

MORE ON WHAT WE SAY

STANLEY BATES AND TED COHEN

In 1958, Stanley Cavell published an article entitled "Must We Mean What We Say". The article was in part a response to a paper of Benson Mates,² but it was also, in the opinion of Vere Chappell, "the most detailed explanation and defense of the procedures of ordinary language philosophy that has yet appeared".³ In 1962 Cavell published another article, "The Availability of Wittgenstein's Later Philosophy" in which he developed his conception of "ordinary language philosophy with special reference to Wittgenstein's procedures in the *Philosophical Investigations*".⁴ In 1963, Cavell's articles were attacked in a well-known article by Jerry Fodor and Jerrold Katz entitled "The Availability of What We Say".⁵ Cavell has chosen not to respond directly to the intemperate attack made on his work though he has referred to it in a later article in a manner which obviously indicates that he does not think that it has succeeded in shaking his position.⁶ Richard Henson has supported certain of Cavell's claims while attacking some and reformulating others, but except for this Cavell has generally been undefended in the literature.⁷ Since Cavell has recently republished both of the articles at issue in a volume of his essays, it might now be a propitious moment to re-examine the criticisms of his work by Fodor and Katz.⁸ It is our impression that a lack of specific

¹Stanley Cavell, "Must We Mean What We Say?", *Inquiry*, Vol. 1 (1958), 172-212.

²Benson Mates, "On the Verification of Statements about Ordinary Language", *Inquiry*, Vol. 1 (1958).

³V. C. Chappell (ed.), *Ordinary Language*, (Englewood Cliffs; 1964), p. 4.

⁴Stanley Cavell, "The Availability of Wittgenstein's Later Philosophy", *Philosophical Review*, LXXI, (1962), 67-93.

⁵Jerry A. Fodor and Jerrold J. Katz, "The Availability of What We Say", *Philosophical Review*, Vol. LXXII (1963), 57-71.

⁶Stanley Cavell, "Aesthetic Problems of Modern Philosophy", in Stanley Cavell, *Must We Mean What We Say?* (New York, 1969), p. 95.

⁷Richard Henson, "What We Say", *American Philosophical Quarterly*, Vol. II, pp. 52-62.

⁸We shall give page references for Cavell, "Must We Mean What We Say?" and Fodor and Katz "The Availability of What We Say", in the body of the text, in parentheses, at the conclusion of quotations. Page references for Cavell will be to both his book, cited in note 6 above, and to the collection of essays edited by Chappell cited in note 3. The first number will be for *Must We Mean What We Say?*, the second for *Ordinary Language*.

defense has caused many people to assume that Cavell's claims are indefensible against these criticisms.

The scope of this paper is relatively limited. We do not intend to develop Cavell's position nor to present any new arguments in its favor. We do intend to show that none of the criticisms made of it by Fodor and Katz has any force.⁹ In order to do this, we shall state what we take to be the central issue which is at stake in the conflict, and then we shall examine in detail the Fodor and Katz paper. In the final, more speculative, section, we venture some opinions about the deeper issues at stake in this controversy. One of the reasons we think that this is worth doing is that we believe that the Cavell papers are still the best discussion and defense of the procedures of "ordinary language philosophy".

I

There is a central issue concerning which Cavell and Fodor and Katz are in disagreement. Stating the issue clearly is a precondition for investigating the Fodor and Katz criticisms of Cavell. Cavell makes distinctions between (1) "statements which produce instances of what is said in a language", (2) "statements which make explicit what is implied when we say what statements of the first type instance us as saying" and (3) "*generalizations* to be tested by reference to statements of the first two types". (3, 77) The issue concerns the relevance of *evidence* to the truth of these various kinds of statements. Empirical linguistics seems to be a discipline which compiles a vast amount of evidence which is relevant to the truth of these sorts of statements. Ordinary language philosophers produce these statements without engaging in the same laborious questioning that empirical linguists must use to compile their evidence. Isn't it obviously the case that we have two competing parties in the

⁹The disregard of "scientific" linguistics putatively exemplified by ordinary language philosophers has been criticized on two accounts. The first is that such philosophers philosophize ineptly since they begin with claims about ordinary language which lack the necessary support. The second is that such philosophers, however well they manage, do less than could be done since they proceed without some helpful machinery. Roughly, the first view claims that ordinary language philosophy is defective; the second claims that ordinary language philosophers either claim or are committed to claiming that scientific linguistic philosophy is defective. The first view is that of the Fodor and Katz paper, and it is the only one with which we are concerned in this paper. The second view can be found in Zeno Vendler's "Linguistics and the *A Priori*", chapter one of his *Linguistics in Philosophy* (Ithaca, 1967). Vendler says, for instance, that "people like Ryle and Cavell do not see much hope in linguistics for philosophy" (p. 6).

Vendler's thoughtful paper merits discussion—both of how accurately it represents Cavell and of issues it raises independently.

same line of work, one of which is industrious and scientific, the other of which is lazy and pre-scientific in its methodology? (Fodor and Katz think that this is obviously the case.) Yet, Cavell says "for a native speaker to say what, in ordinary circumstances, is said when, no [such] special information is needed or claimed. All that is needed is the truth of the proposition that a natural language is what native speakers of that language speak". (5, 79)

The particular example with which Cavell is concerned is a disagreement about the use of 'voluntary' between Ryle and Austin.

Thus, for example, while Professor Ryle tells us that 'voluntary' and 'involuntary' in their ordinary use are applied only to actions which ought not to be done, his colleague Professor Austin states in another connection '... for example take "voluntarily" and "involuntarily"'. We may join the army or make a gift voluntarily, we may hiccough or make a small gesture involuntarily . . .' If agreement about usage cannot be reached within so restricted a sample as the class of Oxford Professors of Philosophy, what are the prospects when the sample is enlarged?¹⁰

Austin makes a type 1 statement which conflicts with what Fodor and Katz say is a type 2 statement of Ryle's (though Cavell calls Ryle's statement a generalization which is a type 3 statement). How is the disagreement to be resolved? Fodor and Katz say

The basic question Cavell raises and seeks to answer is whether there is any reasonable sense in which such disagreements as this one are empirical. The position which Cavell evolves, and which we shall seek to refute is that such disagreements are in *no* reasonable sense empirical. (59)

One could complain that Cavell does not make an explicit generalization about *all* disagreements of a certain kind but this is not the appropriate comment. It is, however, worth stating that Fodor and Katz's characterization of the dispute between them and Cavell is not correct. Cavell does not say that the dispute between Ryle and Austin is not empirical. Indeed, if empirical is taken as "concerned with a matter of fact" it is clear that he thinks the dispute is empirical and that Austin is

¹⁰Benson Mates, "On the Verification of Statements about Ordinary Language", in Chappell, *op. cit.*, p. 68.

correct and Ryle is wrong. What Cavell does claim is that it is not necessary to take a poll to discover who is right and who is wrong, and that the absence of a poll justifying our claims concerning correctness does not make those claims dogmatic or unempirical. Indeed, he says that Ryle could be expected to see that he was wrong, since he is a native speaker. What he is wrong about is a matter of fact—what it is that we mean when we say something. It is characteristic of Fodor and Katz that they continually speak of “empirical evidence” where Cavell talks of evidence. This makes it easier to criticize Cavell since his major claim is that there are areas which we think of as being about matters of fact—hence, empirical—for which we do not need evidence. Cavell’s own formulation is that

we must bear in mind the fact that these statements—statements that something is said in English—are being made by native speakers of English. Such speakers do not, in *general*, need evidence for what is said in the language; they are the source of such evidence. It is from them that the descriptive linguist takes the corpus of utterances on the basis of which he will construct a grammar of that language. (4, 78)

The emphasis on the word ‘general’ is Cavell’s and it is important. Cavell has much more to say on a large number of topics in the two articles which Fodor and Katz cite (though almost all of their references are to “Must We Mean What We Say?”) but it is his view about the relationship of *evidence* to what we say about our language which Fodor and Katz consider “pernicious both for an adequate understanding of ordinary language philosophy and for an adequate understanding of ordinary language”. (58)

II

We now consider nine points made by Fodor and Katz, which more than exhaust their arguments against Cavell. It is worth re-emphasizing that we are presenting no more than indirect support for Cavell, by way of undoing Fodor and Katz’s putative refutation. However, it will be apparent that we have a stronger commitment to Cavell’s position: it is not just that Cavell is right if Fodor and Katz present the only reasons to think he’s not, but that he is right. And, in fact, Fodor and Katz are committed to one aspect of the broad theme Cavell is articulating. They share the view that (at least some) philosophical problems can be dealt with only by taking account of the uses

of ordinary, natural language. On this score they belong with Ryle, Austin, Wittgenstein, and Cavell, and not with those philosophers in the ideal or technical language tradition—most notably, Carnap.¹¹ The main aim of this paper is to send people to (or back to) Cavell's texts. Those who look there will note that one of Cavell's most tantalizing suggestions is that what one knows about one's language can bear on one's philosophical worries precisely because this knowledge is of a special kind.

(1) In response to the remark of Cavell's quoted directly above, Fodor and Katz say,

Thus Cavell argues that Ryle and other native speakers are entitled without appeal and empirical evidence, to whatever type 1 statements they require to support their type 2 statements since type 1 statements are not relevantly confirmed or disconfirmed by empirical evidence. (59)

But Cavell's statement does not imply that he thinks speakers are always entitled to whatever type 1 statements they "require" for whatever purposes they might have. This would commit him to the straw-man position that Fodor and Katz from time to time seem to be attacking: the view that anybody is entitled to say whatever he wants to say about "what we say". (Footnote 19; 21, 93) If one ties the concept of entitlement to the "having of empirical evidence", then it *will* be paradoxical to claim entitlement without the supporting empirical evidence. The central claim of Cavell's article, which Fodor and Katz are seeking to refute, is that, in the area of what we say, entitlement is not tied to empirical evidence. If we rephrase Fodor and Katz's characterization of Cavell as "native speakers are entitled without appeal to empirical evidence to whatever type 1 statements (which are in fact specimens of what we say) they require to support their type 2 statements . . ." what is being said doesn't appear paradoxical, unless the *additional* claim is made, and substantiated, that empirical evidence is always required for something to count as a specimen of what we say. Cavell never argues that no native speaker can ever be wrong about any of the types of statements which he discusses. He only argues that native speakers cannot, *in general*, be wrong about type 1 or type 2 statements.

¹¹They attack both the ideal language tradition, and ordinary language philosophy in their joint article, "What's Wrong with the Philosophy of Language?", *Inquiry*, Vol. V (1962), pp. 197-237.

(2) Fodor and Katz accuse Cavell of a *non-sequitur*, saying that he

argues from the premise that a native speaker is the source of the linguist's empirical evidence for the description of a natural language to the conclusion that the native speaker's statements about his language cannot in turn, be in need of empirical evidence for their support. (60)

They claim that one can distinguish a native speaker's utterances from his metalinguistic claims, and that the latter need not be true, in order for a successful empirical description of the language to be made. Otherwise a linguist would have to be able to separate the truths about the language from the falsehoods before he could begin to describe the language. First, it should be mentioned that Cavell does not claim that empirical evidence is always irrelevant to every type 2 statement made by a native speaker. He admits that they can be mistaken. Presumably if the vast majority of native speakers rejected a type 2 statement advanced by some other native speakers, this would be relevant to any judgment that we might make about the correctness of the type 2 statement. The interesting question, on which Cavell focuses his attention, is how the native speaker who has produced the incorrect type 2 statement can be brought to see that he has been mistaken.

Second, it is worth noting that the metalinguistic claims of native speakers are also utterances of those speakers. This truism may be worth noting at a time when the myth of the "meta-language" or "background language" has been with us for such a long time. Surely one of the commonest methods of a linguist, attempting to achieve an empirical description of a language, is to ask for "metalinguistic" information from native informants. Max Black in discussing the Whorfian hypothesis tells of Whorf that "His theories about the Hopi world view were almost exclusively based upon information supplied by a single speaker of Hopi, living in New York . . . (He paid a short visit to the Arizona Hopi reservation later). Whorf undoubtedly achieved an extraordinary *tour de force* in reconstructing, on so narrow a basis, an interpretation of Hopi thought and culture that competent experts have praised for its faithfulness and insights."¹² Imagine a linguist trying to describe empirically some language in a situation in which all of his informants make false metalinguistic

¹²Max Black, *The Labyrinth of Language*, (New York, 1968), p. 93.

claims (or, perhaps better, attempt to deceive him about the language). It will be extremely difficult, if not impossible, for him to organize coherently his data since there will be a large number of internal contradictions or discrepancies. Ordinarily, the linguist proceeds on the assumption that the metalinguistic claims of native speakers are generally true (just as he assumes that most of the statements he encounters are true or sincere). Of course, it is possible that the natives may wish to deceive him, or to make him an object of ridicule, or to prevent a contamination of their culture that could occur if they let him understand them. A linguist may suspect that one of these things is happening, and then he's in trouble, but in the absence of such special suspicions he would proceed on the assumption of the general truthfulness and correctness of native speakers. Jonathan Bennett discusses this kind of situation in his book *Rationality*.

Suppose we are trying to establish that a certain kind of behaviour constitutes a language, in the only way in which this can be definitively established, namely by translating it. An essential part of our task will be to discover some sort of pattern or regularity in the relationships between the behaviour in question and the experience of the creatures whose behaviour it is. If we can find enough patterns of this kind, then we shall have our translation; but we are likely to find that some of the creatures' performances do not conform to the overall pattern which we have discerned, and if we keep to our translation we shall have to dismiss these nonconforming utterances as untrue. We cannot, however, begin by separating the true from the untrue, and then attend only to the former in developing our translation: whether we take an individual utterance to say something true or not depends upon how we translate it. But also, conversely: how we translate the whole set of utterances depends upon which of them we take to be true. The only way out of the impasse, so far as I can see, is to begin our translation endeavours on the working assumption that every utterance in the language is true—that is, to try to find some pattern which accommodates them all. If we fail in this endeavour, but find some pattern which covers more of them than any other pattern we can find, then we shall base our translations upon that pattern, and shall dismiss as untrue the utterances which do not fit it. In other words, we must stipulate that as many as

possible of the utterances in the language shall be so interpreted as to be true.¹³

Fodor and Katz charge that "what Cavell has failed to show is precisely that the possibility of an empirical description of a natural language presupposes the truth of the metalinguistic claims of its speakers". (60) The charge is beside the point, as is the Fodor and Katz argument in support of it that a linguist can succeed in giving an empirical description of a language *that is not his own* without relying on any metalinguistic claims of native speakers. (This argument does seem dubious to us.) Cavell has no need to show what Fodor and Katz say he has failed to show. What he is claiming is that, in *general*, a native speaker doesn't have to go through the process which a non-native linguist does, in order to "tell what is and isn't English, and to tell when what is said is properly used . . ." He claims that it is possible for a native speaker to make true "metalinguistic claims" which are parts of "an empirical description of a natural language" without *always* having to rely on more than his own knowledge of his native language. Nothing Fodor and Katz say here militates against that claim.

(3) Fodor and Katz present an argument concerning phonology which is totally misguided. Cavell suggests that empirical evidence is relevant to a discussion of phonology. They say,

An argument might be given for classifying the history of a language and the study of special forms in the morphology of a dialect as areas about which a native speaker who is philologically naïve can say little. But clearly such an argument would be impossible in the case of the sound system, since the native speaker knows the sound system of his language in exactly the same way that he knows its syntax and semantics. (60-61)

In support of their claim they append a note in which it is claimed, "Precisely this point is made in M. Halle, 'Phonology in Generative Grammar', forthcoming in *Word*, where Halle demonstrates that the logical form of phonological rules is identical with the logical form of grammatical rules". (61) Obviously, though, even if Halle's demonstration were correct, the truth of his conclusion about the identity of *logical form* is entirely irrelevant to the Fodor and Katz assertion concerning how an individual *knows* the sound system of his language. We

¹³Jonathan Bennett, *Rationality*, (London, 1964), pp. 62-63.

would venture to say that most individuals know ("know" in the sense of "can state") some grammatical rules (though not many, and probably descriptively inaccurate ones) and many semantic connections, since these are, after all, drummed into elementary school children in this country. However, we doubt that very many people know any phonological rules at all. Of course, they know how to pronounce the words they speak (or, better, know how to speak), but they haven't learned this by rule. In the way in which one knows the sound system of his language, which is similar to the way he knows its syntax and semantics, Cavell could accept that type 2 statements about pronunciation aren't relevant to empirical evidence. This would not constitute a *reductio ad absurdum* as Fodor and Katz think. They say that it does because "it entails that a native speaker of English could never be wrong (or at least could not very often be wrong) about how he pronounces (we pronounce) an English word (or spells one?)". (61) Leaving aside the suggestion concerning spelling which seems frivolous, we see nothing to object to in this conclusion. I can never be wrong about how I pronounce a word. Ask me how I pronounce it and I'll pronounce it for you. I may not be able to write it out phonetically but that has nothing to do with a claim that I know how I pronounce it. (The use of 'know' does seem slightly odd, which seems to us to support Cavell's view). The first person plural statements, "We pronounce . . ." are more complicated in just the way that Cavell discusses in the article. Of course, if I'm asked how "we" pronounce a word which I've never encountered before, I may not know, but that only means that I'm not entitled to the "we" statement here.

(4) The next argument that Fodor and Katz have is that

Cavell's explanation of how Ryle went wrong [i.e. that Ryle specifies too narrowly the condition for applying the term 'voluntarily'], even if it is wholly correct, fails to show that Ryle's mistake is not an out-and-out empirical error.

Once again it is the use of 'empirical' that is crucial here. If 'empirical' is taken as, roughly, "about a matter of fact" then of course Cavell has no interest at all in showing that Ryle's error is non-empirical. His point is that it is an empirical error, an error about a fact of language—and we can discover that it is an error without engaging in any detailed empirical investigation but simply by being brought to attend to "what we say". Of course, if "empirical error" is taken to *mean* "error made by

not undertaking a sufficiently wide empirical investigation" then, according to Cavell, Ryle's error is non-empirical, but what difference does this make?

Fodor and Katz give an example to which we might attend for a moment in order to see this issue more clearly. They say that Ryle's error is very much like the obviously empirical error of a biologist who asserts that all reproduction is sexual, omitting cases of fission, budding, and fragmentation. However, there are two sorts of error that the scientist might be making in two rather different situations. (We have no objections to calling both of the errors empirical, in the broad sense of that term that we have specified.) In the first case, the biologist may simply be ignorant of the fact that such processes as fission, budding and fragmentation occur. We show him examples of all these processes; he agrees that these processes are, indeed, examples of reproductive processes and hence, is brought to see the error of his previous claim that all reproduction is sexual, which was too narrow and empirically unjustified. The second kind of case is rather different. Let's suppose that when he is reminded of these processes, he doesn't withdraw his claim. Instead, he says that he's perfectly well aware that fission etc. occur, but that he doesn't call them reproduction. Or, perhaps he says that he did not have them in mind when he ventured a hasty statement about reproduction, but as soon as he is reminded of them he withdraws the statement. It is this last sort of case which is more like Ryle's error. Notice that now the scientist's mistake concerns language—what we mean when we say "reproduction"—and that the question of the relevance or irrelevance of empirical evidence in correcting the error is the point at issue. (This may not be a good example because 'reproduction' belongs both to ordinary language and to the more specialized vocabulary of biology. You get a different answer to the question "Is a tomato a fruit?" depending on whether the ordinary or specialized sense of 'fruit' is intended.)

(5A) Fodor and Katz's next argument concerns two statements which Cavell compares—statement S: "When we ask whether an action is voluntary, we imply that the action is fishy." (12, 85) and statement T: "Is X voluntary? implies that X is fishy." (13, 86) Cavell says that though these statements are true together and false together, they are not everywhere interchangeable. He then goes on in a paragraph quoted by Fodor and Katz to specify what the difference is and exactly when and why they are not interchangeable,

. . . the identical state of affairs is described by both, but a person who may be entitled to say T, may not be to say S. Only a native speaker of English is entitled to the statement S, whereas a linguist describing English may, though he is not a native speaker of English, be entitled to T. What entitles him to T is his having gathered a certain amount and kind of evidence in its favor. But the person entitled to S is not entitled to *that* statement for the same reason. He needs no evidence for it. It would be misleading to say that he *has* evidence for S, for that would suggest that he has done the sort of investigation the linguist has done, only less systematically, and this would make it seem that his claim to know S is very weakly based. And it would be equally misleading to say that he does *not* have evidence for S, because that would make it appear that there is something he still needs, and suggests that he is not yet entitled to S. But there is nothing he needs, and there is no evidence (which it makes sense, in *general*, to say) he has: the question of evidence is irrelevant. (13-14, 86)

Cavell's alleged "first mistake" is to suppose that, given S and T are true together and false together, anything follows just from the fact that S and T are not everywhere interchangeable. But, if one reads the paragraph quoted, it can be seen that Cavell does not claim that anything follows from that fact alone. He carefully spells out what he conceives the difference to be, viz., that a non-native speaker of English could be entitled to T (on the basis of empirical evidence) but not to S. Now this claim might be incorrect, but it must be *shown* by Fodor and Katz to be incorrect, and there is no argument here which tends to show this. Indeed, they ignore entirely his suggestion that what distinguishes S from T is the presence of the first person plural forms. This brings to light one of the most significant features of the disagreement between Fodor and Katz and Cavell, viz., that Cavell is concerned with something like speech acts while Fodor and Katz are concerned with sentences. The difference between S and T relates to the difference in the situations in which they could appropriately be employed. One of Cavell's main points is that "We are . . . exactly as responsible for the specific implications of our utterances as we are for their explicit factual claims". (12, 85) Of course, the kind of issue that is involved here has become clearer since the publication of Austin's *How to Do Things with Words* in which the concept

of “illocutionary force” was developed, and the work of others (especially Searle) in attempting to work out the significance of the speech act.¹⁴ Throughout Cavell’s article there is what he calls a “very heavy reliance on the idea of context” and a rejection of the idea that what has been said can be understood completely *independent* of a context. (16, 89) We shall return to this topic later.

(5B) The remarks above are also relevant to the “second mistake” which Fodor and Katz allege that Cavell has committed here. This is supposed to be the most egregious of philosophical errors, “an outright contradiction”, and the heavy machinery of the propositional calculus is introduced to drive home Fodor and Katz’s point. However, Fodor and Katz seem to have failed to notice that Cavell is talking about cases in which one is *entitled to say* S or T. Cavell claims that the first person plural form embodied in S makes a difference in the entitlement of a native and a non-native speaker to assert S. Fodor and Katz never discuss this contention of Cavell’s so they miss his point. Their conclusion, “Thus, Cavell is simply wrong when he says, ‘it is not clear what would count as a disproof of S.’” (63-64), is simply wrong. They say, “sufficient evidence for a disproof of T would constitute a disproof of S”, but this is what is at issue. (63)

It is not clear just how to schematize Cavell’s claim about S and T, though it is transparent that Fodor and Katz have misunderstood him. Cavell says that S and T “are true together and false together”. Fodor and Katz record this as

$$[(S \supset T) \& (T \supset S)],$$

and go on to note that this implies both

$$(\sim T \equiv \sim S)$$

and

$$(T \equiv S).$$

Why they list both is unclear, since ‘ $(\sim T \equiv \sim S)$ ’ and ‘ $(T \equiv S)$ ’ are equivalent (supposing that ‘ \equiv ’ is the usual biconditional).

Henson confuses things by saying that Fodor and Katz have mistakenly taken Cavell to hold that S and T are materially equivalent when in fact, says Henson, Cavell intends a stronger relation, one which is such that S and T don’t simply have the

¹⁴J. L. Austin, *How To Do Things With Words*, (Oxford, 1962), and John Searle, *Speech Acts*, (Cambridge, 1969).

same truth value but *must* have the same truth value.¹⁵ But this is the usual informal account of material equivalence. (To use Henson's examples, 'a Russian invented the telephone' and 'Raphael designed St. Marks Cathedral' are not materially equivalent—as Henson thinks—although 'a Russian invented the telephone if and only if Raphael designed St. Marks Cathedral' is true.¹⁶

The point: Whether the relation is

- (i) S and T are materially equivalent,
- (ii) $S \equiv T$, or
- (iii) S and T are true together and false together,

it does not follow that whatever confirms or disconfirms T similarly affects S.¹⁷ Nor does it follow that whatever counts as a disproof of T counts as a disproof of S. What Cavell is claiming is that the relation of being-evidence-for is a three-place relation holding between me, my statement, and (my) evidence for (my) statement. When I am *entitled* to my statement, then nothing can stand as evidence to it for me. Perhaps Cavell is wrong about this, but Fodor and Katz say nothing to show that he is.

Cavell's exposition may invite some misunderstanding. It seems clear Cavell is not to be read as holding either (i) or (ii), for T doesn't imply S, nor is ' $T \supset S$ ', true. One can't get from T to S in the way one can get from S to T, and this is obscured as soon as one takes 'S' and 'T' as abbreviations of, or variables for, statements. It is statements-in-context that count here. This much is clear from an attentive reading of Cavell's paper. But when Cavell says of S and T that "the identical state of affairs is described by both" he is easily misunderstood. He has already said that S, at least when one is struck with its *necessity*, is "about the concept of action *überhaupt*", but from the standpoint of one not entitled to S, T (for which he may have evidence) will seem principally to be about language. Or perhaps about someone's concept of action, not, perhaps, one's own. There is an asymmetry between one's relation to S and one's relation to T.

¹⁵Henson, *op. cit.*, pp. 53-54.

¹⁶Cf. W. V. O. Quine, *Methods of Logic*, (New York, 1959), pp. 46 ff.

¹⁷The alert reader will have noticed that there is no single reading of 'S' and 'T' which renders all of (i), (ii), and (iii) grammatical. None of the authors uses all three. Cavell uses only (iii), and he apparently takes 'S' and 'T' to be the names of sentences (or statements). Henson uses (i) and says that Fodor and Katz hold it. In fact Fodor and Katz use only (ii), though they may wish it read as (i). 'S' and 'T' appear unitalicized in both printings of Cavell's paper; they are italicized by both Fodor and Katz and Henson. It is, we trust, unnecessary in this paper to try to straighten this out.

Cavell, also, must be committed to the claim that if “sufficient evidence for a disproof of T existed”, then S would not be stated by native speakers (or would be stated only *extremely* rarely).

Incidentally, Fodor and Katz’s little debating point concerning S (and T) is wrong.

If we discover, as we do, that speakers of English say such things as “he joined the army voluntarily” (that is, he was not conscripted), then, since no implication of fishiness is involved, S and T are both shown to be false. (64)

No doubt speakers of English do say, “he joined the army voluntarily”, but it is clear that the implication of fishiness exists here. Fodor and Katz seem to believe that this phrase is equivalent to “he volunteered for the army”, which it isn’t. Even the latter is beginning to sound fishy, and “he enlisted in the army” actually gives the sense that Fodor and Katz are seeking. The fact that the phrase they suggest does not give this sense should have been obvious to them since they had to add a parenthetical phrase to explain what they mean (explain the fishiness). Cavell does quote Austin who says that we may join the army voluntarily, but although Fodor and Katz say that this example contradicts S, it doesn’t. It doesn’t because Cavell can maintain that the implication of fishiness is present when this statement is made (as, indeed, it is, as we’ve seen above).

(6) Next, Fodor and Katz attack Cavell by saying,

Perhaps Cavell has failed to notice . . . that there are indefinitely many statements which are clearly empirical, but which, like statements of type 1 and 2, one normally does not need empirical evidence obtained by special investigation to assert. p. 64

They then give a list of examples which are all first person singular and plural sentences. If Fodor and Katz had read Cavell’s paper carefully they should have known that he devotes a part of it to the general unique status of first-person claims. They even quote part of this discussion in the next paragraph. The fact that they don’t see that it is relevant to their examples seems fantastic. Fodor and Katz have distorted the issue by moving to first-person claims that are not like S or T (not about language), and then have ignored Cavell’s discussion of these other claims. Of course, Cavell has claimed a special status for

the first person plural statement about *language* which he is investigating—that is, that it is, in some sense, necessary. The necessity is what he is investigating. In “The Availability of Wittgenstein’s Later Philosophy” he says that the knowledge expressed in this kind of claim is “a knowledge of what Wittgenstein means by grammar—the knowledge Kant calls ‘transcendental’”.¹⁸ The point is not that Cavell is correct, but that Fodor and Katz’s criticism on this issue is totally without force because they have ignored what he has written.

(7A) The next point of attack for Fodor and Katz is Cavell’s claim that the request for evidence for a first person plural indicative is only competent when there is “some special reason for supposing what I say about what I (we) say to be wrong”. Fodor and Katz say that Cavell’s first mistake here is not recognizing that we sometimes demand evidence for statements “because we know of no reason why they should be true. Accepting Cavell’s condition on questioning statements and requesting evidence for them would make credulity a virtue and philosophy a vice.” (65) It’s interesting to try to think of an example of a situation in which a first person statement is made for which we demand evidence because we know of no reason why it should be true. Suppose someone walks up to me at a philosophical convention, holds out his hand and says “My name is John Jones”. I know of no reason why that should be true, or do I? Ought I to demand his driver’s license, draft registration, etc. as evidence in favor of his claim? To use Cavell’s terminology, is the request for evidence competent here? Obviously, it isn’t. We rely on the general presumption of truth-telling, and the obvious *general* competence of individuals to produce their own names. But perhaps this isn’t the kind of case Fodor and Katz have in mind. Suppose someone at a party says that he can speak twenty languages. Surely a request for some supporting evidence would be competent here. Isn’t this a case where I know of no reason that what he has said should be true? This seems to us much better described by Cavell—a case where there is some special reason for supposing that what has been said is not true. In this case the special reason is that I know that practically no human being can speak twenty languages. Most of the cases where we know of no reason why a first person plural statement about what we do or say (which are the statements Cavell is talking about) should be true, are cases where we have some

¹⁸Stanley Cavell, “The Availability of Wittgenstein’s Later Philosophy”, in Cavell’s *Must We Mean What We Say?*, p. 64.

reason for thinking that the statement is not true even though it could be true. If a statement about our language is true we have every reason to believe that it is true, since it is "our" language. Of course, the asking of questions where such asking is not justified is *the* characteristic vice of philosophy.

(7B) Cavell's alleged "second mistake" is that his view "entails . . . even if one's life depended on deciding correctly whether to accept a type 2 statement, it would not be competent to question or demand evidence for the statement unless one had a special reason for supposing it to be false". It seems odd that Fodor and Katz don't consider the threat of the loss of one's life as of sufficient moment to provide a special reason for wanting evidence for a statement. It's a bit hard to envisage the situation they have in mind, but let's try. Auric Goldfinger says that he'll kill you if you accept an incorrect type 2 statement, or if you reject a correct type 2 statement. He then produces S and asks whether you accept or reject it. Well, you might wonder why he's doing this. And, surely, someone insane enough to put you in this position may be trying to trap you; maybe it's just a kind of sophisticated sadism to make a philosopher squirm. Suppose someone comes up to you at a philosophical convention with a gun in his hand, aimed at your heart, fixes you with a glassy-eyed stare and says "My name is John Jones. Do you believe me?" Well, do you? Surely whatever you say will be prompted by a desire to avoid being shot. At any rate, any envisagible situation of this sort does seem to provide a special reason for being very careful about what you say. But perhaps this criticism was simply meant as a bit of humor by Fodor and Katz.

(7C) Fodor and Katz next say that Cavell's "third mistake" is to hold that we aren't often wrong about type 2 statements. Cavell, however, does not deny that errors can be made in type 2 statements and, indeed, he instances such an error in "Must We Mean What We Say?" It is true that there is often at least initial disagreement over the implications of philosophically interesting words like "good", "voluntary", "true", etc. Cavell does not, and does not need, to deny this. The interesting question is how can such disagreement be resolved—and Cavell does discuss an instance of a clash of native speakers about what we say. Fodor and Katz say that he avoids the essential problem of how one adjudicates such a clash, but actually Cavell distinguishes a number of different possible things we might want to say in the face of such a disagreement. (One kind of

question—about the uses of particular, specific expressions—is seen by Cavell to be susceptible of answer by undertaking the kind of empirical examination that Fodor and Katz seem to want.) Fodor and Katz misconstrue Cavell's example of the baker who uses 'inadvertently' and 'automatically' interchangeably. Cavell, at the end of a long, careful discussion in which he considers various ways a discussion about the use of 'inadvertently' and 'automatically' might go, says

It *may* turn out (depending upon just what the dialogue has been and where it has stopped) that we should say to the baker: “. . . the distinction is there, in the language . . ., and you just impoverish what you say by neglecting it. And there is something you aren't noticing about the world. (35-36, 105-106)

The Fodor and Katz response is that

From the fact that a speaker does not mark a distinction using the words standardly employed to mark it, it does not follow that what he *can* say is thereby impoverished. (67-68)

They then suggest that the baker may use other expressions to mark the distinction which is marked in Standard English by the use of 'inadvertently' or 'automatically'. This, however, is all beside the point because Cavell says that we may want to say to the baker that there is a distinction in the world that he is missing, but that point comes after one has tried to draw the distinction in other words (as Fodor and Katz suggest) and has *failed* to succeed in doing so.

(8) This leads to the final major point which concerns the correlations between distinctions marked within the language and distinctions in the world and whether one needs, in some sense, the former in order to mark the latter. This is an enormously complicated issue. However, we don't believe that Cavell has limited himself to saying that all perceptible distinctions are marked by single words. What Fodor and Katz call “constructible expressions” could perfectly well be accommodated within Cavell's general position. Then it would be the case that one might argue that a distinction which could not be formulated *in any way* within a particular language could not be made. Secondly, it is clear that a person's *means of communication* with another person are impoverished if he uses interchangeably, words which are distinct. The other major objection which Fodor and Katz make concerning this point is that it assumes

“that English is a philosophically privileged language with respect to the distinctions that it codes”. (68) But this is not quite the assumption. The assumption is that for any speaker, his own natural language is philosophically privileged for him in the distinctions that it codes. Cavell’s view certainly does not imply that the English speaker *is* missing the infinite distinctions that could be coded in other languages. His point is that the English speaker is *not* missing (or making) these distinctions, because the English speaker has a different relationship to English than he has to any other language. Of course, this does not imply that the English speaker can’t learn another language—only that learning another language may involve more than simply learning to translate our present language. We may need to learn to make more distinctions in the world.

(9) In a final footnote, Fodor and Katz suggest that Cavell, rather than concentrating on Mates should have considered “more tenable conceptions of empirical investigations in linguistics, for example, the conception implicit in Chomsky’s *Syntactic Structures*”. (71) Since this footnote is an attempt to associate their own claims with Chomsky’s high reputation it may be worth while to quote some recent reflections of Chomsky on empirical investigation in linguistics:

I believe that modern linguistics has real achievements to its credit, and that some of these do have relevance to philosophical questions. But it must be kept in mind that these achievements owe little to modern science and less to modern technology. The gathering of data is informal; there has been very little use of experimental approaches (outside of phonetics) or of complex techniques of data collection and data analysis of a sort that can easily be devised, and that are widely used in the behavioural sciences. The arguments in favor of this informal procedure seem to me quite compelling; basically, they turn on the realization that for the theoretical problems that seem most critical today, it is not at all difficult to obtain a mass of crucial data without use of such techniques. Consequently, linguistic work, at what I believe to be its best, lacks many of the features of the behavioural sciences. Nor is it obvious that the development of explanatory theories in linguistics merits the honorific designation “scientific”. I think that these intellectual constructions are nontrivial and often illuminating. However, apart from certain insights owed to modern logic and mathematics, there is no reason why they

could not have been developed many years ago. In fact, were it not for the dominance of certain empiricist assumptions to which I will return directly, I suspect that they would have been developed long before now and that much of what is new and exciting in linguistics today would be taken for granted by any educated person.¹⁹

III

In the last section we showed that Fodor and Katz's criticisms of Cavell are without force, in general because they simply ignore or overlook the claim on which rests much of Cavell's view. Here we attempt to go a bit deeper and try to get at the source of the misunderstanding, and make a tentative effort to amplify Cavell's thesis.

Throughout section II we had occasion to point out that Fodor and Katz's use of the notion of "empirical evidence" begs a question. Speaking roughly, they seem to have taken it as obvious that empirical evidence is needed for, and applies in the case of, any empirical question (no matter who speaks on the question), and that any matter about which one can be mistaken is an empirical matter. This much, we have said, simply ignores Cavell's claim that the notion of evidence has no application to certain kinds of first person statements one can make. (When a call for evidence is in order is an intricate question. To the extent that one looks to ordinary usage for guidance in philosophy, one would do well to look to Austin's discussion of evidence in *Sense and Sensibilia*.²⁰) But the way in which the notion "empirical" figures in the discussion is harder to make clear. Apart from the question of evidence, there seems to be a disagreement between Cavell and Fodor and Katz about whether one's statement S (if S is a statement) is an empirical statement, or about whether one's knowledge that S is empirical. In fact Cavell never uses the word 'empirical', but Fodor and Katz are playing fair in bringing it in, for Cavell has set the stage for its entrance. He says,

When (if) you feel that S is necessarily true, that it is a priori, you will have to explain how a statement which is obviously not analytic *can* be true a priori. (13, 85)

¹⁹Noam Chomsky, "Linguistics and Philosophy", in Sidney Hook (ed.), *Language and Philosophy*, (New York, 1969), p. 56.

²⁰J. L. Austin, *Sense and Sensibilia*, (Oxford, 1962), p. 123 and *passim*.

What is 'a priori' being opposed to, if not empirical? So, Cavell is denying that one's knowledge of S is empirical. But, as we've said, it's abundantly clear that Cavell thinks that S is *about something*. That will seem odd, confused, or contradictory if one takes *the* distinction to be "a priori/about matters of fact" (that is, if one takes 'empirical' in roughly Hume's sense). This is how Fodor and Katz take it. Indeed the whole thrust of their articles is that there can be no *a priori* knowledge of the truth of claims about "matters of fact". But there is a sense of 'a priori' in which, from the fact that 'p' is a priori nothing follows about whether 'p' is *about* something. This is roughly Kant's sense, in which 'p' may be a priori and *formal* or a priori and *material*. Cavell apparently takes S to be material, and he surely has Kant in mind when he says of statements like S, "they are instances (not of Formal, but) of Transcendental logic". (13, 85)

Whether this is an accurate or judicious use of Kant is hard to say (explicating Cavell is no picnic either), and perhaps it's unimportant. Cavell no doubt finds this allusion to Kant helpful, and so might anyone trying to find a way of accounting for the special status S seems to have. Kant is, even for his opponents—especially for his opponents—the premier author of such accounts. Still, calling upon this part of Kant (the Transcendental Logic) may produce as much confusion and misguided counter-argument as clarity. It suggests that what is being claimed for S is the status of synthetic a priori judgments. (And in fact it seems accurate to say that this is how the issue appears to Fodor and Katz: they seek to show (again) that there is no a priori reasoning concerning matters of fact.) But the reference to the Transcendental Logic is oblique, as Cavell makes clear a few pages later.

At this point the argument has become aporetic. "Statements about ordinary language" like S, T and T¹ are not analytic, *and* they are not (it would be misleading to call them) synthetic (just like that). Nor do we know whether to say they are a priori, or whether to account for their air of necessity as a dialectical illusion, due more to the motion of our argument than to their own nature. Given our current alternatives, there is no way to classify such statements; we do not yet know what they are. (16, 88-89)

So, we go too fast (over too familiar ground) if we class S as synthetic a priori. We suggested earlier that Cavell himself may have gone too fast in supposing that what we're trying to classify

is a statement. Whether the now familiar notion of a "speech act" (which was not in the literature at the time of the Cavell papers, and is still not a very clear notion) will help is hard to say, and this is not a place to go into the matter. In any case, some broader context than that of statements is required; for Cavell is not talking about the differences between S and T (just like that), but about the differences between the holding and making of S and T, about who *can* make S, and about what happens to T when it moves from one speaker to a differently situated speaker, about who can say what and mean what by what he says. The distinction between the motion of our arguments and our words is not rigid.²¹ Then let us have a look at a different account of necessity. There is another, less familiar theme in Kant which illuminates Cavell's claim about S in another way. To see it, let's return to the question of evidence.

Henson, in his discussion of the relation of evidence to each of a pair of "materially equivalent" statements, makes use of an example like this. If 'F' is 'I am in pain' said by Fodor, and 'K' is 'Fodor is in pain' said by Katz, then 'F' and 'K' are "true together and false together", but Fodor has no evidence for 'F', nor does he need any (nor could he use any). Henson's example makes his point, and is enough to refute Fodor and Katz. (It would be instructive to go to other first-person—third-person cases, comparing them with respect to evidence, knowledge, and truth, along lines begun by Austin: I see a pig/he sees a pig, I know/he knows, I promise/he promises, etc.) But such an example, which gets at the question of evidence-for-S-relative-to-evidence-for-T, deflects one from the core of the question of evidence for S.

If one thinks that S statements are typically made by ordinary language philosophers on the strength of too little evidence, what is the little evidence the philosopher is presumed to have? It must be

²¹Fodor and Katz seem aware that the idea of context is at work in Cavell's arguments. In "What's Wrong with the Philosophy of Language?" they even use the terms 'speech acts' and 'performatives' (though with reference only to Austin's "Other Minds"). They are rather impatient with this idea, holding that one need not be concerned with such "environmental conditions" because, to speak loosely, linguistic theory can proceed without taking them into account and, furthermore, they are not amenable to the formal treatment that theory requires. (See pp. 214-215 and *passim*.) Both claims seem a bit dogmatic in the context of that 1962 paper, and recent work by McCawley and Lakoff, among others, suggests that both may be false. Some samples of this recent work and references to more can be found in Jay F. Rosenberg and Charles Travis (eds.), *Readings in the Philosophy of Language*, (Englewood Cliffs, 1971), and D. D. Steinberg and L. A. Jakobovits (eds.), *Semantics*, (Cambridge, 1971).

When I ask whether an action is voluntary I imply that the action is fishy. (call this R)

And how can one get to S (which is R with 'we' for 'I') with nothing but R to go on? The proper way to get to S is to take R, which reports on me, and then check reports of others, either all of them or an acceptable sample, and add them, or do induction on them or some such. How else get from an 'I' to a 'We'? (Unless one takes it on oneself to speak for a silent majority.)

Cavell's view is that not only is R not inadequate evidence for S, but that R is not any kind of evidence for S. Then what underwrites the move from R to S? Nothing. There is no move. S is the way (the mode?) in which a native speaker holds R (and no one else can hold R).

In the *Third Critique* (section 9) Kant discusses (in an altogether different context) one's "movement" from an 'I' to a 'we'. It is not that one is inferring something, but that in a special case one says 'I' but speaks "with a *universal voice*".²² Kant is analyzing the difference between judging only that an object pleases one and judging that it is beautiful. The latter judgment, too, arises only from one's being pleased, but there is a way of taking pleasure in the act of judging an object which is expressed in the demand that all others should be pleased as well. "The judgment of taste itself does not *postulate* the agreement of everyone . . ." says Kant, ". . . it only *imputes* this agreement to everyone . . .".

Neither does S postulate an agreement. And so it has no grounds, no evidence; it is itself the condition for R.

Now the parallel is far from complete. In fact it is no real parallel. Judgments of beauty and S statements are different. In particular, Cavell calls S a "Categorical Declarative". That is, S neither postulates an agreement nor demands one; in a sense, it evinces a community. The most striking difference between S statements and pure judgments of taste concerns the ways in which they can be defeated. Both S statements and taste judgments can be wrong, but the means available for showing their wrongness to those entitled to make them are different. Kant's *Third Critique* is a much disputed, little read, and poorly understood work. We offer this much, though, as an interpretation of part of Kant's view: a pure judgment of taste can be mistaken, but there is no general schema for showing that one is mistaken.

²²Cavell himself has suggested the relevance to this question of Kant's "universal voice" in "Aesthetic Problems of Modern Philosophy", *op. cit.*, pp. 88-96.

Empirical evidence is irrelevant, as it is with S statements, but for a different reason. If, in fact, when we ask whether an action is voluntary, we don't imply that the action is fishy, then my S is wrong. If we don't in fact take pleasure in the act of judging x, nothing follows about my judgment that x is beautiful, for what I am committed to is not that we *do* but that we *should* take pleasure in that judging. Kant says nothing about how it might be shown that it's false that we should . . . About how to show directly that it's false that x is beautiful, either to another or to oneself, all he says is that it can be discovered that one is not making a pure judgment of taste. In these cases of mistaken judgments, which are not the only ones, the retraction of one's taste judgment follows recognition that it was not really such a judgment (instead it concerned the good or the perfect or the merely pleasant).

In the long run the similarities between S statements and taste judgments are probably more interesting and more profitably studied than the differences. To discover that one's judgment is wrong but still a taste judgment is very much like relinquishing one's entitlement to S. But there is no possibility of a counterexample to a taste judgment.

Things are different with S. First, there is a clear, simple sense in which an S statement, say Ryle's, remains one even though it's wrong. Second, it is shown wrong through something like a counterexample. We do say—we can imagine saying—"He made the gift voluntarily" and so Ryle is wrong. And Ryle sees that he was wrong. We cannot defeat taste judgments in this way.

So, our knowledge of our language is not quite like our "knowledge" (as Kant sees it) of art, and we must look elsewhere for further illumination.

Having moved from Kant's logic to his aesthetics, we conclude with an excursion into his ethics. In the *Grundlegung*, while eliciting the common human understanding of the relations to morality of duty and self interest, Kant sketches a situation. A dealer charges an inexperienced customer the usual, fair, price. It is his duty to do so, and it's also in his interest to do so. Does he deserve moral credit for doing so?

What is the effect or force of this example for one who has been saying either that moral credit is never due one who acts in a way commensurable with his own interest, or that it is always due when one does serve his own interest? How does it counter either claim?

Cavell says, "We know Austin's example counters Ryle's claims . . ." (6, 79) How? Not by showing that there is counter-evidence, not by proving something. One (Ryle) withdraws his S statement not merely because he is shown that others don't do, or say, such and such. Rather, I see that *I* don't say such and such. That is, it isn't that I give up S because my evidence for S, which was R, has been outweighed; my abandonment of S consists in my giving up R.

As with Kant's example of the ambiguous merchant, we don't have a counterexample in the sense in which a black swan is a counter to 'All swans are white'. The example is of nothing new; it is a reminder of an old, deep something.

Again, the analogy with Kant is only partial.

This thrashing around in Kant is understandable, we hope, if not excusable. As Paul Ricoeur has remarked, it is striking that Kant nowhere talks much about language and our knowledge of our language, but that is not inauspicious if, as Cavell says, ordinary language philosophy is not about language, but is about whatever ordinary language is about. We share Cavell's sense that Kant's efforts to elucidate what we hold, as it were, closest to us are the appropriate background for understanding at least some strains of ordinary language philosophy. We've tried to suggest some places to draw on.

MIDDLEBURY COLLEGE
UNIVERSITY OF CHICAGO