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V. WHAT WE SAY

RICHARD G. HENSON

SEVERAL years ago Professor Stanley Cavell[1] defended a view as to the status of a claim made by a native speaker about how he and his fellow native speakers talk—a view which has the welcome consequence that what "ordinary language philosophers" say about their language does not require them to leave their armchairs. This view has recently been attacked by Jerry A. Fodor and Jerrold J. Katz.[2] I shall not attempt a full summary of Cavell's paper or a full defense: I do not agree with all of it that I think I understand. But the arguments put forth by Fodor and Katz, while clearly and persuasively stated, seem to me to be often mistaken or inconclusive. I shall state their major arguments in the order in which they occur, numbering them consecutively throughout.

Cavell was concerned to show that Professor Benson Mates had been wrong about the methods necessary for determining "what we say"; and he was faced with a case, discussed by Mates, which saw Ryle and Austin making incompatible claims about our use of "voluntary" and "voluntarily." Ryle had said, with certain qualifications, that we apply these words only to actions which seem to be someone's fault.[3] Austin, on the other hand, had remarked that "we may join the army or make a gift voluntarily. . . ."[4] Cavell agrees that Austin's examples show that Ryle was mistaken. The main point at issue, though, is the logical, or epistemic, character of certain statements which one makes about his own language, and in particular whether one needs empirical evidence for such statements. Cavell writes:

> . . . native speakers of English . . . 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 . . . but in general, to tell what is and isn't English, and to tell whether what is said is properly used, the native speaker can rely on his own nose; if not, there would be nothing to count. (M, pp. 174–175).

Here Fodor and Katz offer two criticisms:

(1) What Cavell misses is the distinction between what a native speaker says . . . and what he says *about* what he and other native speakers say.

> . . . 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. (W, p. 60).

(2) In respect to the kind of knowledge one has of his own language, Fodor and Katz assert that there is no difference between its grammar and semantics on the one hand and its sound system on the other.

> . . . any argument showing that the native speaker has special license to statements about the syntax and semantics would show also that he is similarly licensed to statements of the analogous form about the sound system. But this constitutes a *reductio ad absurdum* of such an argument because, *inter alia*, 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?). (W, p. 61).

Point (1) represents the view of Fodor and Katz on the general issue at stake in their paper and mine. Some of our metalinguistic remarks are indisputably wrong; but I postpone general discussion of which ones, and how, and what to make of it. As to (2), it seems evident that on certain questions concerning the sound system, a

[1] In "Must We Mean What We Say?" *Inquiry*, vol. 1 (1958), pp. 172–212; referred to hereinafter as 'M.' This was a reply to Professor Benson Mates's "On the Verification of Statements about Ordinary Language," in the same issue. Both papers are now reprinted in *Ordinary Language*, ed. V. C. Chappell (Englewood Cliffs, Prentice-Hall, 1964).

[2] "The Availability of What We Say," *Philosophical Review*, vol. 72 (1963), pp. 57–71; referred to here as 'W.' Fodor and Katz are also concerned, in this article, with some of Cavell's remarks from "The Availability of Wittgenstein's Later Philosophy," *Philosophical Review*, vol. 71 (1962), pp. 67–93; referred to here as 'A.'

[3] *The Concept of Mind* (New York, Barnes and Noble, 1949), p. 69.

[4] In "A Plea for Excuses," reprinted in his *Philosophical Papers* (Oxford, 1961), p. 139.

native speaker should prove nearly infallible and on others not.[5, 6]

In Cavell's paper, a good deal hinges on the similarities and differences between two kinds of statements, typified by *S*: "When we ask whether an action is voluntary we imply that the action is fishy" and *T*: " 'Is *X* voluntary?' implies that *X* is fishy."

> ... though they are true together and false together, [they] are not everywhere interchangeable; the identical state of affairs is described by both, but a person who may be entitled to say *T*, may not be entitled 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. (M, p. 182; quoted on W, p. 62).

In his claim that statements *S* and *T*, though "true together and false together," are not everywhere interchangeable and not epistemically justified in the same way, Fodor and Katz claim that Cavell makes two mistakes:

(3) His first is "to suppose that, granting that *S* and *T* are true together and false together, anything whatever follows just from the fact that *S* and *T* are not everywhere interchangeable. ... No two morphemically distinct linguistic forms are everywhere interchangeable preserving all properties of context, *not even two synonomous versions of S*." (W, p. 63).

(4) His second mistake "consists of an outright contradiction," in that he (i) grants that *S* and *T* are "true together and false together," i.e., that they are (as Fodor and Katz choose to put it) materially equivalent; (ii) says that *T* is subject to empirical confirmation and disconfirmation; (iii) says that empirical evidence is irrelevant to *S*. But from their material equivalence it follows that "any evidence which disconfirms *T ipso facto* disconfirms *S* and that any evidence which confirms *T* likewise confirms *S*." (W, p. 61).

Point (3) is an *ignoratio elenchi*. The very passage which Fodor and Katz quote shows that Cavell does not claim that the important differences between *S* and *T* can be *inferred* from the fact that *S* and *T* are not everywhere interchangeable.

What Fodor and Katz intend in (4) is partly right and partly wrong. From the fact that two statements are materially equivalent it does not in general follow that what confirms or disconfirms one does the same for the other: what disconfirms "a Russian invented the telephone" does not disconfirm "Raphael designed St. Marks Cathedral," although these are materially equivalent; and both are equivalent to "three times three is twelve," to whose truth-value *no* empirical evidence is relevant. (I was so bewitched by Fodor and Katz on first reading that I needed my colleague, David W. Bennett, to point out this feature of confirmation and material equivalence to me.)

But this is perhaps unfair to Fodor and Katz: their argument is marred by the fact that they choose to represent Cavell's description of propositions like *S* and *T* as "true together and false together" in terms of material equivalence, while in fact neither Cavell nor they were concerned with propositions which are connected in so weak a fashion. The letters "*S*" and "*T*" are not variables in this discussion, but names—although they represent, informally speaking, any pair of an indefinitely large class of pairs of expressions any of which could have served in the discussion just as well as *S* and *T*. It is presumably this latter fact which tempts Cavell to speak of them as true together and false together—for strictly speaking, *S* and *T* must each be either true or false, not swinging hand in hand from truth to falsity and back. Very well: let us grant that Fodor and

[5] I am unable to assess the evidence (which, according to footnote 10 of W, p. 61, is contained in M. Halle's "Phonology in a Generative Grammar," *Word*, vol. 18 (1962), pp. 54–72) for the entailment claimed in the above quotation from Fodor and Katz. Assuming that they are right in this claim, I suggest that a distinction between aspects of our knowledge of the sound system analogous to the distinction I shall draw below in regard to our semantical and syntactical knowledge will meet their argument.

[6] It is not clear to me that "native speaker" is exactly the characterization which is needed here; having nothing better to offer, I follow Cavell. Surely *some* who are not (genetically speaking) native speakers would do as well; but of course generalizations about native speakers are not falsified if the same things can be said about some who are *not* native speakers.

Katz are thinking of pairs of expressions such that, within each pair, the members are not just materially equivalent, but are related as S and T are: i.e., are so related that they not only do, but must, have the same truth-value. Granted that they may not have intended to lay down a general principle about confirmation and mutual entailment—granted, that is, that they were not obliged to consider anything except the propositions S and T—I suggest that if Cavell is indeed guilty of a contradiction, then other pairs of expressions related as S and T are would generate a contradiction if the same things were said about them as he says about S and T; and I want to dispel the illusion that (4) is decisive by sketching a nearly parallel case in which a similar argument will be seen to fail. I apologize for the excessive familiarity of what I shall say about the parallel case.

Let U be my utterance "My tooth aches." Let V be someone else's utterance (on the same occasion) "Henson's tooth aches." I take it that U and V are "true together and false together"—i.e., that depending on the occasion of utterance, U will be sometimes true and sometimes false, and that V will always have the same truth-value as U. Fodor might have some evidence in favor of V; it would *ipso facto* be evidence (for Fodor) that I was telling the truth in saying U. But it would be evidence *for Fodor*, and for Katz and for any other similarly situated observer; I am *not* similarly situated. I submit that the entire passage which I have quoted above (from M, p. 182) would be perfectly correct if Cavell were speaking of U and V instead of S and T (and if other concomitant variations were introduced—reading "person with a toothache" for "native speaker," "dentist" for "linguist," etc.).

So much should help to dispel the illusion that (4) is decisive; it remains to show (i) that Cavell was right in what he said about S and T, not just that I am right about what could be said on similar lines about U and V; (ii) *how* the persuasive schema of (4) is mistaken. The former task must be attempted *ambulando*. The latter task then: the crucial weakness of that schema is in its omission of the fact that what confirms or disconfirms a given proposition depends in part upon the situation of the person to whom the "evidence" is presented. In saying this I am not confusing confirmation with success in getting someone to believe something. What is (for you) good evidence that I have a toothache is not just as a matter of fact irrelevant to the strength of my belief, because I have already made up my mind or have enough evidence without it—I have *no* evidence, rather than too much of it to pay any more attention, and I am logically debarred from treating that "evidence" as evidence *for me*. Similarly, in some cases I know something simply because I remember doing it, or seeing it, and what is genuine evidence for you that it happened is entirely irrelevant for me. Indeed, memory is fallible: *sometimes* I need evidence to corroborate what I (at least seem to) remember; but sometimes I do not. (See discussion of point (6) below.)

Now Fodor and Katz are right, in part: if T is proven to be false, and if S entails T, S is false; if T is proven to be true, and if T entails S, S is true. Similar things could be said of V and U; but it does not follow that evidence confirming V is evidence for *me* that my utterance, U, is true or probably so. Thus also for T and S.

So much, I am confident, is consistent with Cavell's view—but it includes a weighty concession to his critics. Neither Cavell nor they seem to me to be entirely right. When a native speaker says something like S, he does not normally say it on the basis of empirical evidence, much or little; but what if something like T should (at least seem to) be disconfirmed by empirical evidence? (I will henceforth use "S_n" and "T_n" as variables, representing any pair of statements related as S and T are.) There are several cases to consider:

(i) S_n (and thus T_n) may in fact be true, even though some evidence is uncovered which seems to count against them. This case is worth noticing only because it is worth remembering that disconfirmation is generally inconclusive.

(ii) The speaker may be ignorant of features of dialects other than his own, so that what he says about what "we" say will be incompatible in those respects with T_n unless T_n is restricted to that speaker's dialect. This case does not, I think, vitiate Cavell's argument: I know of no case in which differences of dialect have led to an S-like claim's being mistaken in a philosophically interesting way. *The Concept of Mind* is not weakened by Ryle's failure to notice such expressions as "... the pore gentleman was mental. ... For an 'ole hour, 'e went on something chronic." (A sample of "ordinary language" cited by Bertrand Russell.)[7] In the unlikely case that differences between dialects of the same language turn out

[7] In "The Cult of Ordinary Usage," *British Journal for the Philosophy of Science*, vol. 3 (1953), p. 305.

not to be inter-translatable, and to have some conceptual significance, this would be of great interest: but even this would not tend to show that information from a user of one of those linguistic-conceptual systems about his own system is mistaken.

(iii) It may happen that T_n is decisively shown to be false by the evidence which a linguist gathers. Could such evidence induce the native speaker to withdraw his S_n? Well, what is to count here as empirical evidence? When one does withdraw such a claim, it is often because someone has offered him a case of a certain description and asked him whether he (or "we" or "one") would be willing to ask or say so-and-so about it. Suppose, for instance, that I have noticed that to say "Jones is a capable fellow" is to say something good of him, to say that he can generally be expected to succeed at things at which he, and probably other people, want him to succeed. And suppose I say "We don't say that something is capable of something, unless we regard that something as good. We say '*liable* to err,' but '*capable* of success'." My generalization is of course false. Someone might get me to see that it is false by asking "What about 'I think he's capable of murder'?" Presented with this suggestion, I might very well modify my initial claim. Now have I been presented with *evidence* against that claim? I have not necessarily, on this occasion, (a) observed someone using (as distinct from raising a question about the use of) the expression, (b) been given any research reports on its use, (c) looked it up in Fowler, (d) remembered myself or another using it.[8] Presented with the example, I have realized that that is a perfectly proper use of the word, such as I or any other native speaker might employ. (See M, p. 174, second paragraph.) An outsider might have as evidence against my initial generalization either the fact that native speakers do use the phrase "capable of murder" with some frequency, or the fact that they report to him that they would be quite willing to use it, that it does not sound odd, etc. But *I* did not weigh any such evidence in coming to realize that I had been mistaken: I did not count my own nose.

A harder case remains. (iv) Suppose I have uttered S (or some sentence S_n) and am presented with statistical data against T (or T_n). Well, what shape is this evidence supposed to be in? Does it take the form "On such-and-such occasions, native speakers were found to discuss the question whether certain actions were voluntary but did not consider the action in question in any way fishy"? If so, I might be quite unmoved by the data, and sensibly so: I might wonder how the investigator was sure the actions were not considered fishy, who (the investigator?) introduced the word "voluntary," and so on. Or do the data take the form of more detailed specification of the circumstances of utterance of "voluntary"? In this case, they might serve me exactly as well as—*and in the same way as*—the examples that someone might present for my consideration in connection with the previous case, e.g., the phrase "capable of murder." Suppose, though, that the data gained by other people from close observation of the speech of my language community do in fact conflict with some statement S_n, and suppose that the specific counter-examples are described to me in full—suppose then that I do not budge, that I still claim that they are improper or do not make sense.

Now the fact that this is logically possible does not establish that it happens; and a more careful treatment of this whole problem would include detailed analyses of several kinds of cases in which it seems to or does happen. But I reluctantly concede that it probably does. If so, this is a serious weakness in my position; the most serious, I hope, because it seems very serious indeed. I offer for consideration two relevant facts and an argument; I shall discuss the matter further in connection with point (7) below. One fact is that some people simply speak more carefully than others, and some have a keener ear for the linguistic proprieties. I do not see the significance of this fact clearly enough to know whom it helps; but it is a fact. Another fact is that when philosophers are discussing and especially when they are arguing about the use of an expression, they are likely to be in a peculiarly bad condition for getting it right, and this for at least two reasons: they let their philosophical prejudices get in the way, and they suffer from sheer excess of concentration. Compare the distortion of one's normal perceptions which may occur when one stares at a familiar object—or repeats a familiar word over and over—until it becomes strange. Perhaps our trouble here comes from the fact that in such cases, as in arguments about what we say, the usual background fades away, with resultant distortion in that which is at

[8] Note the considerable difference between "remembering someone's *saying* 'capable of murder'," and "remembering *that* one can say 'capable of murder'."

the center of attention.[9] But I confess peculiar dissatisfaction with what I have been saying. Does it reduce to the assertion that when such a conflict between native speakers occurs, it is because either (a) they lack sufficient erudition and/or are not paying close enough attention to what they are saying or (b) they are too sophisticated and/or are paying too close attention? (I do not have a handy index of optimum sophistication and/or closeness of attention.)

The argument bearing on the point (as promised in the preceding paragraph) is this: in order to *understand* and thus in order to evaluate evidence against any statement S_n which I might make, I should of course have to rely on my knowledge of the language in which it is presented to me; and that language might be my own and could not be a language which I know better than "my own." (This is tautological.) If S_n concerned a locution with which I am sufficiently familiar, then, evidence which counted against my S_n might strike so deeply at my confidence in my knowledge of my own language that it would be simply impossible for me to accept: it would count as strongly against my understanding the sentences in which it (the evidence) was formulated as against my original claim.[10]

Cavell had said that it would be extraordinary if we were often wrong in statements about what we say, made in the first person plural present indicative; "they are sensibly questioned only where there is some special reason for supposing what I say about what I (we) say to be wrong; only here is the request for evidence competent." (M, p. 183). In this connection, Fodor and Katz accuse Cavell of several mistakes.

(5) He says that type 2 statements[11] can be sensibly questioned only where we have special reason to think them false, whereas Fodor and Katz point out that "we often question statements, and sometimes demand evidence for them, because we know of no reason why they should be true." (W, p. 65).

(6) They add:

> If we are only usually right, then we are sometimes wrong. But, then, it is *always* competent to request evidence to show that *this* is not one of those times. Whether in any particular case a statement is in fact questioned and evidence demanded is a matter of the positive utility of being right and the negative utility of being wrong. (W, p. 65).

(What Fodor and Katz presumably mean here is "Whether in any particular case a statement *ought* to be questioned. . . .")

(7) He holds that we are not often wrong in what we say about our own language; Fodor and Katz grant this for type 1 statements but not for type 2 statements, since a type 2 statement is "a kind of theory ... an abstract representation of the contextual features which determine whether a word is appropriately used." (W, p. 65). Fodor and Katz claim that even the literature of the ordinary language philosophers is rich in disagreements on type 2 statements—witness Ryle and Austin on "voluntary." They remark that Cavell does not discuss cases in which there is a flat disagreement on such a statement; and they claim that such disagreements cannot be resolved by reference to the relevant type 1 assertions, "since the same kind of conflict can arise there too." (W, p. 66).

Point (7) is the one which has great weight; I shall deal first with (5) and (6), which I think have little. As to (5), when I make a type 1 (or even a type 2) statement, I can hardly be in the position of having no reason to think that it is true: I am a component of the "we" whose practice I am reporting, and I have learned the practice from other members of that group. Point (6) seems weak, even outside the realm of the special kind of knowledge under discussion. I am sometimes wrong in my computations in elementary arithmetic; does it follow that I may now be wrong in saying three and four are seven, or that it is "competent" to require special investigation in this case? I am sometimes mistaken in matching

[9] Thus the strenuous efforts of Wittgenstein, Malcolm, Austin, and other analysts to minimize this danger by constant reminders of the circumstances in which we do use certain philosophically troublesome locutions.

[10] I am indebted to Robert C. Coburn for suggesting this consideration; whether my way of developing it is in harmony with his intentions is of course another question.

[11] Cavell had remarked that philosophers make three types of statements about ordinary language:

> (1) There are statements which produce *instances* of what is said in a language ("We do say . . . but we don't say ——"; "We ask whether . . . but we do not ask whether ——"); (2) Sometimes these instances are accompanied by *explications* —statements which make explicit what is implied when we say what statements of the first type instance us as saying ("When we say . . . we imply [suggest, say] ——"; "We don't say . . . unless we mean ——"). Such statements are checked by reference to statements of the first type. (M, 173).

I omit the third type because it seems to play no role in the subsequent arguments.

a given person with a given name; does it follow that I can now sensibly raise the question whether my own name is Richard, or my daughter's Elizabeth? My general objection to Fodor and Katz here may be put as follows: from the fact that one is sometimes mistaken in his assertions about members of a class of entities K, it does not follow that he may on just any occasion be mistaken in just any assertion about some member k of that class. For k may belong to a more or less clearly delimited subclass of K, concerning which subclass he is never mistaken, or never mistaken without there being, on the occasion of his mistake, special features which prompt doubt. (In delirium or amnesia, perhaps I *would* be mistaken about or ignorant of my daughter's name.)

The seventh point is the crucial one. Some consideration of the claim that a type 2 statement is a "theory" will help us see an important difference which Cavell seems to me to slight, though he is clearly not oblivious to it. The difference in question is between (i) generalizing as to what we mean when we say so-and-so, and (ii) recognizing cases *described in some detail* as ones in which, if one said so-and-so, he would be taken to mean or not to mean such-and-such. Utterances of both these kinds are generalizations; the former are more general, in that they abstract from the more or less detailed sketch of circumstances which distinguishes the latter. Since an account of circumstances can be indefinitely detailed, there is perhaps no difference in principle between the two kinds of utterances; but there is more difference in their epistemic status than one might expect from the difference in generality. The former kind is indeed, as Fodor and Katz claim, "a kind of theory," and of course one might over-generalize about it. The latter, if theory at all, is theory of a very special kind.

Suppose someone asks me whether, when I count, I say "five" before "six," or vice versa. If I take the question seriously at all, I suppose I could resolve it right there by counting past six, and finding which I did. Am I then *theorizing* about my own practice? Well, I don't say to myself "First I say 'one'; then I say 'two' . . ."—I just *say* "One, two . . ." This makes for an enormous difference between the status of my report on how I count and the status of my report, say, on how I tie my shoelaces. The latter action is perfectly familiar, but to my fingers rather than to my tongue. If I had to describe it, I should do it either through an effort of imagination or by actually tying them and reporting my actions step by step; but either way, the action described would be different from the act of describing it. But if asked how I count, I do not (need not) perform two different actions at all.

I shall turn in a moment to some of the relevant respects in which counting is an atypical use of language. But what I have tried to bring out is that in some circumstances, we tell (or show) how we would perform some linguistic act simply by doing it. And when we are presented with a suitably detailed sketch of a situation and asked what we would say, or what we would imply, suggest, or whatnot, in saying it, we are very close to that kind of case. Thus the vast difference between *this* sort of question about how we talk, and questions about what is *generally* meant by the use of a given expression. It is a familiar fact that we can generalize after candid and careful reflection and be wrong, even when the materials which should have shown us that we were wrong are in some sense accessible to us, (sometimes) in memory. "I don't believe I have ever known of a star football player who was also an outstanding middle-distance runner." "But how about Ollie Matson?" Of course: once reminded, I realize that I know, and in some sense have known all along, that Matson was both a star football player and an outstanding quarter-miler, thus an outstanding middle-distance runner. No research was needed to persuade me that my remark was wrong, no expert testimony; simply a reminder. I do not mean to suggest that this item of knowledge, temporarily unaccessible to me, was not empirical; I am trying rather to bring out the fact that we sometimes generalize incorrectly even when the knowledge we need to show our mistake is all but immediately available—available, so to speak, for the asking. It is abundantly clear that the same sort of thing happens when we make type 2 statements. The moral is that the proper cure for such mistakes in type 2 assertions is through "assembling reminders" consisting of detailed accounts of cases. (See footnote 9.)

We are hardly through with the question what kind of theory a type 2 statement is, however. Even in the case of counting, one might claim that I must make several empirical assumptions before I can get much good out of it. For the question was not just "Will you on this occasion say 'five' before 'six'?" but "*Do* you (regularly) say. . . ." Am I not then *assuming* that what I did on that occasion was typical of my general practice? And is it not an empirical question whether my practice accords with that of my fellow native counters?

But how seriously can such questions be taken? Sometimes they can be seriously raised—with a child who has not yet quite mastered counting, with a person suspected to be suffering from aphasia. . . . But a community in which people counted idiosyncratically—in which each man, in counting, "had his little ways"—would be a community in which *counting* did not take place. (Can you play chess without the *moves*?) It is an empirical question whether we do have such a practice, but this is not the issue. It is an empirical question whether, on a given occasion, I am in some pathological state which prevents me from counting properly. But we are not here concerned with pathological counters; nor are philosophers of ordinary language concerned with pathological speakers.

Counting, though, is a use of language which is peculiarly favorable to my views. It lacks border-line cases, eccentricities which are only perhaps errors, etc. It is almost unique in that there is, at each step in the process, exactly one right number and so (except for minor elasticities, as between "one hundred and one" and "one hundred one") exactly one right word or phrase to use. A similar situation prevails in respect to certain religious and legal formulae; but for the most part, there will be several different ways of saying whatever one wants to say in a given situation; and occasionally there may be no standard way of saying it. But still, though the rules are vastly more complex and flexible, these other uses of language are governed by rules, and the meaningfulness of an expression *consists in* its conformity to those rules.

The familiar "game" analogy will be useful for exposition. One who plays a game which is moderately complex and highly organized must know the rules and a good deal of its strategy and tactics. There may be *some* rules with which he is unacquainted, although of course such ignorance tends to put him at a disadvantage. There are also likely to be occasional situations not covered by the rules, such that only ambiguous (if any) guidance to player and official can be gained from the rules. But (in the moderately complex and highly organized games to which these remarks are limited) it must be very rare that a player is ignorant how or whether a rule applies to a given situation. Otherwise, he would simply be unable to play the game, and his incapacities would quickly become evident to the other players. One who plays the game often and fairly well knows and can say how "we" play it and does not need to take surveys or (except in especially out-of-the-way cases) consult the rule-book. What a beginner, or any outsider, learns as he comes to the game is empirical in character; the rules might have been different, and of course there are official bodies which make certain changes in the rules of many games. But we are talking about an experienced player of the game and the epistemic status of his reports on how it is played.

The analogy to our uses of language seems to me to be close, differing however in at least these respects: (i) The rules of a (complex, highly organized) game are likely to be more strict and inflexible and comprehensive than the rules of language, deliberately designed to cover any situation the rule-makers can envision. Innovation is often possible in language without prior notice, so to speak, but in the rules of games it bespeaks bullying or chicanery. In this regard, the game analogy perhaps makes my case look better than it deserves; but in the following two respects, the analogy makes my case look weaker than it deserves. For the rules of most of our "language games" differ from those of other games also in that (ii) the "rule-books" for language are not used in the same way as those for games; in particular, a dictionary or a grammar is to be tested by its fidelity to the practice of the speakers, instead of violations being authoritatively established as such by the fact that a player has gone against the rule-book, as in a game. And (iii) many of the rules of games impose what might be called "external impediments" to a player's achievement of his goals, which, of course, are also normally specified in the rule-book. (Given that the object of a football player is, at a given moment, to score a touchdown, and granting that some of the rules specify what is to count as a touchdown, there are other rules which prohibit certain kinds of blocking by his teammates, prohibit him from throwing a second forward pass in a single play, prohibit him from "hurdling" a defender, and so on. I call these "external" impediments, because they could be changed without reconstituting the game, without affecting its basic objectives or strategy. And of course such rules are changed from time to time, often to maintain an interesting balance between attack and defense in the game. But generally speaking the rules of language are not of this sort; one does not normally try to achieve certain things in the use of language, feeling its rules as impediments; it would not often make sense to *wish that the rules were different*, or that one could suspend them, so that one could achieve one's ends more readily—as it often would in a game. It is exactly through fidelity to these rules that one does achieve what he does, in most uses of language; it is through the common understanding of the rules that it becomes clear to one's listener what moves in the *language* game are being made.)

These differences—(ii) and (iii)—between the role of rules in such games as football and in our use of language, seem to me to tell strongly in favor of my position. From (ii), we see more clearly that the "native player" of a language-game is normally one of the collective arbiters of correctness, superior to any rule-book; from (iii), we can perhaps see more clearly that in the mastery of a language, one's knowledge of the common rules is not necessary merely for engaging in the activity properly, or elegantly, or efficiently, but for his engaging in it—making and appreciating its moves—at all. (Much of what I have said would be mistaken if one interpreted, e.g., frightening or amusing people as "uses of language" in this contest. These and many other things can sometimes be done without following any linguistic rules at all. See—of course—Austin's *How To Do Things With Words* (Cambridge, Mass., 1962).

Fodor and Katz next offer a battery of criticisms of what Cavell says about a case in which there appears to be a significant difference between the ways in which a pair of native speakers speak. Cavell asks us to suppose that we become convinced that someone (a baker) uses the words "inadvertently" and "automatically" interchangeably; he claims that this does not prevent someone else (a professor) from saying that when "we" use the one word, we mean something different from what we mean when we use the other; and the professor is entitled also to say to the baker "the distinction is there, in the language (as implements are there to be had), and you just impoverish what you can say by neglecting it. And there is something you aren't noticing about the world." (M, p. 200).

Fodor and Katz point out that Cavell assumes without argument that the case is one in which the baker's use is idiosyncratic, i.e., that we already know that there is a difference in meaning between this pair of words. Cavell's discussion of it cannot be expected to throw light, then, on the difficult questions we have just been discussing, concerning the possibility of conflict between native speakers, or between the reports of one of them and the results of empirical study of their practice. But they offer a doubtful argument against Cavell's claim that the baker's speech must be impoverished.

(8) It may be the case that English contains expressions exactly synonymous with "automatic" and "inadvertent"—indeed, they claim that "there are" such expressions "which can be constructed in English." (W, p. 68). (I find this claim puzzling: *are* there such expressions, or is it only that they can be "constructed"?) But if there are such expressions, they say, the baker may of course use them to mark the distinction(s) which others mark by using "inadvertent" and "automatic"; so his speech is not necessarily impoverished.

Well, one who misses a distinction between such a pair of words *may* notice, nevertheless, the difference(s) which they mark; and he *might* be able to "construct" some other expression to mark the difference in question. (A Bushman who lacks words for numbers over five *might* be able to tell that five groups of four dingoes total fewer dingoes than three groups of seven.) But at what stage of discourse will the baker's rectification prove effective? When he speaks to other people? How easily will they find out (a) that he does not distinguish between the meanings of the two words, and (b) which of them he uses as the other one ought to be used (supposing, that is, that he does use *one* of them correctly and the other one like it)? And—a far harder question—what about the impoverishment of his understanding of what others say? He will not "translate" what they say into his own idiom, because he does not realize that translation is necessary. The baker's insensitivity may not lead *him* into any philosophical difficulty, because he may not engage in philosophical debate; but the remarks of Fodor and Katz suggest that they may be thinking of meaning as a private mental activity. If it were that, one could mean what he chose to mean by his words, or perhaps one could simply *mean*, without bothering to use words; although he could still not simply *understand*, without listening to and discriminating the words of others. But if we are disabused of that error, it is hard to avoid the conclusion that the baker's speech and especially his comprehension of the speech of others is seriously impoverished.

As to Cavell's allegation that the baker fails to notice something about the world, Fodor and Katz offer two objections:

(9)

> First, it is simply false that we have distinct non-synonymous words for each distinction we notice. . . . Hence, from the fact that we do not have distinct words to mark a distinction, nothing follows about whether or not we notice that distinction. (W, p. 68).

And (10), even if it were true that the baker is failing to notice a distinction marked by this pair of English words, this is philosophically unimportant unless we assume that English "is a philosophically privileged language with respect to the distinctions it codes." (W, p. 68). Many natural languages code distinctions which English does not, and *vice versa*; and there are innumerable differences which could be but are not coded by any natural language. If it were the case—as they have argued it is not—that a speaker necessarily misses whatever distinctions are not coded in his (perhaps partly idiosyncratic) lexicon, then Fodor and Katz point out that every speaker of any natural language would be missing not only every distinction coded by other languages and not his own, but also the innumerable differences which are not marked by *any* natural language. The accusation that the baker is not noticing something about the world is thus "completely trivialized." (W, p. 69). It does not follow from all this, according to our authors, that the baker *cannot* be making some philosophically significant mistake in his idiosyncratic use of these words.

> What these criticisms do show is that one cannot establish that a philosophically significant error has been made *simply* by showing that someone has failed to draw a distinction coded in English. Moral: showing that one ought to draw a distinction is not something that can be done just by appealing to the way speakers in fact talk. This takes doing philosophy.
>
> This mistake of inferring "ought" statements about distinctions from "is" statements about what speakers say deserves the name "the natural language fallacy." The general philosophical importance of this fallacy is this: once the natural language fallacy has been recognized, it becomes necessary to raise seriously the question of the utility of appealing to what we ordinarily say as a means of resolving philosophical disagreements. (W, pp. 69–70).

Fodor and Katz are of course right that we notice many differences not "coded" by pairs of words in our language, and that many other languages code some of these and miss some of the differences which are coded in English. They are presumably well aware of the fact (and this is perhaps what they allude to in their remark about "constructing" expressions) that the lack of a single word in one natural language which is exactly synonymous with a single word of another by no means establishes that the speakers of the former cannot notice or give expression to the concept carried by that word in the latter. (See their references, note 27, W, p. 69.)

But two considerations occur to me in favor of Cavell's remark that the baker would be missing something "about the world."

(i) In reply to the charge that Cavell's argument is "trivialized," let us suppose that someone reproaches me for being a teacher when I might make substantially more money, say, in real estate; I reply "You reproach me for not making a few thousand dollars more a year? How trivial that would be in comparison with the billions of dollars I should still not be making—not to mention the francs, piastres, pesos, and the untapped wealth which no one is yet making." I acknowledge that the analogy is not quite fair and the reply partly (perhaps forty per cent) facetious. Consider then any scientist who abandons a research project because what he can hope to learn is as nothing to what he will still be ignorant of. In short, to notice something worth noticing is to do something worth doing, even though one cannot notice everything worth noticing.

(ii) What I said in connection with the "impoverishment" charge applies to the argument I have numbered (9) above. Here I add only that Fodor and Katz may be right that "from that fact that we do not have distinct words to mark a distinction, nothing follows about whether or not we notice that distinction," although I should agree with Roger Brown[12] that the presence of a lexical clue in a given language (e.g., a word for a particular kind of snow) probably increases the "cognitive availability" of whatever that word characterizes (e.g., the difference between that kind of snow and other kinds). But where there *is* a distinct lexical clue provided by the vernacular and a native speaker fails to distinguish between the relevant expressions, it seems a plausible assumption that he *is* failing to notice the difference. That Bushman whose language has no numbers above five cannot be assumed to be incapable of telling the difference between a pack of eight dingoes and a pack of twenty; but if one of us (whose language contains the words for five

[12] *Words and Things* (Glencoe, Free Press, 1958), pp. 235–241.

and twenty and lots of other numbers) never used different number-words for different-sized packs over five, there might be reason for suspicion.

The real importance of the issues discussed in the last few paragraphs presumably lies in their bearing on "the natural language fallacy": but our authors' enunciation of the fallacy is so brief that I am (almost) at a loss what to say about it. I take it that they might be paraphrased thus: it is a mistake to think that the distinctions which are in fact coded by a given natural language are the ones, and the only ones, which one ought to notice. (This is not the only sense which their words might be given: I hope they do not mean "What a word does mean is irrelevant to what it should (be used to) mean.") If I interpret them correctly, they are clearly right in one part of this conjoint claim: but neither Cavell nor anyone else has said that such distinctions are the *only* ones which one ought to notice. (See—if this requires any support—Cavell's first full paragraph on p. 205.) But I am at a loss to see what reason could be given for denying that one ought to notice the distinctions which *are* coded in one's own language. To say they are coded in the language is to say that they are marked in the linguistic practice of those who speak the language, not just recorded in a lexicon which may be obsolete or pedantically over-refined, or whatnot. Of course one does not on every occasion want to make every distinction for which the language offers scope (if indeed that is a coherent suggestion). But in using a natural language, we are not obliged thus to use it to the hilt, so to speak; and to use it correctly, we must mark the distinctions coded by such parts of it as we are using.

Finally, Fodor and Katz consider Cavell's remark that

> such questions as "What should we say if . . . ?" or "In what circumstances would we call . . . ?", asked of someone who has mastered the language . . . is a request for the person to say something about himself, describe what he does. So the different methods [of determining how we talk] are methods for acquiring self-knowledge. . . . (A, pp. 87–88).

(11) Granting that the knowledge in question is "in *some* sense self-knowledge," Fodor and Katz remark that "this has no implications for the methods we can employ in discovering such knowledge, since the knowledge we gain in correctly describing human physiology is also in *that* sense self-knowledge." (W, p. 70). And in a most telling passage:

(12)

> any facet of a speaker's use of English that is not shared by other speakers is *ipso facto* not relevant to a description of English. It is perhaps Cavell's failure to grasp this principle that has led him to suppose that some special privilege accrues to statements we make about our language in the first person plural present indicative. (W, p. 70).

In the last quotation but one, Cavell speaks of the respondent as being asked to "describe what he does," and this language seems appropriate. Speaking is something we *do*, not something which happens to us or in us; we sometimes choose our words deliberately, and we seldom say what we say unintentionally. None of us is today so innocent as to think that the concept of intentional action is easy to characterize, or that what we do intentionally is *ipso facto* easy to report accurately or even honestly; but I cannot take seriously the suggestion that it is "inaccessible" in the degree and manner in which the facts of physiology are.

What I have said above, about counting and the rules of games is the rest of my answer to point (11) and most of my answer to (12). I have admitted that our use of words is very seldom as strictly uniform as our use of the numerals in counting; but it must also be admitted that to speak a language just is to speak it, with very minor aberrations, as the other members of the linguistic community speak it. The problem of dialect is certainly important here, not only in that some speakers will be familiar with special technical vocabularies unknown to others, but also in that people at one level of education will use correctly words which people at a different level will sometimes use incorrectly. But it cannot be too heavily stressed that it is not this kind of difficulty about "how we talk" that contributes to philosophical error. If a native speaker says "When we call something 'precious', we mean . . ." it is possible that he will be unfamiliar with the sense of the word in which a drama critic describes a performance as "perhaps somewhat precious." But this is simply not the kind of problem that causes trouble for philosophers, whatever it may do for lexicographers. The disagreement over "voluntary" did not arise from this sort of ignorance; and no extravagant erudition distinguishes the users of such words as "know," "see," "good," "prove," "true," "think," "mean," "explain," or "faith." Our use of such words could not, in general, be any more idiomatic than our practice of making change or keeping score in a game. I am looking at one side of the coin which Fodor and Katz see from the other side when they say that "any facet of a speaker's use of English that is not shared by other speakers is *ipso facto* not relevant to a description of English." Fair enough; and my side of the coin reads something like "One who moves his knight like a bishop is not accepted as a chess player." (Inscriptions on coins must be brief and unqualified; some of my qualifications and explanations are in preceding parts of the paper. A large question which I have not tried to answer except partially and negatively is: What deviations from the common linguistic practice are philosophically significant? I shall try to deal with this question in another place.)

How important is this dispute about the epistemic status of our knowledge of our own language for the "ordinary language philosophy"? None of the parties to this discussion has suggested that we cannot by *any* means find out what we say, or what we mean in saying it; and is this not enough to enable the ordinary language philosophy to bear whatever burdens it must? We can answer only tentatively pending a full account of the nature of those burdens; but I am inclined to agree with Mates, Cavell, Fodor, and Katz that the present dispute is of great importance.

Cavell has done much to bring out this importance, in passages not discussed by Fodor and Katz or by me. More should be said about this than he said or than I shall attempt here. For the present: the "oppressive" effect of the ordinary language philosophy which Cavell mentions (M, p. 172) comes partly from the fact that, with exceptions and qualifications which have been dealt with here only partially and skimpily (I have said nothing about poetic deviations, for instance), it tells us that we must mean what we say, i.e., what *is meant* by one who utters the words we utter.[13] (I do not pause to argue this now familiar point here.) Any private intention of meaning such-and-such in using a form of words which is not accepted in the practice of the community as an appropriate bearer of that meaning is irrelevant to what is meant. But such an account of meaning can be true only if our knowledge of what we say and what we mean in saying it is—except in very special cases—immune to refutation by empirical evidence about how we talk.

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[13] One may say "Pardon me" and in some sense *mean* "You are very rude to stand for so long in my way"; and in other ways too numerous to deal with here, my statement requires expansion and qualification.