. Moreover, their use of the asterisk to mark certain collocations remains undefined: one gathers that an asterisked sentence contains a violation of some selectional restriction rule, but it is vaguely implied, on p. 590, that syntax has somehow to do with it: "There are several 'syntactic' consequences of the inclusion of hierarchies

as part of lexical structure. (As could be expected for grammatical structures dependent on the lexicon, the distinction between 'semantic' phenomena and the corresponding 'syntactic' phenomena is extremely difficult; and beside our point.)" It is not clear what the quotes around "syntactic" and "semantic" imply. At any rate, the authors come to some rather strange starrings: "A cannon is more deadly than a gun" (p. 593) is starred because a cannon is a kind of gun. "A cannon is more deadly than a pistol" passes without an asterisk. By this principle, "A rose is not a flower" should be starred. These asterisks have become adulterated.

The main importance of this book lies in the bringing together of a number of papers written by philosophers and linguists, all concentrating on a core of common problems, though from different points of view. The first three papers in the Philosophy section deal largely with the question "whether a word has one, several or many senses", which seems a matter of relating surface structures, in this case lexical items, to semantic analyses. The contributors are David Wiggins, William P. Alston, and Wiggins again. Interesting though these discussions are, they do not seem to shed much light on the vexed question of the correct form of dictionary entries, or lexical items, in an adequate description of a language. In particular, it is not clear what is meant by Wiggins when he speaks of "a philosophical theory of dictionaries" in the title of the first article. The two writers appear to have strayed, unawares, into empirical territory, for which they are not optimally equipped. (Not all that uncommon: Quine's second chapter, on "Translation and Meaning", in *Word and Object* contains a great deal of spurious linguistics.) This is not to say that linguists have provided the answers to lexical problems; far from it. These answers no doubt require sustained combined efforts of linguists and philosophers, situated as lexical items are on the interface of grammar and semantics.

There is a subsection, called "The Lexicon" in the Linguistics section (pp. 370-482). Fillmore, Bendix, Bierwisch, Dixon and Hale tackle lexical problems in a more empirical vein: theoretical interpretation of data collected. I was especially impressed with R. M. W. Dixon's "A method of semantic description" (pp. 436-471), and Kenneth Hale's "A note on a Walbiri tradition of antonymy" (pp. 472-482).

Dixon describes a particular sociolinguistic phenomenon in Dyirbal, an Australian aboriginal language spoken in North Queensland. Dyirbal has two distinct varieties, the unmarked everyday language, and a "mother-in-law language", used in the presence of certain taboo relatives, such as the mother-in-law. The two varieties have virtually the same phonology and grammar, but entirely different lexical items. Ordinary Dyirbal has about four times as many lexical items as the MIL-language. For example, where the former has distinct items for "cut deeply, sever" and "cut less deeply", MIL has only one, "cut". On the whole, where

ordinary Dyirbal has highly specific lexical items with subtle shades and differences, the MIL-language has only general terms with a wide application. This observation led Dixon to posit a distinction between nuclear and non-nuclear verbs: "look", he says on p. 441, is a nuclear verb, but "stare" is not. The MIL-language appears to avoid non-nuclear verbs. This distinction suggests a hierarchy in the semantic description of lexical items.

Hale, too, derives his data from an Australian aboriginal language, Walbiri, spoken in the western part of the Northern Territory. He, too, deals with a sociolinguistic phenomenon. The Walbiri have an advanced form of ritual initiation for their men. Those who are fully initiated have to learn a new "language", which is characterized mainly by a replacement of each noun, verb and pronoun of the normal language by an "antonym". Walbiri sometimes refer to this "language" as "the funny language" or "the upside-down language". What is interesting from the point of view of lexical structure is to see precisely how they "invert" the meanings of the various words. Thus, e.g., for "You are tall", they will say "I am short", or for "I am sitting on the ground" the initiated will say, to each other, "He is standing in the sky", or for "This hill-tobacco is strong" they say "That plain-tobacco is weak", for "I am thirsty" is said "He quenched", for "Give me water" one finds "I withhold fire from him", etc. Hales writes: "It is, of course, not surprising that *tjiliwiri* [the antonymous language] is semantically based in the particular sense that it makes use of abstract semantic structures – it would be difficult, otherwise, to conceive of how an individual could learn the system in a relatively short time through exposure to a severely limited and highly unsystematic sample of actual speech. It is abundantly clear that a general principle is learned and that, once acquired, the principle enables the learner to create novel *tjiliwiri* sentences and to understand any well-formed novel sentence spoken by another. This general principle of antonymy is determined by a semantic theory which we know, on other grounds, must be shared by the speakers of Walbiri. The *tjiliwiri* activity provides us with a surprisingly uncluttered view of certain aspects of this semantic theory and is certainly not irrelevant to the much discussed, though occasionally incoherent, question of whether the semantic structures we, as students of language or culture, imagine to exist do, in fact, have any "reality" for the speakers who use the system." (p. 478). He also asks us not to mention *tjiliwiri* to any Walbiri or other Aboriginal unless we are sure that he is fully initiated: "While many Walbiri are eager to have the material recorded and published as a matter of scientific record, they request strongly that it be handled with care." (p. 472).

It is a pity that the editors have not deemed any of Jeffrey Gruber's writings on the semantic structure of lexical items worthy of inclusion in this book.

Inevitably, there is an article by H. P. Grice, called "Meaning" (pp.

53–59). It is a reprint of his classic semantic paper of the same title, in *Philosophical Review* of 1957. Grice's aim is to explain the notion of "meaning". He distinguishes two kinds of meaning, the natural and the non-natural one ($mean_N$ and mea_{NN} respectively). Natural meaning is found in expressions such as "Those spots mean measles", but also, "for convenience" (p. 54), in sentences of the pattern: "A means (meant) to do so-and-so (by x)", where A is a human agent. Non-natural meaning is the meaning found in, or proper to, linguistic expressions. If x is a state of affairs described by the expression x' , then, according to Grice, we can reduce x' $means_{NN}$ something to: $A \ means_{NN} \ something \ by \ x' \ (on \ a \ particular \ occasion)$, which he again reduces to: $A \ means_N \ to \ induce \ a \ belief \ that \ x \ by \ uttering \ x'$. Non-natural meaning is thus reduced to a particular form of natural meaning. Linguistic meaning is explained in terms of the speaker's intention to induce a particular belief in the listener.

The main objection to this theory is that, although the beliefs a speaker may wish to induce in his audience by uttering a particular sentence s on a number of different occasions may vary widely, the meaning of s does not vary accordingly. Speakers use sentences with all kinds of devious intentions. When I say "Paris is the capital of England" I may wish my audience to believe that this is so, but I may as well be anxious to avoid inducing such a belief, which I can achieve if I am known as a pathological liar; or I may instead wish to induce an entirely unconnected belief, for example a belief that I am such an ignorant person that I am unfit even for military service. In all these cases the meaning of the sentence remains the same: if it did not I could not induce the beliefs I intend to induce.

Paul Ziff, in "On H. P. Grice's account of meaning" (pp. 60–65) elaborates this point with great force and wit. I am entirely on his side, when he writes (p. 60): "Because I believe the coin is counterfeit, because it seems to be gaining currency, I mean to examine and attempt to discredit an account of meaning circulated some time ago by H. P. Grice". Grice's theory seems to be as much of a blind alley as behaviourist theories of meaning.

Gilbert Harman, in "Three levels of meaning" (pp. 66–75) also investigates the nature of meaning: "Philosophers approach the theory of meaning in three different ways. (1) Carnap, Ayer, Lewis, Firth, Hempel, Sellars, Quine, etc. take meaning to be connected with evidence and inference, a function of the place an expression has in one's "conceptual scheme" or of its role in some inferential "language game". (2) Morris, Stevenson, Grice, Katz, etc. take meaning to be a matter of the idea, thought, feeling, or notion that an expression can be used to communicate. (3) Wittgenstein (?), Austin, Hare, Nowell-Smith, Searle, Alston, etc. take meaning to have something to do with the speech acts the expression can be used to perform." He maintains that none of these three approaches to meaning can be satisfactory in itself, but that, if taken together, they may well supplement each other to form an adequate notion of meaning.

I have the impression that Harman oversimplifies the issues considerably, apart from being rather unclear about the characteristics of the three “levels of meaning”. He adds a criticism of Chomsky’s competence-performance distinction (“we would ordinarily ascribe knowledge of the rules of grammar (unconscious or conscious) to a linguist or grammarian” (p. 74)), which shows that he has not grasped the distinction.

The next contribution in the book is by Leonard Linsky, “Reference and referents” (pp. 76–85), a reprint of the last chapter of his book *Referring*, of 1967. Linsky argues that it is primarily speakers, not expressions, that refer (to say that expressions refer is philosophese). And when a speaker uses a proper name, he does not refer; but he does refer, or may do so, by the use of a descriptive phrase, such as “the greatest philosopher of modern times”. In this case it makes sense to ask: to whom did the speaker refer? Thus I may refer to someone as the husband of a lady who is, in fact, a spinster, for example when I think, mistakenly, of the man I refer to that he is her husband. “This, then, is the paradox of reference. In saying ‘I am referring to Ludwig Wittgenstein’ I am not referring to Ludwig Wittgenstein.” (p. 79).

He then proceeds to discuss the old problem of reference to non-existing objects (p. 80): “How is it possible to make a true statement about a non-existent object? For if a statement is about something that thing must exist, otherwise how could the statement mention *it*, or refer to *it*? One cannot refer to, or mention nothing; and if a statement cannot be about nothing it must always be about something. Hence, this ancient line of reasoning concludes, it is not possible to say anything true or false about a non-existing object. It is not even possible to say that it does not exist.” He calls this argument “outrageously bad”: “One might as well argue that I cannot hunt for deer in a forest where there are no deer, for that would be to hunt for *nothing*.” He does not solve the problem, however. The nearest he comes to it is when he suggests that “In speaking about movies, plays, novels, dreams, legends, superstitions, makebelieve, etc., our words may be thought of as occurring within the scope of special ‘operators’. Let me explain. Watching the western, I say, ‘I thought the sherriff would hang the hero, but he didn’t’. The context in which these words are said makes it clear that they are occurring under the ‘in-the-movie’ operator.” (p. 82). On the next page he rightly mentions “a kind of ontological anxiety” which has beset philosophers in connection with interpretation within certain “pictures”, or “operators”. This “anxiety” was and is very strong; it has prevented the development of a satisfactory semantics. It is a pity that this idea of operators, within whose scope reference can be made, is not worked out further by Linsky. Further elaboration would generate a theory of presuppositions, of universes of interpretation, of variable binding, – in short, it would open the way towards a semantics of natural language. For this to happen, however, it is necessary to set aside ontological qualms about such mental struc-

tures as a “universe of interpretation”. Present-day mentalistic linguistics might well be of some help here.

I wish to emphasize that, in spite of the reservations expressed above, Linsky's paper is a masterly piece of work, extremely well written, clear and rich in intuitions and budding thoughts.

The same is true of Peter Strawson's “Identifying reference and truth-values”, reprinted from *Theoria* 1964, in this book on pp. 86–99. He, too, is at grips with the problem of reference and existence. In earlier work, he defended the theory that, if a definite description does not have a really existing referent (fails to refer), then the statement in which it occurs lacks a truth value: it is neither true nor false. The existence of a referent is *presupposed* when a definite description occurs. This early theory of presuppositions ran into difficulties: “The king of France invited me for dinner” is clearly false when I say this to you now (except when I refer, in Linsky's sense, to somebody as “the king of France”, although that is not what he is). It is false precisely because there is no king of France. Or when I say “The monster of Loch Ness does not exist”, then this is true or false depending on whether or not it exists. In order to avoid these objections, Strawson introduces the “Principle of Presumed Knowledge”: what is presupposed is that we *know* what is talked about when a definite expression occurs. This “presumed knowledge” is clearly akin to Linsky's “pictures” or “operators”. It is, anyhow, a mental structure. But the ontological qualms referred to by Linsky seem to bedevil Strawson as much as anybody else. He wavers, inconclusively, in this article between two positions, the one being that what is presupposed is knowledge, the other that what is presupposed is existence. He seems unable to disentangle himself, and ends by disclaiming exclusive truth for his theory of presuppositions: it is “apt to seem more intuitively attractive in some instances than it does in others” (p. 99). This is a pity: a further elaboration of the concept “presupposed knowledge” would have opened up a whole fascinating system of interpretation in context.

Then follows a very important article by Keith Donnellan, “Reference and definite descriptions”, pp. 100–114. He calls attention to a distinction which had hardly been noticed in previous literature: the ambiguity of “Smith's murderer is insane”. The definite description “Smith's murderer” is either used to refer to a particular person, say, Jones, who is said to be insane; or it is not used to refer to anyone in particular, but stands for something like “whoever killed Smith”. The former use he calls *referential*, the latter *attributive*. They are said to differ truth-conditionally in that the sentence is uninterpretable (because of a truth value gap) when there is no previous knowledge as to the identity of the person referred to, – in the referential use, whereas such presupposed knowledge is not necessary for the attributive use. The two uses thus differ in their conditions for the sentence to have a truth value.

This is a highly interesting point, the implications of which are still

tar from clear. For example, the question arises whether or not the sentence quoted above can be taken to be true in the attributive sense even if Smith was not murdered, but died by accident, or through suicide. I find the answer doubtful. Yet, it seems that if the future tense is used the sentence can indeed be true, as appears from the non-contradictory character of a sentence such as "Smith's murderer will get a life sentence, I'm sure; but perhaps Smith wasn't murdered at all". On the other hand, with the past tense there does seem to arise a contradiction: "Smith's murderer got a life sentence, I'm sure; but perhaps Smith wasn't murdered at all". This question looks as if it might well be of relevance for the logic and semantics of tense.

Then, one may wonder what the relation is between the ambiguity signalled by Donnellan and the following: "The Pope has always lived in Rome", which either refers to a particular person, say Paul VI, or to whoever has at any time been Pope. There is, furthermore, the obvious question of how to express the distinctions observed in logical form: existing logic provides no answer, which shows, once again, how far removed both logicians and linguists still are from providing adequate semantic representations of sentences.

The next contribution is by Zeno Vendler, "Singular terms", pp. 115–133. It is taken from his book *Linguistics in Philosophy*, of 1967, but perhaps a title such as "A Philosopher in Linguistics" would have been more appropriate. Vendler aims at defining the notion "singular term" in strictly linguistic terms. He says, in his opening paragraph, that the notion has been defined satisfactorily in philosophy, and that the philosophers' conclusions "anticipate in substance the findings of the advanced grammatical theory of today" (p. 115), of which he then proceeds to give a specimen. I am afraid, however, that the specimen is rather poor, from the linguistic point of view. For one thing, the conclusion he arrives at on p. 132 can hardly be called a result of present-day linguistic theory: "We may conclude, then, that given any *the N wh....-phrase*, it is a singular term if and only if the sentence *There is an N wh....* is entailed by the discourse." It would be nearer the truth to say that this is not a conclusion, but rather a definition given in modern philosophical logic, although many logicians would not like the expression "entailed by the discourse". Then there is the difficulty of how to interpret the phrase "there is". This is either "there is in the real world; there exists", or "there is in the discourse; there is previous knowledge of the identity of ...". The latter seems to be correct, and the former wrong. For if the expression is taken to stand for "there is in the real world", then the definition of singular terms does not cover what it is meant to cover, as appears from examples such as "I never met *the man you talked about last night*, – of course not: he doesn't exist.", where the italicised part is no doubt to be taken to be a singular term. On the last page, Vendler opts for the latter interpretation: "there are things that do not really exist", but he does not make it clear

what, then, is meant by “there are . . .”.

Vendler's linguistic analyses are not very satisfactory. To take just a couple of examples, on p. 128 he derives the article in proper names such as *The Hudson*, *the early Mozart*, from an underlying common noun: “the [river called] Hudson”, “the early [period/works of] Mozart”. But then, how could one account for the Dutch “de rivier de Rijn (the river Rhine)? This would then be “the river the [river called] Rhine”, which is clearly absurd. Or else take the German: “Dieser Fluss heisst der Rhein” (this river is called Rhine). Would this be derived from: “Dieser Fluss heisst der [Fluss geheissen der [Fluss geheissen der [Fluss . . .]]] Rhein”? Such infinite regress must certainly be avoided! (I owe the Dutch observation to Arjen Florijn, of the University of Amsterdam.)

Another example of rash linguistics is found on pp. 120–121, where Vendler asserts that patterns such as “dirty Margaret”, “poor Joe”, “poor she”, “miserable you” are “neither common nor universally productive”. I am not certain as to what sense of “common” Vendler is operating with here, but anyway we do not want to disregard for the purpose of linguistic description all expressions or “patterns” which are less “common”, in any sense, than the examples given.

The problem of reference is again at issue in John Searle's “The problem of proper names”, pp. 134–141, reprinted from his book *Speech Acts*, 1969. Searle contrasts Mill's view that proper names have no connotation, but only denotation (no sense, only reference) with Frege's that what we know about the object or person denoted by the name is part of the meaning of the name, since that knowledge is necessary to identify the object or person. He lists three objections to Mill's theory: (1) We can say “Cerberus does not exist”; how could this be possible if the proper name “Cerberus” merely stood for an existing thing? (2) We can say “Everest is Chomolungma”; how could we explain that this conveys factual information and is not merely equivalent to “Everest is Everest”, unless we assume that names have some sort of sense? (3) How can we use a proper name to identify a particular entity unless the name conveys at least some sort of description?

His answer is that, although names have no sense, they have, what he calls, a “descriptive backing”. We cannot use a name in discourse unless we know at least certain things about the bearer of the name: these things known (or believed) form the necessary descriptive backing of the name. This, he claims, solves the problems raised by the three objections: “A statement such as ‘Aristotle never existed’ states that a sufficient, but so far unspecified, number of the descriptive backings of ‘Aristotle’ are false.” (p. 139). “‘Everest is Chomolungma’ states that the descriptive backing of both names is true of the same object”. (p. 139). I.e., it conveys the information that whatever knowledge is sufficient to identify Everest will also do for Chomolungma, and vice versa. The answer to the third objection is that the descriptive backing enables the speaker or hearer

to substitute some sufficient identifying description for the name.

It would seem to me that Searle does not eliminate objection (1). For suppose, as is indeed the case, that it is part of my descriptive backing of the name "Cerberus" that what is denoted by that name is a figment of the mind, does not exist, and never has existed. According to Searle, the (true) statement "Cerberus never existed" would now state that a sufficient, but so far unspecified, number of the descriptive backings of "Cerberus" are false. But surely, this is not the case here. The problem therefore remains that I can, without any difficulty, use the name "Cerberus" for the purpose of identification in discourse even though Cerberus does not exist and I know perfectly well that he doesn't.

We see the same problem arising here, though in a different shape, as arose in the papers by Linsky, Strawson, Donnellan and Vendler: how can we refer to non-existing objects? This, it seems, is the central problem of the theory of reference. Is there anything in common between Linsky's idea of "pictures", or "operators", for identification, Strawson's notion of "identifying knowledge", and Searle's "descriptive backing"? Is there a way in which we can make sense of Vendler's "there are things that do not really exist"? I think the answer is "yes" to both questions, but we must learn to get over our ontological qualms with respect to mental entities. Let us say that when we refer we do not refer directly, or immediately, to actually existing real intiities, but always to entities in the mind, in a particular universe of interpretation defined by certain universe-defining elements in previous context or in the situation. Every element populating such a mental universe must have at least some "descriptive backing", i.e., it must be characterized by at least some predicate in order for it to be a distinct element. For an element to be characterized by a predicate means for the possessor of the universe of interpretation to be provided with a criterion to decide for any thing presenting itself to him as real whether or not it corresponds to the element characterized. If it does, the predicate is said to be true of that thing.

Clearly, we cannot here go into these questions more deeply. It would be necessary to specify exactly how a universe of interpretation constitutes itself, how it changes in the course of discourse, what kinds of elements are possible denizens of these mental worlds. By introducing this notion of a mental universe of interpretation we have, in fact, set ourselves a considerable programme. But it seems as though we simply have to come to terms with some such notion. For one thing, we will then be able to conceive of a semantic interpretation procedure for what are known as semantic representations, or logical analyses, of sentences. Such a procedure would consist in a model-theoretic mapping of semantic representations into classes of interpretation universes. The semantics of a sentence would then state (a) the presuppositions of the sentence, or the conditions for a semantic interpretation to take place, and (b) the effect the sentence has on the universe of interpretation.

Furthermore, a theory of mental interpretation universes would give substance to Linsky's notion of "picture" or "operator", to Strawson's "identifying knowledge", and Searle's "descriptive backing". It would give sense to Vendler's phrase that there are things which do not exist. "There is . . ." now means: "there is in the universe of interpretation . . .". "Exist" has now become a predicate like all other predicates, and not the name of a quantifier. In fact, this mentalistic semantics fits in precisely with so-called "free logic" as developed recently by Hintikka, Lambert, Van Fraassen, and others. I have a feeling that Quine's cases of opaque reference, as well as Donnellan's attributive use, will find expression in the semantic mapping from semantic representations (logical analyses) into interpretation universes. This is not the place, however, to indulge in further investigation of these matters.

The last paper in the Philosophy section is by Quine, "The inscrutability of reference", pp. 142–154. Quine rides his old hobby-horse here again: the indeterminacy of radical translation. Translation, Quine says, from an exotic, completely unrelated language into our own will give rise to cases of indeterminacy, – that is, there will be no way of assessing the correctness of the translation. Suppose some language has a word *gavagai*, and we, linguists in the field, have discovered, by systematic observation of native speakers' overt behaviour connected with the occurrence of this word, that it has something somehow to do with "rabbit". How precisely, we will then be unable to establish. *Gavagai* may stand for "undetached part of a rabbit", or "rabbit stage": there will be no difference in overt behaviour when that is so. Since Quine takes it for granted that "language is a social art which we all acquire on the evidence *solely* of other people's overt behavior under publicly recognizable circumstances" (p. 142; italics mine), and that meanings are mental only to the extent that dispositions to overt behaviour are mental (p. 143), one is not surprised to find him concluding that it is intrinsically impossible to decide what *gavagai* actually refers to: the whole rabbit, or an undetached part, or a rabbit-stage. Since, however, the behaviouristic premises on which this conclusion depends are no longer taken seriously, Quine's problem of the inscrutability of reference vanishes into thin air. If the linguist were to rely, for his descriptive work, only on behavioural data, the problems he would face would be rather more serious than Quine's referential inscrutability. In fact, of course, the linguist relies, and has always relied, on introspective data, i.e., native speakers' judgements as to well-formedness of sentences, their possible meanings and their possible pronunciations. Such data is collected not by speechless observation of behaviour, but by asking, listening and understanding.

Another remark to be made in connection with Quine's unworldly theorizing about very worldly linguistic fieldwork is that all that seems to be at issue is the problem of other minds. It is not clear to me what is gained by disguising this problem in linguistic pseudo-issues.

Then there is the Linguistics section. The part of it called *The Lexicon* has already been discussed. There remain the parts *The Role of Semantics in a Grammar* and *Meaning, Presupposition and Reference*. The section opens with a long “overview” by Howard Maclay, intended as background information for those who might not be very familiar with transformational grammar and its complications. Unfortunately, this survey barely skims the surface. It fails to provide any fundamental insight into the motivation or methodology of the new linguistics. Instead, it enumerates theories and formalisms.

The part on semantics and grammar contains Chomsky’s paper “Deep structure, surface structure and semantic interpretation”, pp. 183–216. When Chomsky wrote this paper, in 1969, it was an important contribution towards the discussion about the status of syntactic deep structure. Quite a few linguists had begun proposing the theory that the syntactic deep structure of a sentence is indeed identical with its semantic representation (SR). This theory followed, historically and naturally, from the Katz-Postal theory that all semantic properties of a sentence are fixed at deep structure level. Since it is not an *a priori* matter that the deep structure posited for purely syntactic reasons should be identical with the SR posited for semantic reasons, the question is an empirical one.

In this paper Chomsky proposes that syntactic deep structure should not be taken as containing all semantic information of a sentence, but only part of that. Having raised doubts as to the actual empirical differences involved between his “extended standard theory” and “generative semantics”, he then points out a number of difficulties arising in connection with the latter. Although the actual issues involved stand out much more clearly today than three years ago, the difficulties raised by Chomsky in this paper are still, for the most part, unsolved and remain disturbing.

By way of contrast, this is followed by James McCawley’s “Where do noun phrases come from?”, pp. 217–231, which is a straightforward specimen of generative semantics. The central issue of this paper is an attempt to give logical expression to the opacity-transparency ambiguity as found, e.g., in “Fred believes that the murderer will be found”. In this sentence, the qualification “murderer” is either the speaker’s (Fred may not believe that the man referred to is a murderer), or else “the murderer will be found” is Fred’s belief. McCawley proposes to split noun-phrases up into variables and descriptions; the latter can be attached to different positions in the semantic tree, so that the underlying position in the tree will determine the sentence’s interpretation.

George Lakoff contributes a very lengthy paper, “Generative semantics, perhaps somewhat ambitious. The paper is far too long to be dealt with in any detail here. Suffice it to remark that there is food in this paper for many a long discussion among specialists.

The second part of the Linguistics section contains contributions by Jerrold Katz (“Semantic theory”; reprinted from his book *The Philosophy*

of *Language*, 1966), by Uriel Weinreich ("Explorations in semantic theory"; reprinted from *Current Trends in Linguistics, III*, 1966), George Lakoff again ("Presupposition and relative well-formedness"), a very short but interesting paper by Terence Langendoen ("Presupposition and assertion in the semantic analysis of nouns and verbs in English"), and the famous paper "Fact" by Paul and Carol Kiparsky.

I shall refrain from discussing all these papers in detail: I could only do them an injustice. But I do wish to call attention to Lakoff's paper on "Relative well-formedness", because this is representative of a tendency in modern linguistics which to me seems pernicious. I mean the tendency to use the asterisk for sentences which, though perfectly grammatical, cannot have some particular meaning. These sentences are said to be "ungrammatical on a reading". The danger involved is that what is, in fact, a semantic observation, will be mistaken for a syntactic one, and that, as a result, syntactic and semantic arguments will not be clearly distinguished. The very issue involved in the controversy between Chomsky's extended standard theory and generative semantics, namely the status of syntactic deep structure vis-à-vis semantic representations, will thus become confused. Or rather, the answer to the question will be pre-empted: if semantic observations are taken to be syntactic, then all arguments built on such observations will also be taken to be syntactic, including the positing of semantic representations. These will then, by fuzzy definition, become syntactic underlying structures.

In this paper Lakoff displays a peculiar variety of this tendency. He stars sentences which, though perfectly grammatical, carry with them certain presuppositions which may seem odd to some people. Thus he stars: "The corpse is sleeping", or: "John told Mary that she was beautiful and then shé insulted him" (that is, on p. 333, but not on p. 336), or: "Jane is a neat housekeeper and she doesn't take baths either". Of this last sentence he says (p. 333): "The construction *A and not B either* carries with it the presupposition that one might expect *A* to entail *not B*. In [22a] *Jane is a sloppy housekeeper and she doesn't take baths either.* such a presupposition is consistent with American cultural values, while in

[22b] ?**Jane is a neat housekeeper and she doesn't take baths either.* it would not be. Hence the ill-formedness of [22b]."

He discusses a number of different examples, all of which are very interesting indeed in their own right. Yet none of them adds any weight to Lakoff's argument that one should take into account, while speaking about well-formedness, pairs of sentences and presuppositions, (PR, S) pairs, rather than just sentences (p. 337). The argument which he does provide is a curious one. If, he says, one does not define well-formedness for (PR, S) pairs, but just for sentences, then one "would define a field of presupposition-free syntax", which would preclude an explanation of the ungrammaticality of **I told you to shave you, (rather than yourself)*, or

the alleged ungrammaticality of *Mary is a taller girl than John* (since this presupposes that John is a girl!).

Apart from the fact that Lakoff makes very liberal use of the term “presupposition” (it is not clear, for example, how reflexivisation could hinge on presupposition in any sense of the term), it would seem that syntax is the study of the formal properties of the sentences of a language; it defines what belongs to the language and what does not. When it appears that given some presupposition, no matter how weird or contradictory, sentence *s* conveys it in language *L*, then, by definition, *s* belongs to *L* and should not be starred. Or else we confuse the definition of the asterisk. It curiously follows from Lakoff’s theory that American English changes when American cultural values change, or that Mr. A, who cherishes certain cultural values, speaks a different language from Mr. B, whose cultural values are rather different. Earlier in this review I referred to Bever and Rosenbaum’s paper in the Psychology section, and their contamination of the definition of the asterisk. Here, too, in Lakoff’s paper, the asterisk is adulterated, to the detriment of clarity in linguistic argument.

It will be clear, after this rapid tour of the Steinberg-Jakobovits volume, that this is a very important book. Not only does it bring together a number of, on the whole, high quality papers, it is also approximately representative of what is being done in serious semantics at the moment. It is not the editors’ fault that so many issues are still unresolved or wide open. It is, rather, their merit to have shown, by putting this book together, the sorry state semantics is in.

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