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The Availability of What We Say

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## THE AVAILABILITY OF WHAT WE SAY<sup>1</sup>

But now, as we conclude, methinks I hear some objector, demanding with an air of pleasantry, and ridicule—"Is there no speaking then without all this trouble? Do we not talk, everyone of us, as well unlearned as learned; as well poor Peasants, as Profound Philosophers?" We may answer by interrogating on our own part—Do not those same poor Peasants use the Lever and the Wedge, and many other Instruments, with much habitual readiness? And yet have they any conception of those Geometrical Principles, from which those Machines derive their Efficacy and Force? And is the Ignorance of these Peasants a reason for others to remain ignorant; or to render the Subject a less becoming Inquiry? Think of Animals, and Vegetables, that occur every day— of Time, of Place, and of Motion—of Light, of Colours, and of Gravitation—of our very Senses and Intellect, by which we perceive everything else—That they are, we all know, and are perfectly satisfied—What they are, is a Subject of much obscurity and doubt. Were we to reject this last Question, because we are certain of the first, we should banish all Philosophy at once out of the world.—JAMES HARRIS

**I**N TWO recent articles, "Must We Mean What We Say?" and "The Availability of Wittgenstein's Later Philosophy"<sup>2</sup> (to which we shall refer as *M* and *A*, respectively), Professor Stanley Cavell has set forth his position on the relation between the claims Oxford philosophers make about ordinary language and the methods and results of empirical investigations of ordinary language. These articles are important because they represent a viewpoint that is widely held by current philosophers—widely held but rarely made explicit. Cavell is surely right when he says that the conflict about the nature of our knowledge of ordinary language "is not a side issue in the general

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<sup>2</sup> Stanley Cavell, "Must We Mean What We Say?," *Inquiry*, 1 (1958), 172-212; and "The Availability of Wittgenstein's Later Philosophy," *Philosophical Review*, LXXI, (1962), 67-93.

conflict between Wittgenstein (together with, at this point 'ordinary language philosophy') and traditional philosophy; it is itself an instance, an expression of that conflict."<sup>3</sup> The position Cavell advocates in *M* and *A* seems to us, however, to be mistaken in every significant respect and to be pernicious both for an adequate understanding of ordinary language philosophy and for an adequate understanding of ordinary language. In the present paper, we seek to establish that this is in fact the case.

In *A*, Cavell's main concern is to expose the inadequacies of David Pole's treatment of Wittgenstein's views on language. In the course of the exposé—especially in the section entitled "The Knowledge of Our Language"—Cavell presents the substance of his conclusions about how we know about our native language. But since in *A* the exposé takes precedence over the exposition, we find little there in the way of argumentation for these conclusions. The arguments are found in *M*. *M* consists of an extensive investigation of the availability of our knowledge of our language, and we shall concern ourselves primarily (but not exclusively) with *M*.

Cavell begins by distinguishing three types of statements philosophers make 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 *explicitations*—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. (3) Finally, there are *generalizations*, to be tested by reference to statements of the first two types.<sup>4</sup>

Cavell concerns himself with the question of the justification of statements of types 1 and 2 exclusively. Since the justification of type 3 statements is entirely a question of the degree to which they receive support from statements of types 1 and 2, it need not be considered independently.

Cavell selects as a paradigmatic example of a type 1 statement Austin's remark, "Take 'voluntarily' . . . : we . . . may make a gift voluntarily." This Cavell takes to be material mode for "We say, 'The gift was made voluntarily.'" As a case of a type 2 statement, Cavell chooses Ryle's remark, "In their most ordinary employment 'voluntary'

<sup>3</sup> *A*, p. 85.

<sup>4</sup> *M*, p. 173.

and 'involuntary' are used . . . as adjectives applying to actions which ought not to be done. We discuss whether someone's action was voluntary or not only when the action seems to have been his fault."<sup>5</sup>

Cavell recognizes that these statements of Austin's and Ryle's go counter to one another: that there is a disagreement here that needs to be resolved. 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.

According to Cavell, though Ryle is wrong about the use of "voluntary," he is not wrong in the way that a scientist is when the scientist asserts a false hypothesis. That Ryle is wrong is, on Cavell's view, a fact for which we do not need and could not have empirical evidence. It is through an investigation of the character of Ryle's error that Cavell approaches the general question of the relation between empirical studies of language and the claims ordinary language philosophers make about language.

Against the contention that Ryle is not entitled to his generalization about "voluntary" because it is unsupported by empirical evidence (or because it conflicts with empirical evidence), Cavell replies:

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 . . . 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.<sup>6</sup>

Thus Cavell argues that Ryle and other native speakers are entitled, without appeal to 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. As Cavell puts it, "for a native speaker to say what, in ordinary circumstances, is said when, no 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."<sup>7</sup>

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<sup>5</sup> *M*, p. 174. Actually, Cavell did not choose these examples himself but took them over from Mates against whom Cavell's arguments in *M* are directed.

<sup>6</sup> *M*, pp. 174-175.

<sup>7</sup> *M*, pp. 175-176.

This argument of Cavell's is, however, a *non sequitur*. Cavell 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. What Cavell misses is the distinction between what a native speaker says (the utterances he produces in the course of speaking) and what he says *about* what he and other native speakers say (the metalinguistic comments he makes when the reflective mood is upon him). There can be no doubt but that most (though definitely not all) of the utterances of a native speaker are utterances of the speaker's language. *This truth is* guaranteed by the truism that a natural language is what a native speaker of that language speaks. However, the statements that a native speaker makes about his language, his metalinguistic claims, need not be true in order for the linguist to have noses to count. 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.<sup>8</sup>

Cavell does admit that there are some questions about a natural language which require empirical evidence to answer them: these include questions about the history of a language, the sound system, and special forms in the morphology of a dialect.<sup>9</sup> But to distinguish all of these as areas of linguistics to which empirical evidence is relevant, while setting grammar and semantics apart as areas to which empirical evidence is not, is simply to make a distinction without a relevant difference. 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 the native speaker who is philologically naive 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

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<sup>8</sup> The claim that the linguist must assume the truth of metalinguistic statements by native speakers in order to describe their language correctly is indefensible. Metalinguistic statements by native speakers appear in the linguist's corpus of the language, but this proves nothing about their truth because the corpus contains both true and false statements without discrimination. If the linguist had to separate the truths about the language from the falsehoods before he could begin to describe the language, he would have to know a very great deal about the language before his descriptive work had even begun.

<sup>9</sup> *M*, p. 175.

its syntax and semantics.<sup>10</sup> Thus, Cavell's statement that the native speaker's claims about the sound system of his language are empirical is inconsistent with what Cavell says about the native speaker's claims about the grammar and semantics of his language. Conversely, 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?).

Cavell's explanation of how Ryle went wrong, even if it is wholly correct, fails to show that Ryle's mistake is not an out-and-out empirical error. Cavell's point is that Ryle specifies too narrowly the condition for applying the term "voluntary." On Ryle's account, "voluntary" can be applied only to "action [which] seems to have been . . . [someone's] fault."<sup>11</sup> But, as Cavell notes, such actions are only special cases of actions properly called voluntary. Giving the neighborhood policeman a thousand dollars for Christmas instead of his usual bottle may be intelligibly described as voluntary, though no moral issue need be involved. Thus, Cavell concludes that the proper application of "voluntary" is subject to the condition "that there be something (real or imagined) fishy about any performance intelligibly so characterized"<sup>12</sup> and that Ryle's mistake is to have formulated the condition in such a way as to leave out a large class of voluntary actions. But this explanation does not show Ryle's mistake to be non-empirical, because many empirical mistakes are of just this form. Consider a biologist who asserts the generalization that all reproduction is sexual. He leaves out a large class of cases of reproduction, as, for example, fission, budding, and fragmentation. Thus, the biologist, like Ryle, errs by construing a condition too narrowly. The biologist takes the condition for the term "reproduction" to be

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<sup>10</sup> That is, what the speaker knows in each case are the general rules which structure the language. 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. Moreover, it is implicit in this article that the content of the latter rules must be stated in part in terms of phonological constructs. For an earlier but more detailed treatment of the phonological component of a grammar cf. M. Halle, *Sound Pattern of Russian* (The Hague, 1959).

<sup>11</sup> *M*, p. 176.

<sup>12</sup> *M*, p. 177.

the production of offspring by sexually distinct parents. Since such uncircumspect generalization is a typical pattern of error in empirical science, Cavell's explanation cannot by itself free Ryle from the onus of empirical error.

In order to show that the philosopher who proceeds from ordinary language is entitled without empirical investigation to assertions of type 2, Cavell says we have to explain three things: what kind of assertions type 2 assertions are; when they should be said, and by whom; and what should be meant in saying them.<sup>13</sup> In particular, Cavell wishes to derive from the answers to these questions the principle that when a type 2 statement is true it is necessarily true.

Cavell holds that type 2 statements are not analytic. Since, however, he wishes to argue that they are not empirical either, he ascribes to them the status of truths of "Transcendental Logic." In order to justify conferring this status upon them, type 2 statements must be distinguished from certain merely empirical statements which are remarkably similar to them. First, there are assertions like: "When we ask whether an action is voluntary we imply that the action is fishy."<sup>14</sup> This is simply a type 2 statement (call it *S*). Second, there are assertions like: "'Is *X* voluntary?' implies that *X* is fishy."<sup>15</sup> For reasons that will be made clear presently, this is not a type 2 statement but merely an empirical one (call it *T*). Concerning the relation between assertions like *S* and *T*, Cavell says the following:

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.<sup>16</sup>

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<sup>13</sup> *M*, p. 181.

<sup>14</sup> *M*, p. 181.

<sup>15</sup> *M*, p. 182.

<sup>16</sup> *M*, p. 182.

Since this is Cavell's main argument for claiming that native speakers need no empirical evidence for statements like *S*, we must examine it in detail.

Cavell's first mistake 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. In particular, none of the differences in status that Cavell finds between *S* and *T* follow. No two morphemically distinct linguistic forms are everywhere interchangeable preserving all properties of context, *not even two synonymous versions of S*. Since Cavell conspicuously fails to specify the properties of the context which must be preserved in such substitution (fails to answer the question: interchangeable preserving what?), what he says about *S* and *T* is literally applicable to every pair of morphemically distinct English forms. Consider the following two (synonymous) versions of *S*: (*S*) when we ask whether an action is voluntary we imply that the action is fishy; and (*S'*) when we ask whether an action is voluntary we imply the action is fishy. Even *S* and *S'* are not interchangeable preserving every property of every context. For example, they are not interchangeable preserving truth or meaning in the context "contains the word 'that,'" nor are they interchangeable preserving truth, meaning, or non-oddity in the context "contains more words than *S'*." Thus, given only the information that two linguistic forms are not everywhere interchangeable, the *only* inference that can be drawn is that they are distinct in content or arrangement of morphemes. Furthermore, it is difficult to conceive of a property of contexts such that the failure to preserve that property when *S* and *T* are interchanged could be sufficient grounds for claiming that, though *T* is empirical, *S* is a truth of transcendental logic.

Cavell's second mistake consists of an outright contradiction. Cavell says that *S* and *T* are true together and false together, that is that  $[(S \supset T) \text{ and } (T \supset S)]$ . He also says that *T* is a statement to which someone is entitled only if he has the appropriate empirical evidence, that is that *T* is subject to empirical confirmation and disconfirmation. Finally, he says that *S* is a statement to which the question of evidence is wholly irrelevant, that is that *S* is not subject to empirical confirmation or disconfirmation. However,  $[(S \supset T) \text{ and } (T \supset S)]$  implies both  $(\sim T \equiv \sim S)$  and  $(T \equiv S)$ . From this it follows that any evidence which disconfirms *T ipso facto* disconfirms *S* and that any evidence which confirms *T* likewise confirms *S*.

Thus, Cavell is simply wrong when he says "it is not clear what



would count as a disproof of *S*.”<sup>17</sup> In particular, sufficient evidence for a disproof of *T* would constitute a disproof of *S*. If we discover, as we do, that speakers of English say such things as “he joined the Army voluntarily”<sup>18</sup> (that is, he was not conscripted), then, since no implication of fishiness is involved, *S* and *T* are both shown to be false.

It should be said at this point that we recognize that in making statements of types 1 or 2 the speaker of English may rarely need actually to conduct an empirical investigation of his own speech or that of other English speakers. But nothing follows from this either about the confirmability or disconfirmability of such statements or about their logical status. What is in question here is whether *in principle* there could be a case in which a type 1 or 2 statement, asserted by a native speaker, is empirically confirmed or disconfirmed. Cavell’s position is the most extreme one. By taking such statements as necessary truths of transcendental logic, he precludes the possibility that any empirical evidence could ever be relevant to their confirmation or disconfirmation. Perhaps Cavell has failed to notice in this connection that there are indefinitely many statements which are clearly empirical but which, like statements of types 1 and 2, one normally does not need empirical evidence obtained by special investigation to assert. Consider the following: “My name is not Stanley Cavell,” “I remember the good old days,” “Our family lives in Massachusetts,” “I own a hi-fi set,” and so forth.

At the next step in his discussion, Cavell retreats from his former extreme position. Where he had previously contended that “the question of evidence is irrelevant [to establishing the truth of statements like *S*],” he now argues:

The claim that in general we do not require evidence for statements in the first person plural present indicative, does not rest upon a claim that we cannot be wrong about what we are doing or about what we say, but only that it would be extraordinary if we were (often). My point about such statements, then, is that 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.<sup>19</sup>

Cavell’s first mistake here is that of supposing that type 2 statements (or for that matter type 1 statements) are sensibly questioned *only*

<sup>17</sup> *M*, p. 182.

<sup>18</sup> On pp. 186-187 of *M*, Cavell uses this example, apparently without noticing that it contradicts *S*.

<sup>19</sup> *M*, p. 183.

when there is a special reason for thinking that they may be false. Clearly, we often question statements, and sometimes demand evidence for them, 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.

Cavell's second mistake is that of maintaining that, assuming it would be extraordinary if we were often wrong about what we say, it is not competent to request evidence for such statements. 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. But what Cavell's view entails is that, 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. In such circumstances, then, on Cavell's view, only the suicidal are competent.

Cavell's third mistake is to hold that we are not, in fact, often wrong about what we say about our language. What has gone wrong here is that Cavell has failed to recognize an important aspect of his own distinction between type 1 and type 2 statements. He is surely correct in maintaining that we are not often wrong when we make type 1 statements. This is not very surprising. Type 1 statements are, after all, no more than reports of rather simple and familiar facts about the speech habits of one's language community. But Cavell is surely wrong in maintaining that we are not often mistaken when we make type 2 statements, and this is not very surprising either. Type 2 statements are, in effect, a kind of theory; they are an abstract representation of the contextual features which determine whether a word is appropriately used. Referring to the literature of ordinary language philosophy on such words as "true," "good," "necessary," "voluntary," "mental," "intentional," and so forth, will show that even sophisticated speakers are often wrong about type 2 statements.<sup>20</sup> It is far more difficult to be right about the conditions for using a word appropriately

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<sup>20</sup> In fact, one need go no further than *M* to show the implausibility of Cavell's claim that we are rarely wrong about type 2 statements. In *M*, Cavell notes that Ryle's type 2 statement about "voluntary" is wrong and we have seen (p. 64 of the present paper) that Cavell's espousal of *S* is likewise ill-advised.

than it is to be right about the instances supporting claims about such conditions because statements of the former kind are explanations of the patterns of usage instanced by statements of the latter kind. Fallibility is the price paid for saying something interesting.

The case in which empirical evidence is most clearly relevant to evaluating what we say about our language is the one where two native speakers disagree about the truth of a type 1 or type 2 statement. But, curiously enough, Cavell fails to tell us how, on his account, such a disagreement could be decided without an appeal to empirical evidence. Consider a type 2 conflict where one native speaker asserts, "When we ask whether an action is voluntary we imply that the action is fishy," and another asserts, "When we ask whether an action is voluntary we do not imply that the action is fishy." It does no good to argue that in such a case we should look to see which assertion is best supported by statements of type 1, since the same kind of conflict can arise there too: One native speaker asserts, "We say *X* but we don't say *Y*" and the other asserts, "We say *Y* but we don't say *X*." One would suppose that, since each of the conflicting statements is in the first person plural present indicative and since the "we" occurring in them clearly refers to speakers of English in general,<sup>21</sup> only an empirical investigation of what speakers of English actually say could decide who is correct in such a disagreement. Since Cavell never considers a conflict of this kind, it is unclear how he would avoid this conclusion.

At the one point in *M* where the crucial question of a disagreement between native speakers about what we say might have arisen, Cavell skirts the problem entirely. "Suppose," asks Cavell, "[a] baker is able to convince us that he does [use the words 'inadvertently' and 'automatically' interchangeably]. Should we then say 'So [a] professor has no right to say how 'we use' 'inadvertently'; or to say that 'when we use the one word we say something different from what we say when we use the other'?"<sup>22</sup> Notice that these questions can be taken in two ways. First, they may be taken as asking what kind of information about the speech of the butcher, the candlestick-maker, and other English speakers the professor requires if he is to show that the baker's use of "inadvertently" and "automatically" is idiosyncratic. If Cavell had taken these questions in this way, he would have been obliged to show that the kind of information the professor requires to support his "we" statements is not empirical in character. But Cavell does

<sup>21</sup> This is the way Cavell himself understands the occurrence of "we" in type 1 and 2 statements. Cf. *M*, p. 201.

<sup>22</sup> *M*, p. 199.

not take these questions in this way. Rather, he takes them to ask: if we already know (how we know we are not told) that the baker's use is idiosyncratic, does the fact that such idiosyncratic uses exist entail that the professor has no right to his "we" statements? Cavell concludes that it does not, but it should be noticed that on this interpretation these questions are completely irrelevant to the essential problem of how one adjudicates a clash between native speakers.

Cavell is right when he argues that there is no sense in which the existence of idiosyncratic English uses precludes "we" statements like the professor's; but he is right for the wrong reasons, and this is of considerable philosophical importance. The reason the professor is entitled to his "we" statement, the baker's use to the contrary notwithstanding, is simply that the professor's statement is about standard (normal, common) English while the baker's use diverges idiosyncratically from the standard. The existence of three-legged dogs does not prevent the biologist from correctly asserting that dogs are quadrupeds. There could be no empirical generalization if it were not possible to ignore unsystematic individual variations.<sup>23</sup> But this is not the argument Cavell gives. What he says is "[we can 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."<sup>24</sup> It is clear why Cavell wants this argument: it is philosophically unimpressive to say to your opponent, "What you have just said diverges from standard English,"<sup>25</sup> but it is most impressive to say to him, "What you have just said shows that there is something you are not noticing about the world." And it is also impressive, though less so, to say, "If you don't make the distinction you just impoverish what you can say." But the former argument is mistaken, and the latter is one to which we are not entitled merely on the grounds that someone uses as synonyms two words that are not interchangeable in English.

What is wrong with the latter argument is this: 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

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<sup>23</sup> For a detailed discussion of this topic cf. J. J. Katz and J. A. Fodor, "The Structure of a Semantic Theory," forthcoming in *Readings in the Philosophy of Language* (Englewood Cliffs, N. J., 1963).

<sup>24</sup> *M*, p. 200.

<sup>25</sup> Cf. J. J. Katz and J. A. Fodor, "What's Wrong with Philosophy of Language?," forthcoming in *Inquiry*, for a discussion of why this reply is philosophically uninteresting.

impoverished. There are expressions  $E_1$  and  $E_2$  which can be constructed in English such that  $E_1$  is synonymous with the standard meaning of "inadvertently" and  $E_2$  is synonymous with the standard meaning of "automatically." Thus, the baker can say anything we can say about cases of inadvertent or automatic acts by using  $E_1$  and  $E_2$  where we use "inadvertently" and "automatically." Of course, for the baker  $E_1$  and  $E_2$  will not be used interchangeably, nor will he realize that  $E_1$  is synonymous in standard English with "inadvertently" and that  $E_2$  is synonymous in standard English with "automatically." Thus, on this account, the baker regards "inadvertently" and "automatically" as referring indiscriminately to the members of the set of acts which are either inadvertent or automatic in the standard sense, and he regards  $E_1$  and  $E_2$  as referring respectively to members of two mutually exclusive and exhaustive proper subsets of that set.<sup>26</sup>

There are two objections to Cavell's claim that if the baker uses "inadvertently" and "automatically" interchangeably, he fails to notice something about the world. First, it is simply false that we have distinct nonsynonymous words for each distinction we notice. There are, for example, indefinitely many distinctions we make among shapes, colors, sizes, textures, sounds, and so forth, for which we have no individuating words. 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.

Second, even if it were the case that each and every distinction which speakers of English notice is marked by a pair of English words, Cavell's argument would fare no better. From the fact that the baker fails to make a distinction marked in English, Cavell could conclude that the baker fails to notice something about the world only at the price of complete triviality. To obtain a philosophically significant criticism of the baker, Cavell's argument requires a further assumption, namely that English is a philosophically privileged language with respect to the distinctions it codes. For there exist natural languages which code distinctions not coded in English, and there exist natural languages which do not code distinctions that are coded in English. The Eskimo-Aleut languages distinguish a wide variety of grades and types of what English speakers just call "snow"; conversely, Shona (a language of Rhodesia) and Bassa (a language of Liberia)

<sup>26</sup> This is the sort of point one is likely to overlook when one's philosophical attention is confined to single words to the exclusion of constructible expressions.

fail to code some of the color distinctions coded in English.<sup>27</sup> Given these facts, we must ask: if the baker is missing something about the world just because he fails to draw a distinction coded in English, in what sense is the speaker of standard English *not* missing something about the world because he fails to draw a distinction coded by Eskimo-Aleut? Either Cavell says the English speaker is missing something about the world, or he says the English speaker is not. In the former case, the charge against the baker is completely trivialized because, then, *all* English speakers are missing every distinction coded in other languages but not in English. (The charge against the baker becomes vacuous when it is noticed that the speakers of all languages are also missing every distinction which could be, but is not as yet, coded in some language, that is, an infinite number of distinctions.) Cavell can avoid this trivialization of his argument only by adopting the latter alternative, but this involves assuming that English is a privileged language, that is, that English codes all and only the distinctions that ought to be coded. But surely Cavell could not justify this assumption if only because, *inter alia*, it implies that Shona and Bassa speakers, simply by virtue of not speaking English, are missing distinctions they ought to draw and that Eskimos, simply by virtue of not speaking English, are drawing distinctions they ought to miss.

Obviously, none of these criticisms of Cavell's argument are intended to show that the baker, in using "inadvertently" and "automatically" interchangeably, may not be guilty of a philosophically significant error. 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

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<sup>27</sup> For a discussion of such failures of isomorphism between natural languages, and for further examples, see H. A. Gleason, *An Introduction to Descriptive Linguistics* (New York, 1955); B. L. Whorf, *Language, Thought, and Reality* (Cambridge, 1956); R. Brown, *Words and Things* (Glencoe, Ill., 1958). Cf. also R. Brown and E. H. Lennenberg, "A Study in Language and Cognition," *Journal of Abnormal and Social Psychology*, 49 (1954), 454-462.

utility of appealing to what we ordinarily say as a means of resolving philosophical disagreements.

The conclusion at which Cavell arrives in *A* on the basis of the arguments from *M* that we have been considering is stated as follows:

If it is accepted that "a language" (a natural language) is what the native speakers of a language speak, and that speaking a language is a matter of practical mastery, then such questions as "What should we say if . . .?" or "In what circumstances would we call . . .?" asked of someone who has mastered the language (for example, oneself) is a request for the person to say something about himself, describe what he does. So the different methods are methods for acquiring self-knowledge: as—for different (but related) purposes and in response to different (but related) problems—the methods of "free" association, dream analysis, investigation of verbal and behavioral slips, noting and analyzing "transferred" feeling, and so forth.<sup>28</sup>

From this it is apparent that underlying Cavell's whole position is a misconception about the availability of our knowledge of our language skills. It is obvious, but not worth arguing, that the knowledge we gain in correctly describing our language is in *some* sense self-knowledge. But 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. What is worth arguing is that anything we learn about ourselves when we describe the language we speak is also something we learn about every other speaker of standard English *qua* speaker of standard English. Conversely, anything we can learn about English by studying our own speech, we can in principle learn by studying the speech of speakers other than ourselves. This is what it means to say that we are studying English rather than the speech pathology and idiosyncracies of English speakers. Put it another way: 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.

We said at the outset that the position Cavell advocates is pernicious both for an adequate understanding of ordinary language and for an adequate understanding of ordinary language philosophy. The first follows from Cavell's refusal to treat empirical questions with empirical methods. If we deny that the truth of our statements about our language must be established by the usual empirical means, we fail even

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<sup>28</sup> *A*, pp. 87-88.

## AVAILABILITY OF WHAT WE SAY

to raise the question of what empirical constraints are relevant to adjudicating the adequacy of a putative description of a natural language. Clearly, answering this question must be the first step in the elaboration of any such description.<sup>29</sup>

That Cavell's position blocks an adequate understanding of ordinary language philosophy follows from the fact that the Oxford philosopher, when he discusses the use of words, *is* pursuing an empirical investigation, and *is not* uncovering truths of transcendental logic. This, of course, does not mean that he goes about with pen and paper recording what people say, when they say it, how frequently it is said, and so on. That is a caricature of what the empirical investigation of language is like, though it seems to be the only conception Cavell entertains as an alternative to his viewpoint.<sup>30</sup> Rather, to say that the Oxford philosopher engages in empirical investigation is to say that his claims about English should be subject to the same modes of confirmation and disconfirmation that linguists accept.

What has until now distinguished the Oxford philosopher from the linguist is primarily a difference of focus. The linguist has traditionally been concerned with problems of phonology, phonemics, morphology, and syntax, while the Oxford philosopher has devoted himself almost exclusively to problems about meaning. What has distinguished some Oxford philosophers is their ingenuity at discovering recondite facts about how English speakers use their language. But methods of confirmation and disconfirmation distinguish neither the philosopher from the linguist nor the philosopher himself.

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<sup>29</sup> To appreciate the importance and complexity of the problem of setting empirical constraints on the description of a natural language, the reader need only examine the extensive discussion this problem has received in the literature of descriptive linguistics.

<sup>30</sup> We recognize that in *M* Cavell is replying to Mates. But, even so, the magnitude of his claims is such that they ought to have been defended against more tenable conceptions of empirical investigation in linguistics, for example the conception implicit in Chomsky's *Syntactic Structures* (S'Gravenhåge, 1951).