

A PRIORI CONVICTIONS ABOUT PSYCHOLOGY:  
A RESPONSE TO SOSA AND TAYLOR

Michael Devitt

In Truth: Philosophical Issues, 8, 1997  
Enrique Villanueva, ed.

Atascadero: Ridgeview Publishing Company (1997): 371-85

In their helpful criticisms of Coming to Our Senses (1996) both David Sosa (1997) and Kenneth Taylor (1997) resist the idea that they are revisionists about meanings. I shall argue that they must accept the idea. Of course, to be revisionist is not necessarily to be wrong: many parts of the theoretical status quo have been wrong in the past and some are surely wrong now. Most of one's best friends are revisionist about something. However, I shall argue, the revisionisms of Sosa and Taylor are wrong. Sosa's rests on the popular but mistaken conviction that a properly scientific explanation of behavior must ascribe narrow properties to thoughts. I shall call this "the narrow conviction." Taylor's eliminativism rests on the popular but mistaken conviction that the explanation must ascribe cognitively rich properties. I shall call this "the rich conviction."

But first I must defend the methodology that leads to my view about meanings.

### 1. Methodology

Taylor charges that my methodology is too "deeply conservative" and too "deferential" to the folk. He takes it to restrict semantics to the study of the "meanings we folk ascribe when we ascribe propositional attitudes (p. 1)." He thinks that this restriction is parallel to one restricting the study of syntax to what can be "explained in folk grammatical terms." He comments: "No working linguist would take seriously the syntactic cousin of Devitt's semantic proposal. Why should it be different in semantics? (p. 2)."

But Taylor has not got my methodology right. These restrictions are totally alien to it.

The "basic" semantic task is to explain the nature of meanings. I propose the following explication of this vague statement: the task is to explain the nature of the properties of mental and linguistic tokens that play semantic roles, particularly the roles of explaining behavior and guiding us to reality. To accomplish the basic task, I argue, we should look to the "descriptive" task for evidence; that is, we should examine the nature of the properties we now do ascribe to tokens to explain behavior and guide us to reality (2.9; "second methodological proposal"). For, given that these ascriptions appear to be successful, it is likely that that these properties really do play those semantic roles and so are meanings that we need to explain to accomplish our basic task. That is the extent of my conservatism.

It is surely a conservatism of the sort enshrined in scientific practice. Contrary to Taylor's charge, the methodology does not

restrict the basic task to these properties that the folk already ascribe.<sup>1</sup> The claim is only that these properties are likely to be among the meanings we need to investigate, but there may well be others. Indeed when I apply the methodology, I contemplate many others (and even accept some: 5.12).

How do we accomplish the descriptive task? Since we do not have an already well-established theory we must use the "ultimate" method: first, we identify uncontroversial examples of tokens with and without a certain putative meaning; second, we examine the tokens to determine the nature of that meaning (2.10-2.11). In the first stage, I suggest, we should be guided by folk opinion (counting ourselves among the folk), because the folk, in their propositional attitude ascriptions, are as expert as anyone at identifying putative meanings. That is the extent of my deference to the folk. I do not take the folk identifications to be infallible, and I give folk opinion hardly any role in the second stage theorizing about the nature of those identified meanings. So, contrary to what Taylor implies, the methodology does not restrict semantics to what can be explained in folk terms. Indeed when I apply the methodology, I follow the custom in both linguistics and semantics of giving some ordinary terms more precise "technical" meanings and of introducing some other terms.<sup>2</sup> I have nothing except a relatively trivial stylistic objection to much more innovation.

## 2. A Shocking Claim About Meanings

For the sake of argument, let us accept the language-of-thought hypothesis. So thoughts are relations to mental sentences. Our focus is on the meanings of mental words. What property of a word explains its role in causing intentional behavior and hence counts as its meaning? In Coming I argue that the property is one of purporting to refer to something in a certain way; it is the property of having a certain mode of reference (4.2-4.3).<sup>3</sup> This mode may often

<sup>1</sup>"What we specify by a t-clause will count as a meaning if and only if it does indeed have a semantic role. Any other property will count as a meaning if and only if it has such a role." (p. 61, emphasis added)

<sup>2</sup>There is an irony in Taylor's appeal to linguistics in criticizing me. On the one hand, linguists conceive of their task in a way that ties it very closely to folk judgments about what is acceptable in the language (although, as Taylor points out, linguists do not confine their theory to folk terms). For they see the task as that of explaining the speaker's tacit knowledge of the language from which these judgments are causally and rationally derived. On the other hand, I have often criticized this close tie to folk judgments; see particularly, Devitt and Sterelny 1989; also, Coming: 77n and 81n.

<sup>3</sup>See p. 153 for a qualification.

be "descriptive," being constituted by inferential links to other words; 'bachelor' is a likely example. But, descriptive modes simply pass the referential buck from one word to other words. The buck must stop somewhere with words that stand on their own referential feet: these words have "causal" modes constituted by direct noninferential causal links to reality; 'Mary' and 'water' are likely examples (4.5-4.6).

Three things about this latter claim are rather shocking. First, the idea that a meaning might be a property of referring by a certain causal mode is alien to the semantic tradition. That tradition thinks of a word's meaning as either the property of simply referring to something - the Millian theory of names, for example - or, alternatively, as a property constituted by the words relations to other words - the description theory of names, for example. Direct-reference semantics, to which Taylor subscribes, is very much part of this tradition (4.8, 4.18).

Second, the idea that behavior can be explained by something so external to the brain as these causal meanings goes against a widespread conviction that, more than anything else, drives people to methodological solipsism;<sup>4</sup> it goes against "the narrow conviction." Thus, it seems as if Sosa thinks that what does the real explanatory work - what future psychology will talk about - must be something internal to the brain, a narrow meaning.

Third, the idea that behavior can be explained by something so cognitively austere as these causal meanings goes against another widespread conviction that plays a role in driving people to holism;<sup>5</sup> it goes against "the rich conviction." Taylor is not driven to holism. Still, impressed by the cognitive diversity of people who are same believers, he thinks that only something much richer and more cognitively fine-grained than these causal meanings could do the explanatory job in psychology.

The two convictions are doubtless causally related. Thus the fine-grained meaning demanded by the rich conviction is sought in the brain. So it may seem natural to think that what explains behavior must be internal to the brain, as demanded by the narrow conviction. Ned Block (1991) is an example of a philosopher who has both convictions.

Coming argues against direct reference and, in effect, both convictions. I shall follow Taylor (n. 1) in setting direct reference

---

<sup>4</sup>In Coming I quote examples from Stephen Stich, Colin McGinn, and Brian Loar (pp. 272-3, 300).

<sup>5</sup>I think that the passages from David Papineau and Ned Block referred to in Coming (pp. 38-9) are examples.

aside here. I shall first demonstrate that Taylor is a revisionist and that his argument rests entirely on the unargued rich conviction. I shall then demonstrate that Sosa is a revisionist and that his argument rests entirely on the unargued narrow conviction. I shall conclude by discussing the convictions.

### 3. Taylor's Revisionism

Let us abbreviate my thesis about mental words as follows:

(T) A mental word explains behavior in virtue of having a certain mode of referring.

Taylor rejects (T), particularly where the modes are causal as I argue they must sometimes be. (T) follows from two other theses that I argue for:

- (1) **Descriptive:** In order to explain behavior, we do as a matter of fact ascribe to a mental word a certain mode of referring.
- (2) **Basic (Normative):** Whatever we ascribe to a mental word to explain behavior does indeed do so (and hence ought to be ascribed for that purpose).

So, Taylor must reject at least one of these theses. In fact, he rejects both. Concerning (1) he says: "I think Devitt mischaracterizes that which is actually specified by our folk opaque ascriptions" (p. 10n); he thinks that I "have greatly overestimated the conceptual specificity of opaquely construed that clauses" even though I travel "in the best of company" in so doing (p. 2). Concerning (2) he says: "that which is 'common and peculiar'...to cognizers of whom the same opaque attributions are true is not causally homogeneous enough to back causally deep psychological explanations of behavior" (p. 2). I have three points to make in response.

1. It is puzzling that Taylor does not see the rejection of (2) as revisionist. Revisionism is the rejection of the theoretical status quo. The status quo in explaining behavior are our ordinary opaque thought ascriptions. These ascribe properties to mental words. In rejecting (2) Taylor is denying that these properties, whatever they may be, do indeed explain behavior; they are "not causally homogeneous enough." He anticipates that I will assess his position as revisionist and responds: "that assessment presupposes that he is right and that I am wrong about the outcome of what he calls the descriptive task" (p. 10n); i.e. it presupposes that I am right about (1). But it does not presuppose this at all. The quoted passage denies the explanatory adequacy of what is common and peculiar to some believers, not of what Devitt thinks is common and peculiar.

(1) and (2) are very different matters. Indeed, since he thinks that in urging (1) I "have greatly overestimated the conceptual specificity" of what we ascribe, he must think that what we ascribe

is less conceptually specific and hence should think that our ascriptions are less suited to the explanatory task on his view of them than on mine: on his view, they should seem to him more distant from ascribing something causally homogeneous enough to explain the behavior of the cognitively diverse. In other words, if his view of the ascriptions were right, he would be more revisionist than I think he is.

2. It is also puzzling that Taylor presents no argument for rejecting (1). He claims that I have "paid insufficient attention to what the folk actually ascribe" (p. 2) and promises to execute this descriptive task "more carefully" than I or my "many fellow travellers have done" (p. 3). Yet he cites no example of our inattention or carelessness; in particular, he cites none in my 108-page discussion of the matter (in ch. 4). More curiously, his own discussion of this matter, although not of (2), is along very much the same lines as mine; see his discussion of Vanya and Tanya (pp. 3-5) and causal theories of reference (pp. 7-9). And the discussion breaks off before drawing any conclusion about the nature of what we ascribe; a fortiori, before drawing any conclusion at odds with mine.

3. In support of his rejection of (2), Taylor repeatedly airs the "worry...that given the cognitive diversity of some believers, it may well turn out that nothing causally homogeneous enough to back causally deep explanations is common and peculiar to them" (p. 5; see also p. 8). By the end, this worry has become a "reason to believe that whatever" some believers share "is too conceptually thin and causally heterogeneous" to explain behavior (p. 9). But a reason is precisely what we have not been given. The cognitive diversity of some believers has been nicely illustrated and the worry has been aired. That is all. We have not been given one word of argument to show that what is shared by common believers is not "causally homogeneous enough to back causally deep psychological explanations." What we have here is a striking example of the unargued rich conviction.

Contrary to what Taylor claims (p. 10n), I do offer arguments for (2) and hence against the conviction. First, I offer a very simple argument: our ordinary ascriptions of properties to mental words to explain behavior are generally successful. So it is likely that those words have the properties ascribed. And if (1) is right that these properties are often properties of referring by causal modes, it follows that such cognitively austere properties do explain behavior, contrary to the conviction. The simple argument is not conclusive, of course. I also argue against various revisionist attempts to undermine this argument (ch. 5). Taylor has not addressed any of these arguments.

In sum, Taylor's whole case against (T) comes down to the rich conviction, which he neither argues for nor defends from my implicit criticisms. I shall return to this conviction after discussing Sosa.

#### 4. Sosa's Revisionism

Sosa defends

a kind of two-factor theory according to which meaningful tokens have two sorts of referential meaning properties. One meaning property is just that of having a specified referent. The other meaning property is that of having a certain functional role.

This functional role is defined by the token's relations to other tokens and to proximate stimuli and output. (p. 1)

So far, the theory appears to be the standard two-factor theory positing two largely independent meaning factors, one a referential factor and the other a narrow functional-role factor.<sup>6</sup> But then Sosa adds:

But the [functional] role constitutes a character: it determines, given external contexts as argument, a referent as value. The other factor can thus be seen as the meaning property of having a specified referent relative to a context and under a mode. (p. 1)

With this addition, Sosa conflates the idea of narrow meanings as functional roles with the very different idea of narrow meanings as functions (in the mathematical sense).<sup>7</sup> I should be sympathetic to the conflation because I once made it.<sup>8</sup> But I now think that it is a mistake.

Before considering this mistake, we should establish quickly that Sosa's view is revisionist. Since his narrow meanings determine reference, he does not reject Representationalism (in my sense) and so is not revisionist in that respect, as he points out (p. 6). But he is revisionist in proposing, somewhat tentatively, that we should ascribe these narrow meanings to explain behavior. The meanings folk ascribe for that purpose all involve reference and are wide (Coming: 4.9, 5.10).

If we believe in wide truth-referential meanings, as I do, then we should have no objection to the idea of narrow meanings as functions,

<sup>6</sup>To be found, e.g., in Loar 1981 and 1982, McGinn 1982, Block 1986.

<sup>7</sup>This different idea can be found in White 1982; Fodor 1987: 44-53.

<sup>8</sup>See particularly my 1989. So far as I know, I am the only person to have made this mistake before Sosa.

because any theory that explains the wide meaning will explain the narrow ones; we get the narrow meanings by abstracting from the links to context that partly constitute the wide meanings. Suppose someone has a mental word that she would express 'water' and that has a wide meaning involving reference to water. Then it follows that the word has a narrow meaning that is a function yielding the wide meaning involving reference to water as its value given something about the actual world as its argument. Given something else as its argument, the function might yield a wide meaning involving reference to twin water or whatever (Coming: 5.10).

If we believe in molecularism, as I do, we should have no objection to the idea that a narrow-meaning-as-function might be the property of having a certain functional role. For, a molecularist believes that the wide meaning of a word might be explained by a description theory and hence be the property of being inferentially linked to other words with certain wide meanings; for example, the meaning of a word that means **BACHELOR** might be the word's property of being linked to words that mean **ADULT**, **UNMARRIED**, and **MALE**. If it is, then the narrow-meaning-as-function of that word will be the word's property of being linked to words with the related narrow-meanings-as-functions; for example, to a word with a function that yields the value **MALE** given something about the actual world as argument. This fact about the original word's narrow meaning simple follows from the fact about its wide meaning.

The mistake in Sosa's theory is the idea that a narrow-meaning-as-function is a property of having a certain functional role that includes links to proximate stimuli and output.<sup>9</sup> No current theory of reference, whether causal or descriptive, adverts to such links. So we have good reason to believe that the links do not partly constitute a word's mode of reference. If they do not, then they do not partly constitute the word's wide meaning of referring by that mode. So they could not constitute the narrow meaning that yields the wide meaning as value given the external context as argument. Crudely, wide meaning = internal narrow-meaning-as-function + external context. So there can be nothing more to the narrow meaning than "the internal parts" of the wide meaning. These parts might include inferential links to other words, as we have noted, but they never include links to proximal stimuli and output.<sup>10</sup>

---

<sup>9</sup>All these links are dispositional, of course.

<sup>10</sup>In allowing that a word may have a narrow meaning constituted by inferentially links to other words, a molecular theory allows functional-role meanings. Nevertheless, the theory is not aptly called "two-factor" because it differs in two crucial ways from standard two-factor theories: its functional-role meanings determine reference; and they do not include relations to proximal input and output. As a result, these functional-role meanings, unlike those

This is not to say that these links have nothing to do with a narrow-meaning-as-function. For, in virtue of what does a particular token word have that function? The answer will surely advert to the token's links to proximal stimuli and output. But this answer explains what the narrow meaning supervenes on in this case; it tells us what mechanisms implement the meaning here. It does not tell us anything about what constitutes that meaning; it does not tell us about the nature of the meaning. That nature is simply the internal property of the word that yields the appropriate wide meanings as values given external contexts as arguments. This meaning is likely to supervene on many different links to proximal stimuli and output in the many people who have mental words with the meaning. Consider the narrow meaning of 'water' for example. It may sometimes supervene on links to visual stimulations by water, sometimes on those to aural stimulations, and so on; where the meaning is gained by reference borrowing, it may sometimes supervene on links to visual stimulations by 'water', sometimes on those to aural stimulations, and so on. These differences are irrelevant to the nature of the meaning, irrelevant to the property of the word that enables it to explain the behavior that Sosa wants it to explain.<sup>11</sup>

Aside from this, the argument that Sosa gives for his second factor is straightforwardly an argument for narrow-meanings-as-functions; he says nothing to warrant functional-role meanings partly constituted by links to proximal stimuli and output. I argue in Coming that nobody else has said anything to warrant it either (5.11-5.12). So I shall henceforth taking Sosa to be simply proposing narrow-meanings-as-functions, overlooking the mistake of conflating the two sorts of narrow meanings.

Construed in this way, Sosa's tentative revisionism is small. The folk ascribe wide Representationalist meanings to explain intentional behavior (and to guide us to reality). According to Sosa's revisionism, they should ascribe narrow Representationalist meanings-as-functions to explain "proto-intentional" behavior. My main point about this revisionism is that "it does not seem to be a BIG ISSUE" because both sorts of explanation are good (p. 308). The revisionist one differs from the folk one in that it adverts to meanings and behaviors that may be "coarse grained" in that there is not much to their natures and "promiscuous" in that they can yield any of a vast range of values by changing the relevant external context

---

of two-factor theories, do not require a distinct functional-role semantics. All meanings, wide and narrow, are covered by the one truth-referential semantics.

<sup>11</sup>The distinction between constituting and implementing a meaning is nicely brought out by Jerry Fodor (1991: 265-6) in replying to Block 1991.

as argument. So the issue of revisionism comes down to whether we should prefer to explain the possibly coarse-grained and promiscuous proto-intentional behaviors rather than the fully intentional behaviors. I think that Sosa and I are very much in agreement about all this. Still, we disagree in our preference. I prefer the folk explanations for a variety of reasons, particularly that proto-intentional behaviors are likely to be too coarse-grained and promiscuous. Sosa is unconvinced, leaning toward the revisionist explanation. Why? The answer is a tentative commitment to the aforementioned narrow conviction.

## 5. The Narrow Conviction

Sosa's commitment is evidenced in his discussion of a nice analogy: a sugar that dissolves in lemonade and another sugar that dissolves in iced tea:

the best explanation of the [first] sugar's, so to speak, wide behavior of dissolving in lemonade seems to be nothing more than the conjunction of its dispositional property of solubility in water-based solvents and its contingent property, together with whatever explains this, of being in lemonade. (p. 3)

Similarly, of course, the explanation of the second sugar's dissolving in iced tea. Here I am in total agreement with Sosa. But when he compares these explanations with explanations of the those of identical twins with different relational states, he concludes:

That we can appeal to those relational states, especially when we are not doing science, does not show that we should not appeal to the nonrelational narrow properties. Moreover, from a scientific point of view, emphasizing the difference between the twins' behavior is like emphasizing the difference between dissolving in lemonade and dissolving in iced tea. (p. 5)

There is an implication here, and in an earlier suggestion of "a psychology whose explanations of behavior yield insight into the intrinsic natures of agents" (p. 5), that the narrow explanation of the twins' common proto-intentional behavior is, but the wide explanation of their different intentional behavior is not, scientific. This is the narrow conviction.

Before, I criticize this conviction, I want to emphasize that Sosa's commitment to it does seem tentative and that I agree with much of what he says. In particular, I agree that the wide meaning that explains an intentional behavior is a conjunction of a narrow meaning and an external context. Furthermore, it could be of interest to break the wide meaning down into these components in giving the explanation. It is worth noting, however, that we do not have any informative way of doing this at present. The properties ascribed by our ordinary thought ascriptions are wide and our only ways of

identifying the relevant narrow meanings are parasitic on these ordinary identifications of wide meanings. (I suggest adding the prefix '\*' to the t-clause of ordinary ascriptions; p.287.)

I am in no position to deny the appeal of the narrow conviction having once embraced it somewhat tentatively myself (1989). I now think that it is quite mistaken. In Coming (pp. 300-3), drawing on Tyler Burge (1986), I argue that there is no basis in science for this prejudice against the relational. Science is full of relational explanations. Whether or not a particular one is appropriate cannot be settled a priori: it has to be settled case by case.<sup>12</sup>

Of course, Sosa, and others before him, do not simply rely on the conviction in arguing for narrow explanations in psychology: they also appeal to Twin-Earth examples. These examples are thought to show that psychological interest is only in what is common to a person and her twin. So that is all that a psychological theory should be concerned with: it should be concerned with narrow explanations.

Twin-Earth examples are misleading. Explanations of wide behavior, whether of the sugar dissolving in lemonade or of Oscar giving water to Mary, can be broken down into an internal factor and an external factor. Where the internal factor does most of the work, as perhaps it does in explaining the sugar, we may often prefer to focus on it and hence prefer a narrow explanation. But where the external factor does most of the work, we are likely to prefer the wide explanation. Twin-Earth examples are misleading because Oscar on Earth and Twin Oscar on Twin Earth are identical, and their behaviors are so similar, that we are not encouraged to think that these behaviors may be largely explained by external factors. Rather, we are encouraged to think that the different external factors for Oscar and Twin Oscar are responsible for the relatively minor differences between Oscar's giving water to Mary and Twin Oscar's giving twin water to Twin Mary, but each behavior is almost entirely explained by a fine-grained internal factor shared by Oscar and Twin Oscar (who, of course, share many fine-grained internal properties). So Twin-Earth examples suggest that some fine-grained internal property carries most of the burden of psychological explanation. I argue that this suggestion is very likely false (287-92, 309).

Theories of reference, particularly historical-causal theories, may well show that the one shared internal factor could, with an appropriate change in the external context, play just the same role in explaining a vast range of behavior involving not only giving but taking, kicking, and many other acts; involving not just water and

---

<sup>12</sup>This point is not affected by Sosa's legislation that the "causal powers" of an object are intrinsic and nonrelational (cf. Fodor 1987, pp. 34-8, discussed in Coming, pp.300-1). Scientific explanations are not restricted to the causal powers covered by this legislation.

twin water but milk, wine, gold and all the other stuffs; and involving not just Mary and Twin Mary but Reagan, France, and any other object that can be named. Contemporary theories of reference make plausible the idea that a great deal of what determines reference, and hence explains intentional behavior, may be determined by what is outside the head. There may often be very little to the function that is the narrow meaning of a word. Hence its contribution to the explanation of behavior may be relatively small. Putting this another way, the proto-intentional behaviors that narrow meanings explain may be so coarse-grained and promiscuous as not to distinguish behaviors involving any named object, any stuff, and so on. The extent to which this is actually the case remains to be seen as we develop theories of reference. It cannot be settled by appeals to Twin Earth.

How is it possible that the explanation of intentional behavior might be such an external matter? According to the Representationalism I urge, the role of a thought in explaining intentional behavior depends on what it represents. We cannot settle a priori the extent to which intrinsic properties of the mind determine this. What matters to the explanation is simply that the thought has its representational property under a mode appropriate to the behavior it is supposed to explain. Theories of reference will tell us about those modes.

Perhaps I am wrong in my predictions about theories of reference and we will discover that internal factors are more important to the determination of reference than I have suggested. Perhaps this will show that we should indeed prefer psychological explanations that advert to narrow-meanings-as-functions rather than wide meanings. As I have said, this is not a BIG ISSUE. However, the issue is not settled in favor of the narrow by an a priori conviction that a scientific psychology must be narrow, nor by Twin-Earth examples.

## 6. The Rich Conviction

The narrow conviction leads Sosa to a minor revisionism. The rich conviction is likely to lead Taylor to major revisionism, one that abandons Representationalism.

This conviction is that the explanation of behavior requires a cognitively rich and fine-grained property of a mental word. The property of referring by some causal mode could not meet this requirement. Only a "descriptive" property largely constituted by many of a word's inferential connections to other words could do the job.

Consider, Taylor, for example. He is struck by the fact that mental words that share the property ascribed to them by belief ascriptions may differ greatly from each other in their other properties. He emphasizes that people be who have words that share the ascribed property may be cognitively diverse. (This is apparently

even more true on his view of what is ascribed than on mine, as I noted in section 3.) In the light of this diversity, this property could not do the job of explaining behavior. We need something much finer-grained. Why? Taylor does not say, but presumably the answer is: because the cognitive diversity among people who have words that share this finer-grained property will be much less.

It will indeed be less, but unless we go all the way to holism, which Taylor does not, there will still be some cognitive diversity among those who share the explanatory property. That is, there will be diversity unless we require that the explanatory property of the word is constituted by all of the word's functional relations, with the result that the property can explain the behavior only of functionally identical people. One wonders what basis Taylor could have for his view that the property our belief ascriptions ascribe allows too much diversity for explanatory effectiveness but that holism allows too little.

Very likely, anything differs from anything else in some respect. Yet, anything is similar to anything else in some respect. Among the similarities between things we often find a property that explains the behavior or characteristics of those things. The things that can be explained by this property can otherwise differ. Perhaps, sometimes they will differ a lot, perhaps sometimes a little. But the mere fact that they differ a lot is no reason for concern about the explanation. A penguin is very different from an eagle but the fact that they are both birds explains a lot about them; Clinton is very different from Joyner-Kersey but the fact that they are both Americans explains a lot about them. A car wheel is very different from a Ferris wheel but the fact that they are both wheels explains a lot about them. There is no scientific principle that says: "If a property is to be explanatory, things that share it must only differ to degree  $n$ ." No whistle is blown when explained things differ a lot. There is no way of telling a priori that so much difference is too much.

It is beside the scientific point that the people who have a mental word with the wide referential property we ascribe are cognitively diverse. What matters is whether ascribing such properties best explain behaviors. The fact that these ascriptions are so successful in ordinary life and the social sciences is evidence that they are good. Do they face rivals that are as good? We have just taken a somewhat dim view of one: that we should ascribe narrow proto-referential properties. Taylor does not say precisely what rival he has in mind but he seems headed toward one that ascribes a nonholistic non-reference-determining functional-role meaning. Coming takes a very dim view of this rival (5.11-5.12). First, we have been given no idea how to explain such a meaning. Which of the indefinitely many functional roles of a word constitute its meaning? Second, we have been given no idea how such a meaning could explain intentional behavior. All in all, the evidence is strong that our

ordinary ascriptions do indeed provide the best explanation of behavior.

It is of course possible that ordinary ascriptions do not. It is possible that a future psychology will show that "causally deep" explanations ascribe much richer meanings than do our ordinary ascriptions. Taylor is convinced that this is so. But he has not produced any evidence that it is so and I have attempted to show that it is not so.

In conclusion, Sosa proposes a mildly revisionist view largely on the basis of the tentative a priori conviction that we should ascribe narrow meanings to explain behavior. Taylor urges what is likely to be a very revisionist view solely on the basis of the a priori conviction that only something cognitively richer than what we ordinarily ascribe to explain behavior could do the job. I have argued that these revisionisms and convictions are wrong.<sup>13</sup>

University of Maryland, College Park

#### REFERENCES

- Block, Ned. 1986. "Advertisement for a Semantics for Psychology." In French, Uehling, and Wettstein 1986: 615-78.
- . 1991. "What Narrow Content Is Not." In Loewer and Rey 1991: 33-64.
- Devitt, Michael. 1989. "A Narrow Representational Theory of the Mind." In Rerepresentation: Readings in the Philosophy of Psychological Representation, ed. Stuart Silvers, 369-402. Dordrecht: Kluwer Academic Publishers. Reprinted in Mind and Cognition: A Reader, ed. William G. Lycan. Oxford: Basil Blackwell. Lycan 1990: 371-98.
- . 1996. Coming to Our Senses: A Naturalistic Program for Semantic Localism. New York: Cambridge University Press.
- Fodor, Jerry A. 1987. Psychosemantics: The Problem of Meaning in the Philosophy of Mind. Cambridge, MA: MIT Press.
- . 1991. "Replies." In Loewer and Rey 1991: 255-319.
- Loar, Brian. 1981. Mind and Meaning. Cambridge: Cambridge University Press.
- . 1982. "Conceptual Role and Truth-Conditions." Notre Dame Journal of Formal Logic 23: 272-83.
- Loewer, Barry, and Georges Rey, eds. 1991. Meaning in Mind: Fodor and His Critics. Oxford: Basil Blackwell.
- McGinn, Colin. 1982. "The Structure of Content". In Thought and Object, ed. A. Woodfield, 207-58. Oxford: Clarendon Press.
- Sosa, David. 1997. "Meaningful Explanations." In this volume.
- Taylor, Kenneth A. 1997. "Same Believers." In this volume.
- White, Stephen L. 1982. "Partial Character and the Language of Thought." Pacific Philosophical Quarterly 63: 347-65.

---

<sup>13</sup>My thanks to David Sosa and Kenneth Taylor for comments that have led to improvements in this response.