REVIEWS

Samuel Guttenplan (ed.), *Mind and Language: Wolfson College Lectures 1974,* Oxford, Clarendon Press, 1975. pp. 158.

This volume is a collection of seven essays, six of which are more or less extensively rewritten and supplemented transcripts of the series of Wolfson Lectures given in Oxford in the Spring of 1974. The contributors are G. E. M. Anscombe, D. Davidson, M. A. E. Dummett, P. T. Geach, D. Føllesdal, and W. V. O. Quine. The seventh essay is a further piece by Quine of which the editor claims, what would actually hold of almost any of Quine's philosophical writings, that it is "an illuminating companion" to his lecture. There is in addition a short introductory article by the editor.

The title of the volume might encourage the expectation that the essays are devoted to a single theme: philosophical questions to do with the relationship between the concepts of thinking and speaking. In fact this is true only of Davidson's essay, though Quine's second contribution touches on the area. Nor, contrary to the impression given by the Introduction and with the exception of the contributions of Dummett and Geach, are the essays united by a concern with Fregean topics. The questions treated are actually quite diverse, linked only by being of substantial recent interest to workers in the philosophies of language and mind; and each of the essays is quite self-contained.

The Introduction contrasts contemporary philosophy of language with its 18th century precursor which, it seems, tended to suppose that the fundamental questions concerned the first origins of language — rather as we might ask how Chess first developed. Guttenplan attributes to Frege's work our realisation that the connection between mind and language is determined by the fact that "concepts needed to describe the one are part of the description of the other"; and he attributes to this realisation our dissatisfaction with the presumption of the 18th century genetic approach that we may coherently speculate how languageless but otherwise mentally fully equipped creatures might evolve a system of communication.

If this were true, it seems to me, we should have rejected the 18th century tradition for a bad reason, since the question whether the same concepts are needed to describe linguistic practice and mental activity is quite independent of the question whether the latter could exist without the former. In fact, the scepticism which we feel about the legitimacy of the 18th century presumption is probably more directly the effect of the work of the Logical Positivists, and the later Wittgenstein, than of Frege's writings.

In "Thought and Talk" Davidson's principal thesis is that no creature can have thoughts unless it has the ability to interpret the speech of others; whether or not it presupposes the ability to speak, thinking at least presupposes the ability to understand speech.

A slightly paradoxical result of the truth of this claim would be that animals, even those we rank among the more *intelligent* species, have no thoughts; so the simplest patterns of teleological explanation of behaviour, those proceeding in terms of an attribution of a desire and a belief, — to which Davidson adverts in the course of the paper — are inapplicable to them. If we seek to explain the behaviour of a cat scratching at the food cupboard door in terms of its desire to eat and the belief that the cupboard contains food, we fail. Indeed, it would be an error to attribute any *desire* to the cat, for it is only against a background of attributed beliefs that its behaviour can be evidence for its possession of particular desires. And if such a result is paradoxical for animals, it is more so in the case of people who, lacking for medical or other reasons the ability to understand speech, succeed nevertheless in directing and organising their lives.

The final stages of the route whereby Davidson arrives at this position could have done (though not in Dummett's opinion — see p. 117, lines 26-7) with better signposting. The crucial turns come only in the last couple of pages of the essay. By then Davidson has sketched his views, familiar from other writings, on the character of the knowledge which a more or less successful interpreter of others' speech must have; and on the limitation inherent even in the best speech-interpretative theories, that they will standardly involve the attribution on occasion of false beliefs to members of the relevant speech-community. Given this background, it follows that in order to interpret the

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speech of others, we need to possess the concept of false belief. But Davidson now proceeds as if he had established the converse — that in order to possess the concept of (false) belief, one must be an interpreter of the speech of others. For his next step is to argue that only possessors of the concepts of true and false belief can actually *have* beliefs, on the ground that someone cannot have a belief unless he understands the possibility of being mistaken. However, Davidson produces no argument for this last claim; and I have to confess that the precise manner eluded me in which the required dependence, of possession of the concept of belief on the ability to interpret others' speech, was supposed to emerge.

Davidson notes (p. 16) the familiar idea that fine distinctions in belief — those resulting from our preferring one description of a belief to an extensional equivalent — are left underdetermined if all we have to go on is non-linguistic behaviour. If it is contingently true that a = b, only *linguistic* acts on Smith's part can supply a basis for attributing to him the belief that Fa while withholding attribution to him of the belief that Fb. But Davidson does not regard this thesis as sufficient for his main claim; at best they show that "unless there is behaviour that can be interpreted as speech, the evidence will not be adequate to justify the fine distinctions we are used to making in the attribution of thoughts." I should have thought, however, that if we really cannot do anything to justify our preference for attributing to Fido the belief that his master is at home, rather than the belief that the president of the bank (who is his master) is at home, it is seriously open to question whether the dog's behaviour can give sense to the attribution which we choose to make. And the same would then apply to any attribution of cognitive thought to non-articulate creatures. If that is right, then Davidson misses the opportunity to explore another route whereby his main thesis, or something interestingly akin to it, might be shored up. And a fuller exploration of the conditions for correct reporting of beliefs might have been a welcome contribution to our understanding of the semantics of the apparently non-extensional.

Føllesdal's paper, "Meaning and Experience", is mainly concerned with the topic of radical interpretation, and the ideas proposed in connection with it by Quine and Davidson over the years. The interest of the topic, as Føllesdal clearly brings out, derives in large measure from the focus into which it brings the question how our experience can serve as evidence for our judgements of meaning; for, from the sort of general empiricist standpoint to which Føllesdal and his luminaries subscribe, our judgements about meaning cannot legitimately be more refined than our experience constrains us to make them.

After some brief introductory remarks about the intentionality of experience — the sense in which the character of the "irritations of his sensory surfaces" does not completely determine what, as it seems to the experiencer, he is experiencing — and its place in the philosophies of Husserl, Heidegger and Sartre, Føllesdal passes to discussion of Quine's thesis of the indeterminacy of translation. Føllesdal is concerned to argue that this indeterminacy is not a special case of the under-determination of theory by fact, and to bring out what he sees as two quite distinct paths in Quine's thought leading to indeterminacy of translation as the conclusion. The first path utilises the premise of Piercean verificationism, – that the meaning of a statement consists in the difference its truth would make to possible experience, — together with the thesis of the underdetermination of our theory of nature by the totality of experience possible for us. The idea is that, by the latter thesis, there will be more than one way to interpret the speech of another consistently with all the data; whence, by the Piercean premise, the distinctions between rival hypotheses in alternative such interpretational theories are empty of content.

Føllesdal's exposition of this point, however, does nothing to reduce the sense of puzzlement one has about the interest of this conclusion when drawn, as it appears, from incompatible premises. For if someone is a Piercean empiricist, how can he subscribe to the idea that *distinct* theories may be compatible with the totality of evidence available to us?

The second path to indeterminacy is, Føllesdal owns, nowhere found explicitly in Quine's writings. It is based on the idea that all the truths that there are are candidates for description within our theory of nature, and that in translation we are not adding to that theory — "we are not describing a further realm of reality" — but are simply correlating two such comprehensive theories; so there are, as it were, no facts for the translation to get right or wrong. Hence indeterminacy of translation. Føllesdal does not make it clear, however, how he would reply to the complaint that the force of this 'argument' simply depends on a prior choice about just what kinds of fact there are; if, in particular, there are facts about the rightness or wrongness of a certain translation of one language into another, then our comprehensive theory of nature will have to include them. In general my impression of the status of, and relations between, indeterminacy of translation and under-determination of theory by experience in Quine's thought was little clearer after Føllesdal's discussion than before.

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Next Føllesdal compares the respective conceptions of Quine and Davidson of the conditions which radical interpretation ought to aspire to meet. He brings out clearly a range of difficulties in the notion of 'same stimulus' underlying Quine's conception of stimulus-meaning; and pinpoints the limitations of the Charity Principle that in translation one should aim simply to maximise agreement between the two linguistic communities. Charity, Føllesdal argues, must at least be tempered by some sort of *weighting* among types of judgement; agreement about phenomena, for example, seems more important than agreement about one's immediate physical environment, which in turn seems more important than agreement in more theoretical judgements. Moreover the evidential sources for a translation must be widened beyond the Quinean confines of observation of assent and dissent elicited by the querying of sentences in various stimulus-situations. We ought to take account of spontaneous verbal behaviour, other uses of language besides assent and dissent, and other language-related activities, — especially ostension. We must take account, indeed, of speakers' actions in general, and particularly of those non-linguistic actions prompted by linguistic interaction with other speakers. And we should be prepared to let evidence drawn from these wider fields compromise the aim simply to maximise agreement; we should aim to maximise agreement, that is, only within the constraints of an appropriate weighting of types of judgement and of due heed to the total range of behaviour, verbal and non-verbal, displayed by the studied linguistic community.

Anscombe's "The First Person" is an extremely subtle and intricate contribution to one of the most difficult issues in the overlap area between the philosophies of language and mind: the problem of giving a satisfactory account of the semantic rôle of "I". No short summary will give an accurate impression of the knotted geography of her argument. In very broad outline, however, it is as follows: —

(i)   Is "I" any kind of referring expression? If it is, it cannot be *explained* as "that word which each person uses to refer just to himself"; for the "himself" in the explanation has to be construed not as the ordinary reflexive — for which "speaking of himself" is compatible with his ignorance that he is speaking of himself — but as what is called by grammarians the 'indirect reflexive', whose rôle has to be explained by appeal to the use of "I". (p. 46).

(ii)   Moreover the existence of an expression which everyone uses when and only when speaking of themselves is no guarantee of the currency of an expression with the meaning of our "I". For merely to be master of an expression of whose use that is a correct characterisation is no guarantee of self-consciousness. (p. 49 − 50).

(iii)   "I" is misconstrued if taken to be any kind of proper name. Certainly "I" has the prima facie syntax of a proper name; it is substitutable salva congruitate — provided appropriate adjustments are made to verb inflections, pronouns in apposition, etc., — for personal proper names in subject position. And it obeys the rule: if X asserts "F(I)", X speaks truly if and only if F(X). But if "I" is a proper name, it cannot *barely* refer but must be associated with a criterion of re-identification of the object for which, for each of us respectively, it stands. Whereas the application of such a criterion seems to be no essential part of mastery of the use of "I".

(iv)   "I" is misconstrued if assimilated to the demonstrative referential devices, "this" and "that". The difference is that demonstrative pronouns are not, contrary to what is sometimes thought, guaranteed a reference if correctly used; though they are guaranteed something to "latch onto". (p. 54). Whereas "I", if it is thought of as a referential expression at all, will have to be thought of as secure against failure of reference. Simply thinking "I . . . . ." guarantees the presence of its referent to consciousness. If we think of "I" as referential, we must take it that its reference is guaranteed not simply in the sense that the user of "I" must exist — otherwise he would not be using "I"! -- nor even in the sense of guaranteed existence of the object meant by the user, but more: the guarantee is that what the user takes to be the reference of the term is the reference of the term. (p. 56−7). No account of the semantics of "I" treating it as a referential expression is adequate unless it explains our possession of such a guarantee.

(v)   If we have such a guarantee, then the reference of "I" cannot be anything essentially corporeal. For if it were, we can imagine certain special circumstances, e. g. a state of total sensory deprivation, in which we should not have the guarantee. In fact, if "I" is referential, then "nothing but a Cartesian ego will serve" as its reference. (p. 58).

(vi)   If we choose to think of "I" as referential, we owe in any case an account of the *kind* of thing it stands for. But the kind, *Cartesian ego,* is imponderable. No account can be given of what would guarantee continuity of reference through successive uses (by me) of "I" — or guarantee, indeed, that any single use was not *multiply* referential — if "I" is to stand for such a thing. The proper conclusion is therefore that, despite the analogies noted, "I" is not a referential expression at all.

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The difficulties both of the subject matter and her exposition make it hard to be sure how likely it is that Anscombe is right about all this. Nor does she offer very much of a positive account of the semantic rôle of "I" with which to replace the received referential view. What is certain is that anyone who had supposed that the received view is at worst an over-simplification, and that "I" is merely one of a class of "token-reflexive" expressions – along with "you", "this", "here", "now", etc., – which systematic semantic theories should treat in more or less the same way, is set a considerable challenge by this paper.

Quine warns the "more omnivorous" of his readers to expect little of novelty in "the Nature of Natural Knowledge". The paper begins with an emphasis that scientific theorising is a precondition of philosophical scepticism; continues with speculation how the efficacy of induction may be explicable along Darwinian lines; introduces the notion of an observation sentence; develops the conjecture that the best way of understanding the relation between evidence and scientific theory is to investigate how language learning proceeds *via* grasp of observation sentences to grasp of theoretical ones; and concludes with an attempt to face the difficulty, – which we noted above in connection with Føllesdal's essay, – how the underdetermination of theory by the totality of possible observations is a coherent possibility for one who holds that there is no meaning but empirical meaning.

The predominant sense which the paper inspired in this reader at any rate was not so much of *déjà lu* as of questions unanswered. I shall mention just four.

First, an observation sentence, as characterised by Quine, is one whose truth-value depends solely upon, and so may vary with, intersubjectively observable circumstances obtaining at the time of utterance; which circumstances must be such that anyone familiar with the language in question, irrespective of other background knowledge, would be able to appreciate that the relevant observation verifies, or does not verify, the sentence. So – Quine's example – "there goes John's old tutor" is not an observation sentence, whereas "there goes an old man" is. But how clear is this explanation for *operational* purposes? In particular, could a radical interpreter straightforwardly determine whether a sentence was in this category by study of the community's patterns of assent to and dissent from it? Certainly, variations in assent and dissent depending upon current intersubjectively observable circumstances ought to be comparatively straightforward to check; but whether the sentence is thought of as verified *by* the relevant circumstances, and whether, if it is, that it is so is thought of as appreciable by anyone competent in the language or only by those in possession additionally of certain background information, look to be matters which it might prove problematic to determine. (Notice also that Quine's own example – "there goes an old man" – is actually *not* an observation sentence in the explained sense; for appreciation that it is verified by a particular observation requires background knowledge about the rate and manner in which people typically age.)

Second, even if it is right to think of the order of language acquisition as proceeding through grasp of observation sentences – whatever they are – *via* grasp of more or less observational standing sentences – "whose truth-value endures regardless of occasion of utterance" – to grasp of theoretical sentences, the crucial question is not so much in what way the training which people are actually given carries them along that path, but rather: what would any successful training need at successive stages of the path to make plain? Given that someone has grasped, for example, a certain range of observation sentences, what more exactly does he need to grasp in order to understand standing reports of observation of the kind needed for the purposes of scientific theory? Quine allows that the enquiry must have a preliminary speculative phase before we can go on to profit by empirical investigation of a child's actual learning of language. But there seems to me some doubt whether empirical investigation has a part to play at all, whether what is really wanted for Quine's purpose is not rather a *model* of an idealised training which would serve to make plain everything which a trainee would need to have made plain. And to call for such a model is just to call (obliquely) for a philosophical explanation of the content of and relations between the relevant kinds of sentence.

Third, Quine speculates about the learning of the logical constants. "We can easily imagine how the child might learn the truth-functions – negation, conjunction, alternation. Take conjunction: the child notices, by degrees, that the adult affirms "p and q" in only those circumstances where he is disposed, if queried, to assent to "p" and also to "q". But how easy are the other two cases? The child can be expected to notice that the adult affirms "not p" only in circumstances where he is disposed, if queried, not to affirm "p". But the converse won't hold; and the child simply will not observe adults affirming "p or q" only in circumstances when they are either prepared to assent to "p" or prepared to assent to "q". It is in fact no easy matter to give a description of the assent conditions of all logical compounds which *both* analyses those conditions as a function purely of

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the assent conditions of the constituent sentences *and* accords with ordinary usage. (That is how it came about that the Intuitionists' explanations of the logical constants are *revisionary* of classical mathematics.)

Fourth, Quine's response to the problem of the possible distinctness of empirically equivalent theories seems rather thin. "What wants recognising", he claims, "is that a physical theory of radically different form from ours, with nothing even recognisably similar to our quantification or objective reference, might still be empirically equivalent to ours, in the sense of predicting the same episodes of sensory bombardment on the strength of the same past episodes." Perhaps this is a possibility; but it is quite unclear how a Quinean empiricist could be in a position to grant it. For it evidently is not a possibility which we could suppose to be realised by any set of sentences which we understand *before* we understood how to eliminate our devices of quantification and objective reference — which we do not. On the other hand once, if it were possible, we understood how to eliminate them, the correct response for the Quinean would be simply that such a theory would be no more than a terminological variant of our own theory. So the original problem seems to stand.

Quine's second paper, "Mind and Verbal Dispositions", is a vigorous condemnation of "mentalistic semantics" and dualism in the philosophy of mind. Mastery of language is to be described, in the first instance, in terms of dispositions to observable behaviour, in anticipation of — in the second instance — a neurophysiological account of the bases of these dispositions. The same goes for mental states in general. But Quine shies away from endorsement of the so-called identity-theory of the mind, that mental states are states of the body, preferring to describe himself as repudiating the mental; for there is otherwise, he feels, a temptation to indulge in mentalistic explanations, comforting oneself with the belief that one is indirectly speaking of some neural mechanism — if only one knew what. Quine is more generous to animals and linguistically incompetent humans than Davidson, for he proposes to regard mental activity in general as constituted by dispositions to *mostly* verbal behaviour.

Towards the middle of the paper, in the context of the idea that understanding a declarative sentence is knowledge of its truth-conditions, Quine asks in just what behavioural dispositions this knowledge can be taken to consist. No special difficulty arises, it seems, when we are concerned with observation sentences; here knowledge of truth-conditions can be identified with the disposition to respond to querying by assenting to the sentence when and only when its intersubjectively observable condition of truth is realised. But with standing sentences, in contrast, "the stimulating situation at the time of querying them will usually have no bearing on the verdict; and for this reason we cannot identify the understanding of a standing sentence, even approximately, with a disposition to assent or dissent when queried on particular occasions."

Now, one would have thought that most standing sentences, and especially those at the base of the pyramid of empirical science, are associated with relatively clear cut conditions of experimental confirmation and disconfirmation; and that the relevant dispositions would pertain to the experimental realisation of those conditions — (though whether such dispositions could be held to constitute knowledge of a *truth-condition* is quite another matter). But Quine pursues a dramatically different course. "I do not know how", he confesses, "in general in terms of behavioural dispositions to approximate to the notion of understanding at all, when the sentences understood are standing sentences. . . . . . Perhaps the very notion of understanding, as applied to single standing sentences, . . . is simply an untenable notion, notwithstanding our intuitive predilections."

It is presumably Quine's holism, his view that only theoretically integrated large *sets* of standing sentences have determinate conditions of experimental confirmation and disconfirmation, which encourages him to take this line. And he is ready to draw the related conclusion that there is no satisfactory notion of equivalence of meaning for pairs of standing sentences. So far as I am aware, Quine has nowhere else made explicit that he considers it a consequence of his general views that no account can be given of what it is to understand individual theoretical sentences, or standing sentences in general.

The attempt (p. 90), however, to infer indeterminacy of translation for theoretical sentences still seems to be checked by the same old problem. It is one thing to hold that there are no constraints on the translation of a theoretical sentence beyond ensuring an appropriate place for its counterpart in a theory which meets certain conditions as a whole; and quite another, further thing to hold that *incompatible* manuals of translation can furnish distinct ways of meeting that constraint.

Dummett's "What is a Theory of Meaning" is the first of two published papers in answer to this question, the second appearing in Evans and McDowell, eds., *Truth and Meaning*, OUP 1976. Together with its fifteen page appendix, this first paper makes up over one quarter of the present

volume. It is the most difficult piece in the book: the argument is dense, a broad sweep of ground is covered, individual sentences are complex, and signposting and recapitulation are infrequent.

Arguments against the appropriateness of describing our understanding of statements in general in terms of knowledge of conditions of truth are familiar from many of Dummett's writings, and are developed in detail in "What is a Theory of Meaning? (II)." This prior paper is devoted primarily to criticism of Davidson's Tarski-style truth-theoretic conception of the form which a theory of meaning for a natural language should take; but the criticism here in no way depends on Davidson's recourse to truth as the central notion in his blueprint. Rather it bears on the sense of triviality, or — more kindly — of incompleteness, with which Davidson's enterprise, despite its technical sophistication, leaves many philosophers.

In rough outline, the main part of the paper develops and defends the following seven theses:

(i)   A satisfactory theory of meaning for a particular language will be a complete description of what is involved in mastery of that language. To elucidate the principles which would govern the construction of such a theory will be completely to elucidate the concept of meaning from a philosophical point of view; it will be to answer, implicitly or explicitly, all philosophically worth-while questions about meaning.

(ii)   A theory of meaning must be a theory of understanding, a complete description of what is known by anyone who knows the particular language.

(iii)   A theory of meaning ought to do more than state, or otherwise show, what is known by anyone who understands any particular expression of the language; it should, in addition, give an account of what it is to grasp the concepts whose possession is involved in understanding that expression (p. 101). A *modest* theory of meaning completes only the first of these tasks; a *full-blooded* theory of meaning completes them both. Davidson's theory is a modest theory.

(iv)   Davidson's theory can pass, if at all, for a theory of meaning only if speakers of the object-language are thought of as in possession of the information — as knowing the propositions — which the axioms express. In particular, it won't do to think of them as knowing merely that the axioms, i. e. the sentences, express truths. So we are owed an account of what it is to know the propositions expressed by the axioms. Davidson provides none; and the natural adaptation (p. 113) of something which he has suggested as an account of what it is to understand whole sentences of the object-language leads nowhere.

(v)   Davidson's *holism* connects with his conception of a theory of meaning precisely by enabling him to discount the range of questions which a full-blooded theory would seek to answer. For the holist there *is* no specifying what knowledge of the propositions expressed by the individual axioms and T-sentences of a Davidsonian theory consists in. "On such an account there can be no answer to the question, what constitutes a speaker's understanding of any one word or sentence: one can say only that the knowledge of the entire theory of truth issues in an ability to speak the language, and, in particular, in a propensity to recognise sentences of it as true under conditions corresponding, by and large, with those stated by the T-sentences." (Notice that the connection is between holism and the belief that modesty is all we can reasonably ask of a theory of meaning. No connection has been made out, in contrast with the impression Dummett gives — "It is just here that the connection becomes apparent between a theory of meaning that proceeds via a theory of truth and a holistic view of language" —, between holism and the belief that a theory of *truth* is the appropriate form for a modest theory to take.)

(vi)   Holism is essentially unable to give any account of the distinction between factual dis-agreement and discrepancies in understanding among speakers. Therefore it can actually give no content to the notion of a mistake, which it invokes — or which Davidson at any rate invokes — to account for the lack of fit which there will inevitably be between some judgements by speakers of the object-language and the prescriptions of the best theory of truth which we can design for that language.

(vii)   A modest theory of meaning is either no better than a translation manual — a theory which simply pairs expressions in two studied languages —, or it must be interpreted holistically, in which case its claim to give a systematic account of mastery of a language is inadmissible since holism cannot allow that such an account is possible.

In the Appendix Dummett revises his opinion of Davidson's conception. Davidson's holism is now seen not as involving a repudiation of the need for full-bloodedness but as itself a full-blooded account. What is involved is, in essentials, the astonishing claim that understanding a language involves knowledge of how to attribute references and extensions to the singular terms and functional expressions in some very large, privileged class of its sentences in such a way as to have

the largest possible number of them come out true. So understanding a particular singular term or predicate, for instance, would be a matter of knowing what reference, or extension, it received under this optimum total assignment. The axioms of a theory of truth for the language would then aim at spelling out what this assignment was in the case of each primitive expression of the language. As an account of speaker's knowledge, Dummett, unsurprisingly, finds this picture too fantastic to swallow.

If the reader shares my experience, he will be perplexed by at least two phases of Dummett's essay. First, there is the matter of what exactly full-bloodedness would involve. Arguing that a modest theory is no better than a translation manual, Dummett is nevertheless ready to grant (p. 103) a point on which Davidsonians place great stress (see e. g. Evans' and McDowell's Introduction, op. cit.); viz. knowledge of the propositions expressed in a correct translation manual is compatible with complete ignorance of the languages it correlates, while knowledge of the propositions expressed by a correct modest theory of meaning is not compatible with complete ignorance of its object-language. But, Dummett writes, "a translation manual presupposes a mastery of some other one language – that into which the translation is made – if we are to derive from it an understanding of the translated language; but a modest theory of meaning presupposes a mastery of *some*, though unspecified, language if we are to derive from it an understanding of the object-language." So a modest theory makes exactly the same kind of presupposition as a translation manual, only less specifically. Whereas the significant contrast is rather between theories which make such a presupposition and "those which (like full-blooded theories of meaning) involve no such presupposition at all."

But now, what form would a theory take which was superior both to translation manuals and to modest theories of meaning in this respect? It would have, it appears, to be a theory such that one could derive from it an understanding of the object-language without requiring yet to be a master of any language. So by what medium are the contents of the theory supposed to be communicated?

There are at least two different kinds of thing which we could reasonably call explaining (p. 101) "what it is to have the concepts expressible by means of that language", – which full-blooded theories essentially do explain. The first is simply to explain the concepts, i. e. give what we should ordinarily take to be an explanation adequate for the purpose of *introducing* someone to those concepts. This is, as we just saw, what Dummett seems to have in mind at least part of the time. The second would be: to characterise what differentiated possession of each particular one of the relevant concepts from the lack of it, to characterise how someone's performance would distinctively manifest his possession of the concept. And this, or something like it, is what Dummett seems to have in mind when he later calls for an account of what knowledge of the propositions expressed by Davidsonian axioms is supposed to consist in. It seems fairly evident that while there may be some sort of internal relations between the paradigm form of introductory explanation of a certain concept and a satisfactory account of how the success of the explanation might be manifest in a trainee's behaviour, giving an account of what distinctively manifests a certain piece of knowledge need not in general amount to doing or saying anything which could serve to equip someone with that piece of knowledge.

Now, it seems to me, the kind of full-bloodedness which it makes sense to aspire to would involve doing the second of these things, rather than the first. Neither a translation manual nor a modest theory does either; so in that respect one is no better than the other. Nevertheless, if it is right to think of mastery of a language as (implicit) knowledge of a set of propositions, then the task of characterising for a particular language just which propositions these are is prior to the task of explaining what knowledge of them consists in. And the fact is that a Davidsonian theory will have a claim to have fulfilled the first of these tasks which no translation manual could match.

The second perplexing phase of the paper concerns the claimed link between Davidson's holism and his modesty. The holist (of the paper, not the Appendix) claims that there is no such thing as behaviour which distinctively manifests an understanding of any particular sentence, still less of any particular sub-sentential expression. Therefore the demand for full-bloodedness is misconceived, and modesty is all we have the right to ask for. But surely this cannot ever have been Davidson's thinking. For was not his blueprint conceived in order to do justice to our capacity to understand utterances of types which we have never heard before? Davidson's kind of theory is supposed to provide a detailed model of how recognition of the meanings of novel utterances is possible for us. Since the answer, crudely, is: by understanding the constituent words and the way in which they are strung together, it appears that anyone who takes the question seriously *has* to think of speakers of the object-language as knowing the propositions which the axioms and theorems (in this case, the T-sentences) of its theory of meaning share. In other words, it is quite unclear why Davidson should have thought a theory of meaning should take the kind of shape which he commends unless

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he thought it legitimate to think of speakers of the object-language as in possession of the information codified by its axioms. And if that is legitimate, the question is how, for a holist, it can be any *explanation* of speakers' capacity to understand a novel utterance that they know that which is stated by an appropriate theory about the constituent expressions and mode of construction, — when it is then denied that this knowledge can be manifest in anything less than mastery of the whole language? It is as if to the question, how do speakers understand novel utterances, we had simply replied: by understanding the language in which they are framed!

Holism of this kind, then, so far from providing a motive for modesty, seems to undercut the whole Davidsonian enterprise! The puzzling thing is that this is exactly the conclusion which Dummett (disjunctively) comes to in the second horn of his dilemma (vii) above. But then what becomes of the exegetical insight, (v)?

Perhaps it was an awareness of this difficulty which in part prompted the revised assessment of Davidson's position in the Appendix. However the 'full-blooded' holism there attributed to Davidson does not appear to be full-blooded in either of the senses distinguished above. We can no longer regard Davidson's account, Dummett says (p. 130), "as lacking an account of a speaker's grasp of the senses of those words. On the contrary, what a speaker implicitly knows is that reference is determined in this holistic manner . . . For instance what a speaker knows when he knows that "Oxford" denotes Oxford is, on this account, that "Oxford" denotes that object which is assigned to the name "Oxford" under the preferred total assignment to the names and predicates of English." Thus the notion of full-bloodedness which Dummett now seems to have in play is that, to qualify as full-blooded, a theory must be an account of the sense — of how the reference is determined — of every expression in the language. But the sort of shape taken in the quoted passage by the account offered by full-blooded holism for "Oxford" would be useless both as an explanation of what "Oxford" actually denotes and as an explanation of how knowledge that "Oxford" denotes Oxford might be manifested. And it is actually unclear whether the holist of the Appendix would be prepared to countenance the latter question, or what answer he might give.

Geach's "Names and Identity" proposes and defends two main claims. The first is that, *contra* Ramsey and ' followers of Montague", there is an absolute distinction between names and predicates; namely, predicates are, whereas names are not, negatable. That is, whereas we can coherently treat 'not F . . .' as an expression with the same syntax as 'F . . .', to do the same with 'not a' and 'a' will actually generate contradictions. (p. 144 − 6).

Geach's argument to this conclusion makes the assumption that the result of replacing 'a' in 'Fa' by its supposed negation, 'not a', will be something contradicting 'Fa'. But since the parallel assumption holds for 'F . . .', to dispute Geach's assumption would be already to introduce an asymmetry between names and predicates in point of negation.

The force of Geach's argument, assuming it is sound, can be seen as follows. Consider this schema:

(B) ($\exists$ C) (A): B can be concatenated with A to make a true sentence, if and only if C cannot be concatenated with A to make a true sentence.

The schema seems valid if we restrict the range of 'A' to names, or singular terms in general, and those of 'B' and 'C' to predicates — for we can always take C to be the negation of B; but invalid if we restrict the range of 'A' to predicates, and those of 'B' and 'C' to singular terms — since there seems no reason why there should correspond to an arbitrary name, B, a name, C, such that, irrespective of our choice of predicate, A, 'BA' is true if and only if 'CA' is false.

Now if throughout Geach's argument on p. 144 of *Mind and Language,* the reader substitutes 'b' for '$\sim a$', he will observe that what the argument essentially establishes is that the above schema is *necessarily* false under the second interpretation of the range of its variables; whereas our intuitive impression was merely that there was no reason why it should hold true.

The intuitive asymmetry with respect to negation is familiar from Strawson's work (*Subject and Predicate in Logic and Grammar,* Methuen 1974), and is developed in Dummett's *Frege: Philosophy of Language,* ch. 4 (Duckworth 1973). So it is a little surprising to find Geach writing as though no-one else had ever thought about the point. And I remain unsure exactly how to elicit from it a practical criterion of whether an expression is a name.

Geach's second principal contention is that Frege's notion of a name is at once too broad — since it lets in definite descriptions — and too narrow — since it excludes words for genera and species. In fact he proceeds to reject altogether the distinction of syntactic category customarily drawn between proper and common names — "a position to which I have never firmly committed myself before". 'Horse', a common name of individual horses, may also be viewed as a proper name of the

kind of animal horses are — where the last phrase is *not* to be thought of as denoting any kind of abstract entity. (p. 156).

Geach's principal arguments for drawing this unfashionable conclusion seem to be that common names are both substitutable *salva congruitate* for proper names — provided we are not so naive as to think that the need to prefix a common name with an article in most such substitutions in English indicates anything very much — and are associated with criteria of identity. (pp. 154-5). For example, "there is one sense of 'the same animal' in which whenever we rightly call something by the name 'Bucephalus', we are naming the same animal; there is another sense of 'the same animal' in which whenever we rightly call something by the name 'horse', we are speaking of the same animal." The equivalence relation, 'is the same animal as', which holds between any pair of horses is for Geach, the criterion of identity associated with 'horse'.

If Geach is right about this, then, as he acknowledges at the end of the paper, it appears that a sentence like "Horse died at 30 years of age", and so also "Horse did not die at 30 years of age", has to be regarded as possessing determinate truth conditions. (For both can be made to result from the substitution of a common name for a proper name in sentences with determinate truth-conditions.) But what exactly are the truth-conditions of such a contradictory pair? Geach does not know what to suggest; and if the reader finds himself in the same situation, he may be tempted to think that there is more to the syntactic division between proper and common names than Geach allows, and that there must therefore be something the matter with Geach's arguments for eschewing it.

Subsidiary themes in the paper are the utility, indeed indispensability of Frege's notions of argument and function in the analysis of propositions; a well-argued rejection of Dummett's notion of a simple, as contrasted with a complex, predicate (see Dummett, *Frege* ch. 2); and criticism of Dummett's treatment of the senses of predicates as a species of Fregean object. (*Frege*, ch. 9).

The eminence of the contributors lays this volume open to evaluation by the most demanding standards. We have to ask not, do the papers by and large contain interesting ideas?, but — at the least — do they carry our understanding of their subjects forward?, or, would it be likely to be to the serious detriment of anyone interested in those subjects not to acquaint himself with these papers? It seems to me that only in the case of Anscombe's and Dummett's papers is it clear that the correct answer to the latter question is 'yes'.

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The complexity of philosophical linguistics presents many difficulties for anyone who tries to write an introductory text. One danger lies in the daunting amount of material which must be covered. Another lies in the sophistication of the conceptual structure employed. *Philosophical Linguistics* does not entirely escape from either danger, but neither does it succumb to them.

Three tactics for overcoming these difficulties stand out: the reading material is covered as a natural development from the main themes of the book; the authors do not fail to signpost their own recommended positions with regard to the problems encountered; 'mini-systems' of formal semantics and pragmatics are constructed to display the form that such theories might take.

In the two chapters of part one, *Semantics* and *Pragmatics,* Kasher and Lappin outline those pursuits within philosophy of language which bear most directly on linguistic theories. Chapter 1 deals with the general question of the relation of language to the world, with particular reference to the construction of formal semantic theory. The reader is acquainted with the tradition in philosophy which is built out of the work of such philosophers as Frege, Russell, Wittgenstein, Carnap, Tarski, Davidson and others. The authors adopt a Carnap-Davidson line with respect to the analysis of meaning relations: the meaning of sentences is to be explicated in terms of truth-conditions; Tarskian recursive definition of 'truth-in-L' is amended to include tacit reference to ideal speakers and standard speech contexts.

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