
This volume contains thirteen papers (and nine commentaries) presented at the Second Jerusalem Philosophical Encounter in April, 1976. Originally planned to celebrate the 60th birthday of Yehoshua Bar-Hillel, it became a commemoration upon his sudden death.

It is out of the question to devote as much space to each contribution as it deserves. Instead, we will treat at length the three contributions that seem philosophically most important and then discuss more briefly five others. Those we will not take up are generally narrower in focus and include: E. Zemach, ‘Awareness of Objects’; J. Hintikka and L. Carlson, ‘Conditionals. Generic Quantifiers and Other Applications of Subgames’; H. Schnelle, ‘Circumstance Sentences; A. Margalit, ‘Open Texture’; and P. Strawson, ‘May Bes and Might Have Beens’.

*Putnam.* ‘Reference and Understanding’ by Putnam stirs up a stew of issues. It is not always easy to pick out which issue is being addressed. Ostensibly Putnam’s point is to expose the distance between “the theory of language understanding and the theory of reference and truth” (p. 199), but his pet worry with realism soon bubbles up to make one wonder just what’s cooking.

Putnam accepts the view that “understanding a language consists in being able to use it (or to translate it into a language one can use)” (p. 199). As for what that consists in, Putnam endorses the admittedly oversimplified model, due to Carnap and Reichenbach, of the speaker/hearer possessed of an inductive logic, a deductive logic, a preference ordering, and a rule of action. No mention is made of how such speakers/hearers’ utterances of sentences and acceptance of sentences others utter provides the basis for a theory of understanding a language. Presumably, “in even such a terribly oversimplified model, speech will affect behavior in a rich variety of ways” (p. 200), but until we are told something about the relationships between
sentences and the psychological states their utterance expresses, we are in no position to see why the suggested model is a model of language use, much less of language understanding. Part of the problem here, the problem of giving a detailed model of how the relationships between linguistic and nonlinguistic behavior are mediated by psychological states, is that much of language use is nonlateral and/or indirect (Bach and Harnish, 1979, Chs 4, 9, 10) and that if the admitted oversimplification includes the assumption that all use is literal, the notion of literal use must be explained without recourse to the dreaded notion of meaning (which Putnam describes, with more pith than perspicuity, as "a coarse grid laid over use" (p. 200)).

At any rate, Putnam is perfectly clear about what he thinks understanding a language is not. He rejects the view that "to understand a statement is to be explained as knowing its truth conditions" (p. 210), and his reason for rejecting it makes clear why he believes the theory of understanding and the theory of truth to be so far apart and why the issue of realism is relevant, for

If truth is correspondence to reality, it would seem as if knowledge of what the correspondence is is presupposed by knowledge that such and such a statement stands in the relation in question to anything or does not... But in what could this knowledge—which does not consist in the acceptance of any statements, because it is prior to the understanding of all statements—consist? (p. 210)

According to Putnam, the role of the theory of truth for a language is not to specify what competent speaker/hearers associate with sentences but to help explain "the contribution of linguistic behavior to the success of total behavior" (p. 203). With this in mind Putnam sketches a realist account of "epistemic reliability" (pp. 204–8) and argues that, being causal, this account works only from a realist standpoint (pp. 208–10). His main reason for insisting that a reliability theory be realist is not that reliability is "a fact of nature" (p. 204) but that a non-realist approach cannot do justice to the behavior of modal statements. Here he deals ingeniously with his favorite version of such an approach, Dummett's, and shows that unlike "is true," "is warrantedly assertible" cannot play the role of T-predicate in a reliability theory.

Three heady problems rise from this cauldron of semantics, metaphysics, and epistemology, one about truth, one about realism, and one about reliability. They may be taken as objections but we would be content if they be taken as requests for (considerably) further explanation and argumentation.

First, Putnam offers no account of why, on the view that understanding a statement is knowing its truth conditions and on which truth is correspondence, understanding requires knowing "what the correspondence is" or even believing that truth is correspondence. Why should language users, in accepting some statements and rejecting others, need to have a theory of truth or even the rudiments of one? Perhaps a redundancy theory of truth is inadequate, but maybe language users, as opposed to theorists, need nothing more (if even that). The real problem with the view that understanding a statement is knowing its truth conditions is that it gets the theorist no further than it would get the language user if it were correct. If understanding "Snow is white" consisted in knowing that it is true if snow is white, one would still have to understand that. It is not obvious that linguistic understanding requires metalinguistic knowledge, specifically knowledge of truth conditions, but even if does, Putnam has not shown that speakers must, if the correspondence theory of truth is correct, have that or any theory of truth. Rather, so it seems, one understands "Snow is white" iff one knows that it is true iff snow is white—and knows what it is for snow to be white, which is not a linguistic matter at all.

This is why, secondly, we wonder whether the issue of realism is relevant where Putnam takes it to be. It is one thing to understand "Snow is white" and another to know what it is. metaphysically speaking, for snow to be white. For at that level what is at issue is not whether "Snow is white" represents the state of affairs that snow is white rather than that Archimedes drowned in a bathtub, but rather what sort of a thing is any such state of affairs and its constituents. It is completely mysterious to us why anyone should think that questions of semantics should be affected by the metaphysical issue of realism vs. idealism. Putnam seems, both here (esp. pp 208–9) and elsewhere, to equate this issue with the (meta-) semantic issue of whether the appropriate T-predicate for Tarskian semantics should be "is true" or "is warrantedly assertible" (or something of the sort). But surely a metaphysical idealist could gladly accept "is true" as his T-predicate despite his denial of realism. His version of what it is for snow to be white could ipso facto be his version of what it is for "Snow is white" to be true. Also, it seems that Putnam does not
realize that if the metaphysical issue of realism vs. idealism has any semantic bearing at all, it bears not just on the meaning of "is true" but also on that of "snow" and of "is white."

Third, there is a problem about what the theory of epistemic reliability, which Putnam thinks the notions of truth, reference, and correspondence are good for (rather than for the theory of language understanding), is a theory of. As we have seen, Putnam regards reliability theory as what is needed to help explain the contribution of linguistic behavior to the success of total behavior. But when he speaks of a speaker as being reliable, does he mean as a believer or as a (true) sentence producer? Unless we make the naive (and possibly question-begging) assumption that speaker/hearers always speak literally and sincerely, the two sorts of reliability come apart. Part of the trouble here is that Putnam seems to equate belief with accepting statements and statements with sentences. However, maybe Putnam includes cognitive activity with linguistic behavior and holds that spoken language is the language of thought and that statements don't have to be made to be accepted. Ignoring the not insignificant problems of ambiguity and indexicality which arise here (and which are conveniently neglected on p.205 despite their obviousness there), we are at least entitled to some remarks about how belief is connected to accepting a statement.

This last problem is no mere quibble. Even if, in the end, we were to accept the view of Harman's (1973, Ch. 4) that a regimented public language is the language of thought, we should not start with the assumption that the theory of content for thought is a trivial extension of the theory of content for public language. If anything, "the theory of content for public language reduces completely to the theory of content for thought" (Schiffer, forthcoming) even if thought is in public language. That is, as Schiffer puts it, the semantic reduces to the psychological or, if you will, linguistic semantics reduces to psychosemantics. Sentences, sequences of marks and sounds, have contents only because the psychological states they are used to express have contents. The irony here is that Putnam's idea of separating the theory of truth from the theory of content and incorporating it instead into reliability theory is precisely what Schiffer and Field (1978) have proposed. By tackling the theory of mental contents head on (after in effect substituting "cognitive" for "linguistic") they have followed Putnam to a tee. The notions of truth and

truth conditions have no place, according to them, in the theory of internal functional role (i.e., of contents from the standpoint of methodological solipsism in the sense of Fodor (1980)). Instead, these notions enter in from the standpoint of reliability theory. Of course, now we are talking about that theory's help in explaining not the contribution of linguistic behavior to the success of total behavior but rather the contribution of cognitive activity to the success of behavior, linguistic and otherwise.

A brief look at Dummett's comments on Putnam may help clarify ours. As with Putnam, Dummett supposes that realism "consists in an adherence to a truth-conditional semantics" (p. 218). He agrees with Putnam further that if semantics is equated with theory of meaning and that with theory of language understanding, then there are the sorts of "acute difficulties" that Putnam mentions with truth-conditional semantics, specifically with the supposition that understanding a sentence consists in knowing its truth condition. For Dummett it is not even clear what that means. So he applauds Putnam's "powerful and ... original defense" (p. 219) of realism, which displaces truth-conditional semantics from the theory of understanding to reliability theory, not that he accepts Putnam's defense. Instead, he clarifies the non-realist semantics he has long endorsed by explaining its underlying metaphysical commitments. These are not idealist in any traditional sense. Rather, Dummett supposes that there is a distinction (perhaps not clearcut) between "hard facts" and "soft facts."

Soft facts are ones stated by forms of sentence the condition for whose truth – or for a justified assertion of them – is either not fully determinate or else depends in part upon facts about ourselves ... that do not enter explicitly into the statement of fact. (p. 222)

Moreover, there are soft facts and they are not reducible to hard facts. Dummett cites Kripke's examples of belief ascriptions, statements about causal relations, and statements about what someone is free to do, and he includes the phenomenon of open texture discussed by Margalit as an aspect of softness.

Dummett's clear and concise sketch of the metaphysical underpinnings of his non-realist semantics sets the stage for an objection to Putnam, which we consider shortly. First we should reiterate our insistence on the separation of semantics from metaphysics, for Dummett's notion of soft facts makes it seem as though there is an
We accept Putnam's point as an ad hominem reply to Dummett but want to insist on something stronger. Semantic indeterminacy does not require nonrealist semantics, i.e., an alternative to the bivalent "is true" as a T-predicate, not just for Putnam's reason but for the following. Any conception of a public language is an abstraction from the notion of individual idiolects, and, supposing as we do that the theory of idiolects depends on the theory of individual psychologies, an idiolect is an abstraction from individual psychology. Linguistic meaning is a coarse grid laid over idiolectical meaning, and idiolectical meaning is a coarse grid laid over mental content. Accordingly, we suggest that the real locus of the dispute between Putnam and Dummett is not at the level of language but at the level of the psychological.

Kripke. Kripke's 'A Puzzle about Belief' is itself a puzzle. But before presenting our puzzle first let us explain what he takes his to be.

After fifteen pages of preliminaries in which he likens his view of names to Mill's (in contrast to Frege's), reminds us "that codeignative proper names are not interchangeable in belief contexts" (p. 243), and introduces "the disquotational principle presupposed here, connecting sincere assent and belief" (p. 248), the puzzle appears. Monolingual Pierre, a normal (but not entirely chauvinistic) French speaker sincerely utters, on the basis of what he has heard of a certain famous city across the channel, "Londres est jolie," from which we naturally conclude that

(1) Pierre believes that London is pretty.

Later, for reasons that need not concern us, he moves to England, in particular to a slum of London, where he learns English by mixing with his uneducated neighbors (i.e., they know no French). Part of what he learns is that the city he lives in is called "London" and, judging from his surroundings, he sincerely utters, "London is not pretty." Should we conclude that

(2) Pierre believes that London is not pretty.

It seems that we should, and for much the same reason (using the disquotational principle) that we supposed Pierre to believe that London is pretty. But Pierre "has no inclination to change his mind for a moment about the city he still calls 'Londres'" (p. 253), so (1) is
still true. If both (1) and (2) are true of Pierre at the same time, then we appear to have ascribed contradictory beliefs to him.

But it is clear that Pierre, as long as he is unaware that the cities he calls ‘London’ and ‘Londres’ are one and the same, is in no position to see, by logic alone, that at least one of his beliefs must be false. He lacks information, not logical acumen. He cannot be convicted of inconsistency. (p. 257)

The puzzle is how to characterize Pierre’s ignorance. Presumably he is ignorant not merely of (1) – Kripke does not mention this piece of ignorance – but on the other hand, Pierre is not ignorant that London is London and that Londres is Londres. The puzzle is how, on Kripke’s ‘sense-less’ theory of names, to explain Pierre’s ignorance that Londres is London. (Kripke embellishes the puzzle by altering the example in a way that seems to land us, not just Pierre, in a contradiction. This time, having “no right to any opinion as to the pulchritude of the whole city” (p. 258), Pierre merely withholds assent to “London is pretty,” from which we conclude,

(2') Pierre does not believe that London is pretty.

If we, demonstrating our logical acumen, conclude from this together with (1) that,

(3) Pierre believes and does not believe that London is pretty.
then we seem to have fallen into a contradiction. To avoid this we had better find some better answer than “Both” to the question, “Does Pierre, or does he not, believe that London is pretty? I know of no answer to this question that seems satisfactory” (p. 259). We will not pursue the embellished version of Kripke’s puzzle, for it would seem that the principle of total evidence shows that (2') should not have been accepted in the first place.)

Although Kripke’s first illustration of his puzzle is the London/Londres example, the puzzle does not depend on the believer’s having two languages. He gives the case of one Peter who, having learned “Paderewski” as the name of a famous pianist, assents to “Paderewski had musical talent”. Later Peter learns “Paderewski” as the name of a Polish statesman and, skeptical of the musical abilities of politicians, assents to “Paderewski had no musical talent”, mistakenly thinking that this time “Paderewski” does not denote the pianist. Should we infer not only that Peter believes that Paderewski had musical talent but also that Peter believes that Paderewski had no musical talent? If so, we must characterize Peter’s beliefs in such a way that Peter is innocent of inconsistency, being merely the victim of ignorance. We need to find a way to express what Peter is ignorant of; surely he is not ignorant that Paderewski = Paderewski!

The obvious solution would seem to be that what Peter is ignorant of is that Paderewski the statesman = Paderewski the pianist. The trouble is, or seems to Kripke to be, that no such solution is available because proper names are not semantically equivalent to definite descriptions, at least according to Kripke’s well-known view of names. If they were, then “Paderewski” would be ambiguous for Peter, with one sense given by the description “the statesman called ‘Paderewski’” and the other by “the pianist called ‘Paderewski’.” With these as the two senses the obvious solution just given would work straightforwardly.

This is not the place to challenge Kripke’s approach to names, although a case can be made for the ‘Nominal Description Theory,’ first mentioned by Russell (1919, pp. 174–5) and recently defended by Loar (1976, pp. 370–3) and more fully by Bach (1981b), according to which a name N is semantically equivalent to the definite, though usually not complete (i.e., whenever N has more than one bearer), description “the bearer of N.” Instead let us play along with Kripke’s approach to names and consider the consequences for belief contexts involving names. In particular, can the obvious solution mentioned above be reconciled with the claim that names occur rigidly even in belief contexts?

As Schiffer (1977) has pointed out, a belief sentence like “Peter believes that Paderewski had musical talent” does not fully specify the content of the belief ascribed. What is missing is a specification of the “mode of presentation” (Schiffer borrows the phrase from Frege) under which Paderewski is being thought of. Accordingly, there are distinct modes of presentation, expressed by different definite descriptions, for Peter’s two beliefs about Paderewski. Now Kripke does seem to consider this obvious solution in discussing (pp. 259–62) the view that a believer associates “uniquely identifying properties” with the names he uses but these, according to Kripke, “description theorists have regarded as ‘defining’ proper names” (p. 261). But sophisticated description theorists have not done this. Schiffer nowhere says that the meaning of a name is the individual concept (as
expressed by a definite description) under which one thinks of what
the name names, and Loar (1976) is explicit on this point. As Schiffer,
Loar, and Frege himself make abundantly clear, the linguistic mean-
ing of a sentence containing a proper name may not fully determine
the content of the belief expressed in its utterance. In his theory of
belief Frege was (usually) careful not to confuse “sense” in the sense of
mode of presentation with linguistic meaning.

The ready availability of the obvious solution to Kripke's puzzle
makes puzzling his puzzlement about it. Although Kripke presented
the puzzle in 1976, the published version in the book under review
did not appear until 1979. Not only did Schiffer’s article appear in plenty of
time (1977) for Kripke to have considered it, it appeared in a volume to
which Kripke himself contributed. Thus our puzzle.

We do not have a solution to our puzzle, but there is more to
Kripke's puzzle and his commentary on it than we have mentioned
so far. We will discuss several of his remarks that do not presuppose
the confabulation of linguistic meaning with sense. For Kripke does
address the view that a unique identifying description associated with
a name must be included in a complete specification, in which a
name is used to refer to the individual the belief is about, of the
content of the belief. But we must first be clear about what he is
denying when he says that people often “do not assign ‘senses’ of the
usual type to the names that uniquely identify the referent” (p. 246). If I
state that

(4)  Paul believes that Kistiakowsky discovered silly putty.

I, not Paul, am using “Kistiakowsky” to refer to the famous chemist
Kistiakowsky. Even if Paul thinks of Kistiakowsky under that name,
he is not referring to the chemist under that name. Of course, as Loar
(1972) has pointed out, referring expressions in belief contexts can be
used both to refer and to express how the ascriber thinks of the
individual his belief is about. But Loar reminds us also that such
expressions can be used merely to express an element of the content
of the belief. It is a philosophical commonplace that when an expres-
sion is used (in a belief context) merely to refer it occurs trans-
parently, and when used to express an element of content it occurs
opaquely, although which way it occurs generally goes unmarked in
the ascription. That is why Kripke’s puzzle, if it is a puzzle, does not
have determinate consequences for what he calls our general practice
of reporting beliefs” (p. 239) and why he exaggerates in saying that it
marks “an area where our normal apparatus for the ascription of
belief is placed under the greatest strain and may even break down”
(p. 270). For our normal practice is too ill-defined for niceties like
Kripke’s puzzle to affect. It is questionable whether the distinction
between transparent and opaque occurrences of referring expressions
in belief contexts is semantic and not merely pragmatic, and it seems
that our “normal apparatus” for ascribing beliefs underdetermines
our “general practice” of reporting beliefs. That is why the obvious
solution to Kripke’s puzzle would require, if incorporated into our
normal practice of ascribing beliefs, complete specifications of belief
contents. But of course our ordinary practice does not demand that.
For this reason it is important for philosophers not to read any
commonsense psychology into our ordinary practice of ascribing
beliefs. Unfortunately, most philosophers who write on belief are
guilty of this. Such reading in of commonsense psychology is abetted
by the practice of referring to the that-clauses in belief sentences as
content clauses, as if all referring expressions occurring in that-
clauses are used to express elements of content. And, as we have
seen, even when they are so used, the entire content may not be
expressed.

Kripke’s strongest claim in connection with his puzzle does not
depend on confusing the linguistic meaning of a proper name with the
sense associated with it in a person’s thought of an individual bearing
the name. For Kripke suggests (p. 260) that “the puzzle can arise
even if Pierre associates exactly the same identifying properties with
both names” (“Londres” and “London”). The trouble with the case
that he makes for this claim is that he does not include the property of
bearing a certain name among the properties in question. Only thus
can Kripke pretend that Pierre could associate “exactly the same
uniquely identifying properties” like being “the largest city in (and
capital of) England” and containing “Buckingham Palace, the residence
of the Queen of England” (p. 260) with the city he thinks of as “London
and the one he thinks of as “Londres.” Clearly, being called “London
in England is not the same property as that of being called “Londres” in
France.

It might be noted that Kripke does consider the view that “being
called N” is part of the meaning of the name N, but his argument
against this stronger view is invalid. He observes (p. 274) that one
might know that "Alienists are called 'alienists'" is true without knowing what "alienists means and without knowing "Psychiatrists are called 'alienists'" is true also. Kripke rightly remarks, "None of this goes to show that 'alienists' and 'psychiatrists' are not synonymous, or that 'alienists' has being called 'alienists' as part of its meaning when 'psychiatrists' does not" (p. 274). But then he concludes, "Similarly for 'Cicero' and 'Tully.' There is no more reason to suppose that being so-called is part of the meaning of a name than of any other word." But there is reason: whereas a psychiatrist is called an "alienist" because he is an alienist, not because psychiatrists are called "alienists," an individual, say Freud, is called "Freud" because he bears that name.

Searle. In 'Intentionality and the Use of Language' Searle renders the valuable philosophical service of distinguishing "intentionality-with-a-t" from "intentionality-with-an-s." Searle does not say this, but it seems to us that a great many philosophers either use the terms interchangeably or use one as if it had the meaning of both. Although Searle formulates the difference very clearly, we believe that much of what he says about intentionality and intensional needs qualification or elaboration. Also, what Searle presents as important analogies between intentional states and linguistic acts strike us as theoretically trivial.

Searle does not dwell on intentionality-with-an-s. He mentions the existential generalization and substitutability tests for extensionality, and asserts that a sentence is intensional if it fails one of these tests, perhaps with an eye to the possibility that a sentence might pass one test but not the other. However, his extension of the notion of intentionality-with-an-s from sentences to states requires more explanation than one sentence in a note (Note 5, p. 197).

Searle is right to claim that "there is nothing inherently intensional about intentionality" because, in general, "intentional mental states are not about representations, they are representations" (p. 187). Of course, just as a report about a belief is intensional, so is a belief about a belief — both are representations of representations — but these are special cases. Searle suggests most plausibly that because sentences about intention states are often intention-with-an-s, "discussions of intentionality suffer from a confusion of levels which is somewhat analogous to a use-mention confusion" (p. 187), but his observation that the de re/de dicto distinction applies to belief reports does not justify his rejection (p. 197) of that distinction as applied to belief itself (see Bach, 1981a). Searle encapsulates his "picture of intentional states" as follows:

Every intentional state consists of a certain representative content in a certain mode. The same content can occur in different modes, as for example, when I believe it will rain, hope it will rain, want it to rain, etc. It is necessary to distinguish the representative content from the intentional object. An intentional state will have an intentional object if and only if its representative content is satisfied by an object or state of affairs. (p. 188).

As Searle admits (after doing it repeatedly), it is "misleading" to speak of "intentional objects if they could be either particulars or states of affairs" (p. 189), not just for the reason he gives, that states of affairs are not objects, but also because his notion of satisfaction is thereby blurred. For example, a false belief about a real object has a particular but not a state of affairs as intentional object. Still, Searle is absolutely right to point out that the so-called propositional attitudes do not have propositions but states of affairs as their objects (unless these happen to be propositions). Thus, contrary to common practice, propositions should be described as contents rather than objects of belief, desire, etc.

Searle's initial standard of clarity lapses into obscurity when he tries to offer an account of intentionality: "A mental state is intensional if and only if the specification of the content of that mental state requires the specification of some object or state of affairs which is not identical with that mental state" (p. 182). Avoiding the nineteenth century mistake of ascribing "peculiar ontological status" (i.e., intensional inexistence) to intentional objects, Searle remarks that the intentional object of a mental state is just the actual object or state of affairs represented by an intentional state. If there is no such object or state of affairs then the intentional state does not have an intentional object though it does still contain a representation (p. 185).

Unfortunately, he helps himself to another nineteenth century notion when he says, "Any representation is internally related to its object in the sense that it could not be that representation if it did not have that object" (p. 184). Such a claim is either false or at best stipulative. Specifying the intentional object of a representation is not required to specify it, unless one stipulates that this part of what it is to specify a representation. There are all sorts of ways to specify representations that do not specify their intentional objects, e.g., "Goldbach's Con-
jecture,” “Picasso’s last painting,” and “Reagan’s greatest fear.” We can individuate representations differently for different purposes. From the standpoint of cognitive psychology, we may want to individuate by representational content (and functional relations) whereas from the standpoint of truth-theory what matters is the state of affairs satisfying the representation. In any event, Searle’s test for intentionality in terms of the essentialness to a state of its object (which need not exist) gets us nowhere.

Searle does not really offer a theory of representative content and intentional objects (sometimes described as “conditions of satisfaction”), although he does remark that the distinction “is parallel to Frege’s distinction between sense and reference” (p. 185). A theory of intentionality would tell us which mental states have representative content and the conditions under which two different intentional states have the same representative content. Moreover, it would tell us what relation must obtain between a representative content and an intentional object that makes the content “directed at” the object. Searle does not consider the goal (it is not clear whether he would endorse it) of giving an account of representational content independent of intentional objects. Since, as he himself acknowledges, the existence of an intentional object is not necessary for an intentional state to have representational content, it should be possible to investigate the features of mental states that determine their representative contents without mentioning intentional objects. (Searle neglects the familiar distinction between representation and representation of.)

Searle is concerned next to show that “the philosophy of language is a branch of the philosophy of mind” (p. 190) rather than the other way round, i.e., that the intentionality of linguistic acts derives from the intentionality of the psychological states they are performed to express. Searle allows that “given the system of representation provided by language, we can have vastly richer and more complex intentional states than we could without language. But language does not create intentionality” (p. 190). In support of this eminently plausible view, Searle offers three analogies between intentional states and speech acts. First, just as a speech act has illocutionary force and propositional content, so an intentional state has a psychological mode and a representational content. The notion of satisfaction applies to both, to statements and to beliefs, to orders and to desires. Second, both speech acts and intentional states have direc-

tions of fit. Just as an assertion is false if its content does not fit the world, so is a belief. And just as an order is not complied with if the world does not fit its content, so a desire can be unfulfilled. Underlying the first two connections is the third, that “a speech act with a propositional content is an expression of the corresponding intentional state, and the propositional content of the speech act is identical with the representational content of the intentional state” (pp. 191–2).

What do these three analogies amount to? The notion of direction of fit, despite its intuitive appeal, does no theoretical work, and the first and third analogies flow trivially from the fact that “the performance of the act is eo ipso an expression of the corresponding intentional state and the propositional content of act and state are identical” (p. 192). Now, whereas Searle explains this connection by pointing out that the presence of the state is the sincerity condition of the act, we go further. In our view illocutionary acts just are expressions of intentional states (anything more to a speech act is perlocutionary), and types of illocutionary acts are best classified in terms of types of intentional states expressed (Bach and Harnish, 1979, Chs. 1 and 3). Since there is nothing more to illocutionary force than type of state expressed (except for the special case of conventional illocutionary acts, ibid. Ch. 6), the above three connections between speech acts and intentional states are utterly trivial (virtual identities), being nothing more than features shared by expressed intentional states and actual intentional states.

Moreover, these analogies provide no support whatever for Searle’s claim that the representationality of sentences derives from that of intentional states; his description of the relation between speech acts and intentional states makes no mention of the meaning of the sentences used to express the state, and of the fact that sentences can be used nonliterally to do so. Searle simply does not address the issue of the relation between linguistic meaning and intentionality. He would maintain, and we would agree, that the former derives from the latter, but his three analogies provide no support for this claim.

Finally, Searle denies (giving no reasons other than the failure of behaviorism) that “there is a nonintentional explanation of intentionality” (p. 195). Is he suggesting (like Brentano) that when physics, chemistry, and biology (but not cognitive psychology?) have explained all that they can, still intentionality will not be accounted for? This would be a curious position to take if one believes also that
“Whatever else intentionality is, it is a biological phenomenon, and it is as likely to be as causally dependent on the specific biochemistry of its origins as lactation, photosynthesis, or any other biological phenomena” (Searle, 1980, p. 424). If intentionality really is a biological phenomenon, seemingly it should have a biological explanation. Searle may be right to regard mental states, as opposed to sentences and pictures, as intrinsically rather than derivatively intentional, but this does not mean that their intentionality cannot be explained by natural science. On the other hand, if in denying that there is a nonintentional explanation of intentionality, Searle means only that “there is no analysis of intentionality into logically necessary and sufficient conditions” (p. 195), it is not enough to claim that analytical behaviorism is false. Field (1978) has suggested the possibility of a materialistically adequate, functionalist theory of content, and though not yet developed, this sort of approach should not be ruled out of court. Keeping in mind that reductionist theories need not be eliminative and that intentionality does not imply consciousness, we should not feel threatened by the feasibility of such a theory.

Kasher. After saluting Bar-Hillel and pragmatics, A. Kasher in “What is a Theory of Use?” proposes new goals for pragmatics, presenting some major problems and suggesting some ingredients of possible solutions.

Kasher regards the goal of pragmatics “to be the specification and explanation of the constitutive rules of the human competence to use linguistic means for effecting literal purposes” (p. 38). He takes it that “a human competence is defined by a finitely representable, flexible system of constitutive rules” (p. 39), and that literal purposes are those which “do not induce any assumption or presupposition about any other use of the same means under the same circumstances” (p. 40).

As a condition of adequacy on pragmatic theories incorporating the notion of contextual appropriateness Kasher (p. 41) suggests the following:

(CA) Context C is linguistically appropriate for speaker Alpha to utter in it sentence S of Language L if and only if there is a linguistic institution of L which grants Alpha an institutional role which enables him (or her) to achieve a literal purpose he (or she) entertains in C, by uttering in it the sentence S.

Kasher poses problems for certain crucial notions in (CA) and relates them to the work of Searle and others. Kasher then distinguishes “two major ways of tackling these problems: on the interactive view, the ability to use language to effect given purposes results from an application of the general human powers of Use and Will to particular systems of linguistic means” (p. 46). On the intra-active view, pragmatic competence is a “matchless power of our mind, which provides general purpose of a unique kind, to be effected by putting to use, in an unparalleled way, available and suitable linguistic means” (p. 46). Unfortunately, Kasher does not identify anyone as either an interactionist or as an intra-actionist, but he suggests that Grice and Searle (respectively) might be cases. He also thinks both are wrong and tries to steer a middle course. Apparently against intra-actionists Kasher contends (p. 49) that all theoretical achievements of the theory of conversational implicature are derivable from appropriate applications of principles of rationality to standard cases of linguistic activity, and he attempts to do so for four of Grice’s maxims of conversation (pp. 48–49). Against the interactive view Kasher contends that there are particular rules of use which are not derivable from general principles. Although Kasher considers the case of the purported ambiguity of indefinite articles (“John wants to meet a spy”) from the point of view of Hintikka’s game theoretic semantics, he actually gives no examples of such rules.

The upshot of these criticisms is Kasher’s intermediate view: some pragmatic rules are very general across institutions; some are restricted to particular institutions; and some are confined to certain expressions in a given language (p. 51). This would all be very ecumenical if there was any reason to suppose there are such ‘rules,’ but Kasher has given us no reason to believe in their existence. Nor has anybody else. Still, we are indebted to Kasher for taking on the important but difficult topic of contextual appropriateness.

Dummett. ‘The Appeal to Use and the Theory of Meaning’ hardly mentions use but concerns, rather, what Dummett uses “use” to refer to. Most idiosyncratically, he interprets the doctrine of meaning as use as claiming that “the knowledge in which a speaker’s understanding of a sentence consists must be capable of being fully manifested by his linguistic practice” (p. 135), but at least his discussion makes clear what his conception of use has to do with his pet project of defending justification—over truth-conditional semantics. “A model of
meaning in terms of a knowledge of truth-conditions is possible only if we construe truth in such a way that the principle of bivalence fails; and this means, in effect, some notion of truth under which the truth of a sentence implies the possibility, in principle, of our recognising its truth” (p. 135). Since we see no reason to concede that warranted assertibility is a notion of truth at all, we prefer to construe the issue neutrally as about the choice of a T-predicate for semantics.

Dummett registers the familiar charge that ordinary, bivalent truth-conditional semantics is ultimately uninformative and essentially circular (p. 130) and maintains that justification-conditional semantics is not. Unfortunately, he does not defend the assumption, which he shares with his opponents, that the theory of understanding belongs to the theory of meaning. As mentioned earlier, Putnam rejects the view that “to understand a statement is to be explained as knowing its truth conditions” (p. 200), and one of his reasons is that understanding does not require metalinguistic knowledge of any sort. If so, then the question of what is the best T-predicate for semantics does not concern the theory of understanding.

At any rate, according to Dummett the constraints imposed on the theory of meaning by the identification of meaning with use are indicated “very precisely” by “the thesis that what constitutes the justification of an assertion determines, or shows, the sense of the sentence asserted” (p. 132). To do that, however, this neo-verificationist thesis will have to be formulated more precisely. For one thing, assertions can be justified (in some sense) even if what is asserted is not, and they can fail to be justified even if what is asserted is justified. But even if we restrict our attention to the justification of what is asserted and the relation of that to the meaning of what is asserted, we still need a reply to easy objections like the following. Imagine a clever device that assigns values of 1 or 0 to sentences that are punched into it. Miraculously, the 1-sentences are almost always true and the 0-sentences almost always false, so that the device’s reliability is beyond doubt. Suppose that a conscientious speaker had instant access to this device and consulted it whenever he was about to open his mouth. Now clearly the sort of justification thus provided for his assertions is utterly irrelevant to the theory of meaning, which requires something other than “is assigned the value 1 by the device” to play the role of T-predicate. Dummett needs a notion of justified assertion that is connected with an account of what it is for a speaker to understand what he asserts. As Dummett himself insists, “our grasp of what is to count for us as showing that it is true must be derived in some way from our knowledge of its meaning” (p. 130). It is incumbent upon Dummett, if he is to maintain his thesis, to develop more precisely a notion of justification that helps to explain how a speaker can make assertions comprehendingly without understanding their truth conditions (in the ordinary sense of “truth”).

We repeat our agreement with Putnam that understanding sentences is not a matter of having metalinguistic knowledge of the form “S is true iff p,” regardless of how “true” is interpreted. Our reason is not the same as Putnam’s. We agree with Schiffer (forthcoming) that “the theory of content for public language reduces completely to the theory of content for thought.” Specifically, how a speaker understands the sentences in his language is to be characterized in terms of the attitudes he uses them literally to express. So, for example, the justification and the truth condition of an indicative sentence depend on the justification and the truth condition of the belief it is used literally to express.

Quine. In ‘Use and Its Place in Meaning,’ Quine (re) places use in the analysis of meaning: “what, then, is the semanticist to study and analyze if not the meanings of words? The use of words . . . We can take . . . the use and let the meaning go” (p. 1). But what will a theory of use look like? Aside from special cases, Quine doesn’t say. Instead he notes that we often give the meaning of a word by giving something with same use. But because of ambiguity and what not, we must often cite sentential contexts, so that “what is more to the point than the relation of synonymy of words to words and phrases, then, as a central concept for semantics, is the relation of semantical equivalence of whole sentences” (p. 2). But what is semantical equivalence? It is not that “their utterance would be prompted by the same stimulatory situations” because “they cannot both be uttered at once” (p. 2). Rather, “we can cut through all this if we limit our attention to the cognitive equivalence of sentences; that is, to the sameness of truth conditions” (p. 3). But this will work only if we “limit our attention for a while in yet another way: we must concentrate on occasion sentences” (p. 3). So the picture looks like this:

First, there is the relation of sameness of overall stimulation of an individual at different times. This is defined, theoretically, by sameness of triggered receptors. Next there is the relation of cognitive equivalence of occasion sentences for the individual.
This is defined by his disposition to give matching verdicts when the two sentences are queried under identical overall stimulations. Next there is the relation of cognitive equivalence of occassion sentences for the whole linguistic community. This is defined as cognitive equivalence for each individual. Finally there is the relation of cognitive synonymy of a word to a word or phrase. This is defined as interchangeability in occassion sentences salo equivalence. We could take the nominal further step, if we liked, and define the cognitive meaning of a word as a set of its cognitive synonyms (pp. 5–6).

The account does not extend to standing sentences (p. 7), and even worse, it must be restricted to non-opaque contexts (p. 6). So what are we left with? An account of cognitive synonymy for some occurrence sentences for a given speaker of a language (at a certain time).

What is the point of all this? This old behaviorist story has gone nowhere in 30 years. Why retell it again, and without even mentioning the best available efforts to explain meaning in terms of use, the theories of Grice, Schiffer and Searle?

Dascal. In 'Conversational Relevance,' Marcelo Dascal is worried about the vagueness of Grice's maxim of relevance and believes that a precise conception of relevance is needed "to account for the widespread occurrence of ... conversational implicatures" (p. 155). However, with good reason Dascal doubts the feasibility of any formal account of relevance. He likens the hearer's problem of identifying an implicature with scientific hypothesizing, for which "there is no 'method of discovery' or 'abductive logic'" (p. 167). Identifying an implicature requires making "a hypothesis about the speaker's intentions that explains away the apparent irrelevance of his utterance, by explaining how the utterance is in fact relevant" (p. 167). We agree, but would add that identifying an implicature is a special case of the hearer's general task of explaining the speaker's utterance by identifying his intentions (Bach and Harnish. 1979. pp. 89–91). Implicatures belong to the category of indirect (constative) illocutionary acts (ibid. pp. 165–173), and those involving exploitation of the maxim of relevance rely on the fact that the ostensible irrelevance of an utterance is recognizable as such.

Surely relevance is a vague notion, but we believe Dascal's worry about it is misplaced when he poses his problem, "how can such precise messages as the implicatures be conveyed via a reasoning that makes essential use of such an imprecise notion as relevance" (p. 159)? Whereas the requirement of relevance triggers and guides the hearer's search for nonliteral and indirect uses of language, rarely does it determine precise content. Moreover, the philosophical theory of conversational implicature does not require an answer to Dascal's question, which really belongs to social psychology. People's judgments of and agreement about relevance (or salience, another fascinating notion for the theory of cooperative behavior) require explanation all right, but the philosophical theory of conversation can get along without such explanation. Just as it can proceed without a psychological explanation of the fact that people understand their language in much the same way, and just take this to be a fact, so it can proceed on the assumption that somehow or other people for the most part have a shared sense of relevance/irrelevance. Notice that the task of social psychology here includes explaining discrepancies in judgments of relevance, as a function, we may suppose, of social differences and of personal unfamiliarity. Not just conversational implicature in particular but communicative success generally requires the exploitation of mutual beliefs, and the more people know or can expect of one another, the more they can communicate successfully. The job of a philosophical theory is to explain what successful communication consists in: the job of social psychology to explain the success or failure of particular efforts to communicate.

We must also question Dascal's suggestion that there are two kinds of relevance, semantic and pragmatic. Better, there may be semantic as well as pragmatic contributions to what is relevant to a conversation at a given point. Insofar as so-called semantic relevance is a relation between propositions, rather than of utterances to junctures of conversations, the relation is really one of informational connectedness (strictly speaking, then, "relevance logic" is a misnomer). Moreover, Dascal's examples of semantic relevance are dubious. For instance, in regard to one of Grice's examples, Dascal supposes that "there is a clear semantic relation between the terms 'garage' and 'selling petrol'" (p. 170) and claims the same for an occurrence of "Smith" and a use of "he" to refer to Smith. In any case, Dascal acknowledges that "nothing in my argument hinges on the acceptance of a well-defined borderline between semantics and pragmatics" (p. 164). However, despite his many valuable observations about conversational implicature, they are too fragmentary, though mutually relevant, to constitute an argument at all, at least for anything beyond the fact that the subject is complex.

Davidson. The central question in Davidson's 'Moods and Per-
formances is this: can a theory of truth explain the difference between the moods? First he remarks,

The moods indirectly classify utterances, since whatever distinguishes sentences can be used to distinguish utterances of them. So we may ask, what is the relation between these two ways of classifying utterances; how are assertions related to utterances of indicative sentences, for example, or commands to utterances of imperative sentences? (p. 9)

Davidson recognizes (pp. 9–10) that the two ways of classifying are not identical; “many utterances of indicative sentences are not assertions” (e.g., plays, pretense, joking, and fiction). Moreover, even in cases where we do mean something, we can be speaking nonliterally or indirectly (see Bach and Harnish 1979, Ch. 4), so that, for example, an interrogative sentence can be used to make a statement and an indicative one to issue an order.

Davidson next (pp. 11–13) argues against Dummett’s view that assertions, commands, and questions consist in uttering indicatives, imperatives and interrogatives under conventionally given conditions. The existence of nonliteral and indirect acts makes it difficult to formulate such conventions, but Davidson’s objection is that they do not exist. One piece of evidence for this is how elusive they are to specify. If such a convention were embodied in a linguistic device, “why would not the tease or romancer include that very item in his utterance?” (p. 12) Thus, “what must be added to produce assertion cannot be merely a matter linguistic convention” (p. 13). Strawson came to that conclusion in 1964, though not for the same reason.

Nevertheless Davidson believes (p. 14) that there is a conventional connection between the moods and their uses, and thinks this shows reductive theories like Lewis’ (1970) to be incapable of accounting for differences among moods. According to Davidson, an adequate theory of the moods must: (1) show or preserve the relations between indicatives and corresponding sentences in other moods (p. 14); (2) assign an element of meaning to utterances in a given mood that is not present in utterances in other moods. And this element should connect with the difference in force between assertions, questions and commands in such a way as to explain our intuition of a conventional relation between mood and use (p. 15); (3) be semantically tractable (p. 15). Davidson does not say what the point of (1) is. There are all sorts of relations between different sentences (phonological, mor-

phological, syntactic, semantic, pragmatic) so we need motivated relationships in conditions of adequacy on theories. Nor does Davidson say what being ‘semantically tractable’ is, though being embraced by a truth theory suffices.

Davidson’s proposal is to view utterances of sentences like (1a) as utterances of the two sentences in (1b), and to treat nonindicatives analogously:

(1a). Jones asserted that it is raining.

b. Jones asserted that. It is raining.

As Davidson puts it “just as a nonindicative sentence may be decomposed into an indicative sentence and a mood-setter, so an utterance of a non-indicative sentence may be decomposed into distinct speech acts, one the utterance of an indicative sentence, and the other the utterance of a mood-setter” (p. 18). What are these mood-setters? “We may think of non-indicative sentences, then, as indicative sentences plus an expression that syntactically represents the appropriate transformation; call this expression a mood-setter.” Davidson notes that we mark these moods using such varied devices as verb, word order and intonation. Yet he thinks there is some expression that “syntactically represents” the transformation of an indicative expression into a non-indicative with just the right combination of these devices. Nothing with less than the power of a transformational grammar is likely to suffice for this job, but is a mood-setter an expression for that? Its hard to say since Davidson is obscure here. At one point (p. 19) he says “the mood-setter cannot be any actual sentence of English since it represents a certain transformation.” Later he says “we can give the semantics of utterance of an imperative sentence by considering two specifications of truth conditions, the truth conditions of the utterance of an indicative sentence got by transforming the original imperative and truth conditions of the mood-setter. The mood-setter of an utterance of ‘Put on your hat’ is true if and only if the utterance of the indicative core is imperative in force” (p. 19). But, crucially, Davidson tells us nothing about what being ‘imperative in force’ amounts to. Does it mean ‘has the force of an imperative’? (After all, imperative is a kind of sentence, not a kind of force.) If so, Davidson’s theory of mood for imperatives reduces to this: imperatives (defined in part structurally) have the force of imperatives. Indeed, but what force is that?
Curiously, Davidson thinks his proposal satisfies requirement (2) above when he writes, "the conventional connection between mood and force is rather this: the concept of force is part of the meaning of mood. An utterance of an imperative sentence in effect says of itself that it has a certain force" (p. 20). Ignoring the usual problems with finding mention where there appears to be only use (recall Church's translation arguments), this 'saying of itself' is unlikely to be a satisfactory explanation for our intuition of a conventional connection between mood and use. And even if it were, it would not constitute a theory of mood yielding a satisfactory answer to Davidson's original question (how are assertions related to utterances of indicative sentences, or commands to utterances of imperatives), because the answer "utterances of imperatives have the force of imperatives" is hardly informative.

CONCLUSION

The volume under review would have benefited from more interaction among the participating symposists. Barely half of the papers are followed by comments, and these are often tame and occasionally obsequious. The collection contains much that is interesting and suggestive, but there is little cohesion among the parts. Of the four (out of thirteen) papers concerned with general issues of meaning and use (those by Quine, Kasher, Dummett, and Putnam), only Kasher's gives any indication that the theory of speech acts exists. And if that isn't the theory of use, we don't know what is.

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