Meanings and Psychology: A Response to Mark Richard

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In his rich paper, "What Does Commonsense Psychology Tell Us about Meaning" (1997), Mark Richard uses some characteristically ingenious examples to make four critical points against my views in chapter 4 of *Coming to Our Senses* (1996). These criticisms are aimed both at my view of the attitude ascriptions which partly ascribe meanings and at my "Representationalist" view of the meanings ascribed. The criticisms are interesting and very intricate. Still, with one exception, I shall argue that they clearly fail. The exception is the defense of contextualism. Even there I am inclined to think that the phenomena he points to are best explained pragmatically rather than semantically.[1]

My Representationalism is the view that meanings are entirely constituted by representational properties. The "representational properties" of a token include any property that plays a role in determining what it represents. So the meaning of *a sentence* is exhausted by the properties that determine its *truth condition*.[2] And the meaning of *a word* is exhausted by properties that determine its *reference*. So Representationalism is a sort of truth-referentialism. What is the argument for it?

1 The Methodology of Naturalistic Semantics

The argument starts with three important "methodological" questions. What are the semantic tasks? Why are they worthwhile? How should we accomplish them? I think that these questions get insufficient attention in semantics and that the explicit or implicit answers to them are mostly unsatisfactory.

In chapter 2 of *Coming* I address the methodological questions.[3] I define three semantic tasks by focusing on the purposes for which we ascribe meanings: in particular, the purposes of explaining behavior and using thoughts and utterances as guides to reality. I then propose a methodology for accomplishing these tasks.

We should tackle the "basic" task of explaining the nature of meanings by tackling the "normative" one of explaining the properties that we ought to ascribe for semantic purposes ("first proposal"). Our ordinary attitude ascriptions use 'that' clauses ("t-clauses") to attribute certain properties for semantic purposes. These properties are *putative* meanings. Given the apparent success of the ascriptions it is likely that these putative meanings are *real* ones. So we should look to the "descriptive" task of explaining putative meanings for evidence for the normative/ basic one ("second proposal"). To accomplish this descriptive task we need to discover what is common and peculiar "in all possible worlds" to the tokens to which we ascribe a putative meaning. Because we approach this task pretty much from scratch, we should use the "ultimate" method ("third proposal"). The preliminary first stage of this method identifies examples for a straightforwardly scientific examination in the second stage. Intuitions and thought experiments of the sort that dominate semantics are important in the first stage. However, they are empirical responses to the phenomena and are open to revision at the second stage.

My discussion of the semantic tasks is an attempt to explicate our notoriously vague talk of "meanings"[4] by specifying a subject matter for semantics that is both *worthy* of investigation and appropriately *fundamental*. What about other views of the tasks? *Coming* takes a brief and largely critical look at these (2.7). But my point is *not* that the properties that I have called "meanings" are the *only* *real* meanings nor that the tasks I have called "semantic" are the *only proper* tasks for semantics. I doubt that there is any interesting matter of fact about such claims. I do claim that those properties are worth investigating and that those tasks are worth performing. I claim further that those tasks are appropriately fundamental. Perhaps all this is true of other tasks, but that always needs to be demonstrated.

Demonstrating this is not as easy as Richard may think. He contemplates briefly another view of the semantic tasks with a locus on "homilies about understanding and communication" rather than on attitude ascriptions (p. 87) and with the following view of our purposes:

> To say that Fish means what I do with 'text' is to say that he "uses" it in ways similar to that in which I and my fellows do, where (in turn) any number of accounts of "use" might flesh the proposal out. (For example, use might be inferential or functional role, or some elaborate construction from the group form of life.) (p. 110; n.2)

This will not do as a characterization of the semantic tasks because it hardly begins to address the demand for explication and worth. What is to guide us in choosing this functional role and not that, this construction from the group form of life and not that, as the meaning of 'text'? What special role is the chosen property supposed to have? Why is it theoretically worthwhile to investigate a property with that role? Richard faces a lot of work to answer these hard questions.

2   Representationalism

I argue for Representationalism by applying my methodology. The second methodological proposal instructs us to look to the descriptive task for evidence about the nature of meanings. The third proposal instructs us to use the "ultimate" method on the descriptive task. Applying this "ultimate" method, and drawing on the classic discussion of transparent and opaque attitude ascriptions generated by Quine,[5] we find support for the descriptive version of Representationalism (4.2).

The focus is on definite singular term tokens. The methodology opens up the possibility that the folk might, in their use of t-clauses, ascribe more than one putative meaning to a token. This is indeed what we find: the folk seem to ascribe at least three different sorts of putative referential meaning to a singular term token: the property of (purportedly) referring to a specified object under a specified mode (opaque ascription); the property of referring *en rapport* to a specified object ("rapport-transparent" ascription); the property of referring to a specified object ("simply-transparent" ascription).

Let us follow Richard in ignoring the second sort involving rapport. Concerning the other two sorts, a key claim is that a sentence like

(1)   Ralph believes that Ortcutt is a spy

is ambiguous. On its opaque reading it ascribes to singular terms in beliefs the property of (purportedly) referring to Ortcutt under the mode 'Ortcutt.' On its transparent reading it ascribes to singular terms the property of simply referring to Ortcutt in some way or other. The first property is what is common and peculiar to all such terms to which the opaque ascription applies; the second property, the transparent ascription.

We now take this descriptive account of putative meanings as evidence of meanings (4.3). If this account of the properties that we do ordinarily ascribe for semantic purposes is correct, then probably these are properties that we ought to ascribe for those purposes and so probably they are meanings; so probably tokens have more than one meaning; and probably Representationalism is correct as a *normative/basic* doctrine too. For, these ordinary ascriptions *successfully* serve those purposes of explaining behavior and guiding us to reality. In particular, opaque ascriptions are particularly suited to the task of explaining behavior.

This summarizes the core argument for the core theory. Much more needs to be done, of course. I argue against rival theories. First, the 'Fido'-Fido or Millian theory of proper names, urged so determinedly by direct-reference philosophers, is rejected. That theory is right about one meaning of a name token, the transparently ascribed one, but wrong in denying the other, the opaquely ascribed one that is so important in explaining behavior. This denial is theoretically *ad hoc* (4.8). Second, semi-Representationalist two-factor theories and anti-Representationalist verification and use theories are rejected as descriptive theories; in particular, there seems to be no evidence that the folk ever ascribe a non-reference-

determining functional-role putative meaning (4.9–4.10). In the light of these descriptive failings the onus of showing that semi- or anti-Representationalist theories are nevertheless normatively correct, hence correct about meanings, is heavy. *Coming* Chapter 5 argues that these theories do not come close to discharging this onus.

The core theory needs some "bells and whistles." Attention to various puzzles including those due to Richard (1983), Hector-Neri Castaneda (1966, 1967), and Saul Kripke (1979b) shows that we need to posit meanings that are even more "fine-grained" than those attributed by ordinary opaque ascriptions (4.14–4.17). Attention to the ascription of thoughts to foreigners shows that meanings ascribed by opaque ascriptions are usually a little more coarse-grained than previously suggested (pp. 233–5).

3   Very Fine-Grained Meanings and Their Ascription

Richard's initial criticisms arise from two puzzle cases, the kicked sheep and Kripke's Pierre (secs 6–7). He raises some interesting points about the descriptive issue, the issue of what we ordinarily ascribe in attitude ascriptions to explain behavior. He concludes that my view of these ascriptions is totally wrong. He takes this discussion to support his critical point (1) against my position on the basic issue of what meanings are (p. 88); "it undermines Devitt's account of meaning" (p. 96). I can have no objection to this way of proceeding since it accords with my second methodological proposal that we should look to the descriptive issue for evidence on the basic one. However, I think he is mostly wrong on the descriptive issue and very wrong on the basic one.

I shall discuss his case of the kicked sheep. The methodological proposal that we should look to the descriptive issue for evidence on the basic one is not to say that we should look *only* there. In this case, I think it is better to consider what really does the explanatory job rather than folk opinions of what does. So I shall start by setting the descriptive issue totally aside. Initially, I shall make no appeal to any view of ordinary attitude ascriptions, nor will I even use any of those ascriptions.

I shall also set aside—this time until the end of the section—consideration of the psychological generalizations that might cover this case, Richard's "warhorses." I shall focus instead on the particular explanation that applies. What really does explain why Smith kicked that sheep?

The kicking was caused by a belief *prompted by sight and sound* of the sheep, a belief that Smith would express: "That sheep is going to nibble my shoe." Call this a "sight-belief" (ignoring sound to simplify). Earlier, so the story goes, "Smith smelled the sheep he eventually kicked, knew he was smelling one or another sheep, but was oblivious to which, of the many sheep he saw, he smelled" (p. 93). Clearly, for explanatory purposes, we need to distinguish his sight-belief from a "smell-belief" he does *not* have: one *prompted by the smell* of the sheep that he would express in the same way. An essential part of what was causally efficacious

about the sight-belief—an essential part of what caused the intention to kick and hence the intentional behavior of kicking—was that the belief was prompted by the sight of the sheep. It is a commonplace that a causal explanation holds only "under a certain description." In this case, the description is 'that sheep that Smith sighted' or 'that sheep, qua sheep that Smith sighted.' The explanation is good only where the belief and intention are of the sheep under that description and the intentional behavior involves the sheep under that description.

Although I think that this is clearly so, Richard's endorsement of what he takes to be commonsense psychological explanations (pp. 93–95) shows that he would deny it. So, I shall argue for it by embellishing the story in the following way: The kicked sheep is Smith's pet, but Smith does not recognize her by sight because he is short-sighted. Perceiving a sheep that he recognized as his pet could never prompt him to a belief he would express, "That sheep is about to nibble my shoe," nor to an intention he would express, "I will kick that sheep." Though short-sighted, Smith has an unusually acute sense of smell and so earlier recognized his pet by her smell. Having thus recognized her he set out to pat her but at the time of the kicking incident thinks that he has so far failed to locate her in the crowd. So Smith has two "contradictory" sets of thoughts about his pet. The first are sight-thoughts he would express:

> That sheep is not my pet
> That sheep is about to nibble my shoe
> I will kick that sheep
> I will not pat that sheep

The second are smell-thoughts he would express:

> That sheep is my pet
> That sheep is not about to nibble my shoe
> I will not kick that sheep
> I will pat that sheep

And we have two "contradictory" behaviors to explain, a kicking and an attempted patting. The sight-thoughts explain the kicking and they do so in virtue of a property of their tokens of 'that sheep' that distinguish them from the tokens of 'that sheep' in the smell-thoughts. It is not simply in virtue of being thoughts prompted be *some sort* of perception of the pet that the sight-thoughts explain the kicking, for the smell-thoughts are also prompted by some sort of perception of the pet and they cause other behavior and discourage kicking. Similarly, the smell-thoughts explain the attempted patting in virtue of a property of their tokens of 'that sheep' that distinguish them from the tokens of 'that sheep' in the sight-thoughts.

My theory makes the needed distinctions (pp. 235–6). I think that tokens of 'that sheep' in the sight-thoughts have a very fine-grained Representationalist

meaning: the property of designating the pet sheep by a visual demonstrative mode. This very fine-grained meaning differs from that of tokens of 'that sheep' in the smell-thoughts which have the meaning: the property of designating the pet sheep by an olfactory demonstrative mode. In my terminology, these two modes are, for Smith, ununified species of the general demonstrative mode, a mode involved in a less fine-grained meaning that all these tokens share. A meaning that explains behavior must involve a mode with no ununified species (pp.231–2). So the meaning involving the general demonstrative mode will not explain the kicking: we need to posit the very fine-grained meaning involving the visual demonstrative mode.

All this is concerned with the basic issue of what properties *do* explain behavior; hence with the normative issue of what properties we *should* ascribe for that purpose. I have not yet touched on the descriptive issue of what properties *the folk* ascribe to these tokens for that purpose—the focus of Richard's discussion—nor have I appealed to any view of the ordinary attitude ascriptions that do the ascribing. To this I now turn. We must see whether findings on the descriptive issue undermine the view on the basic issue.

Richard thinks that the folk would explain the kicking by:

(4) Smith kicked that sheep because he thought that sheep was going to nibble his shoe, and he believed that kicking it would stop it from nibbling.

According to my initial Quinean theory of attitude ascriptions the tokens of 'that sheep' in the causal clause of (4) do not specify the property of referring to the sheep by the visual demonstrative mode but rather that of referring to it by the general demonstrative mode. So, given what I have just argued, (4) comes out false. Richard is certain it is true. He thinks that there are many similar cases of commonsense psychological explanation and that they provide decisive evidence against my account of meanings: the folk successfully explain behavior without ascribing the very fine-grained meanings that I have claimed do the explanatory work in such cases.

I may seem to have an easy response. I contemplate, and mildly prefer, a modification of my initial Quinean theory of attitude ascriptions (pp. 221–2). This modification is a minor move toward what I call "the Hidden-Indexical" theory and what Richard calls "contextualism." According to the modification, in certain unusual cases the *context* determines that a very fine-grained meaning is ascribed. So if (4) is one of those cases, specifying the very fine-grained meaning involving the visual mode, the folk use of (4) could be no threat to my account of meanings.

Richard has a very good objection to (4) being such a case. If the context plays this role it must surely do so in virtue of the speaker's intending to specify the very fine-grained meaning *rather than the normal one*. He could hardly intend this if he is "unaware of the failure of unification" (p. 95), as he may well be in

this situation. Furthermore, Richard points out, such cases of failure of unifica-
tion that are unknown to the speaker are not unusual.

   So the modification is no help with (4). Although ordinary opaque ascriptions
like (4) will normally explain behavior, I must accept that they will not do so
where there is an unknown failure of unification: (4) is indeed false. Furthermore,
I agree with Richard that the folk would offer (4) initially to explain the kicking.
Do cases like this count decisively against my account of meanings, as Richard
supposes? I think not.

   First, I disagree with Richard that the folk would regard the smell-thoughts
and the failure of unification as "completely irrelevant" to the success of (4) as an
explanation (p. 94). Thus, I think that if my embellished story were brought to
their attention, the folk would generally be very uneasy about (4) and likely (per-
haps after some coaching on qua-clauses) to assent to a replacement along the
following lines:

   Smith kicked that sheep, qua sheep that he sighted but not smelled, because
   he thought that sheep, qua sheep that he sighted but not smelled, was going
   to nibble his shoe, and he believed that kicking it would stop it from nibbling.

I think that this is particularly likely if the folk were *also* asked to explain the
attempted patting, a "contradictory" behavior. For that explanation I think that
they would assent to something along the following lines:

   Smith attempted to pat that sheep, qua sheep that he smelled but not sighted,
   because he thought that sheep, qua sheep that he smelled but not sighted, was
   his pet.

   Second, although I do not suppose that every member of the folk would be
uneasy about (4) and could be brought to the accept the above replacement, I do
not see that this provides any persuasive evidence against my account of mean-
ings. First, (4) is close to being right and I suspect that "near enough is good
enough" may be a guiding attitude of many folk explanations. Second, the case is
very subtle, as philosophers' fascination with similar puzzles like Richard's and
Kripke's attest. In the light of this, and my argument on the basic issue, I am quite
happy to say that the folk who stick with (4) in these circumstances are simply
wrong. And I am happy to say this also about Richard for endorsing such obsti-
nacy.[6] Although the second methodological proposal instructs us to look to ordi-
nary ascriptions for evidence of meanings, it does not instruct us to look
uncritically.

   Kripke's case of Peter and Paderewski poses the same apparent problem for
my account, as Richard notes (p. 90), and I would give the same response (pp. 236–
8). Kripke's case of Pierre and London is also similar if 'London' in an ordinary
attitude ascription specifies the "disjunctive" mode of referring to London by
'London' or 'Londres' or... . In *Coming* I claim that 'London' usually but not

always specifies this disjunctive mode (pp. 233–5). As a result of reflecting on Richard's discussion, I am now inclined to think that this claim is too simple, at least. However, this delicate issue is largely irrelevant to my account of meanings.

Finally, it is worth noting that Richard, in his use of his term "d-senses," gives a rather misleading impression of the account of meanings that is at stake here. He tends to magnify *Coming*'s commitment to causal theories of meaning in general and to the sort of causal theory I urged in *Designation* (1981a) in particular. In *Coming* (chs. 1 and 3) I defend a *molecular* localism according to which description theories of meaning *may* cover many terms and I predict, although I do not argue, that such theories *will* cover many. I argue that description theories "pass the referential buck" and so some terms must be (at least partly) covered by causal theories (pp. 160–2). But I do not argue which ones are. I do think that a causal theory along the lines of *Designation* is a large part of the truth for names, demonstratives, and some other singular terms (p. 169). But *Coming* does not argue for this theory and uses it for illustrative purposes only. I do argue that any meaning of a word is constituted by properties that go into determining its reference—that is my Representationalism—but I do not argue for a detailed causal or descriptive theory of any particular meaning. So what is *not* at stake in this section is some causal theory of fine-grained and very fine-grained meanings.[7] What *is* at stake is whether there are such meanings and whether they are to be explained in terms of modes of reference (that may be causal, descriptive, or partly both).

The discussion has all been about explanations of behavior. What about the psychological generalizations, Richard's warhorses? For me, as Richard suggests (p. 93), they are true only if restricted one way or another so that the modes involved in the meanings that the laws advert to have no ununified species (pp. 231–2).

In sum, Richard's interesting discussion of ordinary attitude ascriptions in some puzzling cases includes a mistake that vitiates his argument against my account of meanings. The mistake is to suppose that the folk find the failure to unify modes of referring irrelevant to the explanation of behavior. More importantly, Richard makes the mistake of attending only to ordinary attitude ascriptions. These reveal folk opinions about the explanation of behavior. We need also to consider what really does explain behavior. Attention to this in the puzzling cases confirms that the job is done by meanings involving very fine-grained modes of referring.

4 Two-factor Theory

Richard extends my talk of the unification of modes of referring to talk of the unification of meanings and tokens. In criticism (2), he uses this extension and my methodology to argue that, contrary to what I claim, I am committed to a two-factor theory according to which one factor of a token's meaning—or, as I prefer to say, one of its meanings—is a non-reference-determining inferential role (sec. 8). Insofar as I follow this argument—which is not far, I'm sorry to

say—I find it way off track, reflecting a mistaken view of the place that talk of unification plays in the application of the methodology. Richard may have been misled by my normative talk of the properties we should ascribe to explain behavior. I shall try to clarify that talk.

My core argument is that behavior is partly explained, ultimately,[8] by a fine-grained property ascribed by an ordinary opaque attitude ascription. So that property is a meaning. I equate this claim on the basic issue with one on the normative issue: the fine-grained property is a meaning because it is one we ought to ascribe to explain behavior (first methodological proposal). What does this normative claim mean? Clearly not that we ought to ascribe that particular fine-grained meaning to explain just any old behavior: the meaning may be totally irrelevant to the behavior (the token in my belief box meaning **THE ORIOLES HAVE HAD A POOR SEASON** was irrelevant to my choosing a certain tomato at today's market). The normative claim means that the property is a meaning because it is one we ought to ascribe to explain behavior *in certain circumstances*. Which circumstances? There must be some "matching of meanings: the matching of a meaning in the intention that must cause the behavior to be explained with a meaning in the explaining thought" (p. 231). The core argument leads us to expect that these matched meanings will be the fine-grained ones ascribed by ordinary opaque ascriptions. But consideration of some puzzle cases shows that this is not always so: in those cases the matched meanings must be *very* fine-grained meanings. Why is that? Why can we mostly explain behavior with a matched fine-grained meaning but sometimes require a matched very fine-grained one? The talk of unification plays a role in answering this question: "a mode involved in a matched meaning must have no ununified species" (p. 232). Fine-grained meanings usually meet this test but in some puzzle cases only very fine-grained meanings do. In brief, we talk of matching and unification to characterize the circumstances in which we ought to ascribe a given meaning to explain behavior.

That is the place of the talk of unification. The talk has no place in explaining the nature of these meanings—fine-grained and very fine-grained alike—that can explain behavior where they are matched and unified (the basic task). Take one of these meanings. What is common and peculiar to all the tokens that have it? The answer, I argue, is a certain mode of referring. Whether this is so or not, one thing is certain: a token's property of being Unified with others, in Richard's sense, can be no part of the answer. For, tokens in the puzzle cases have a fine-grained opaquely ascribed meaning *and yet are not Unified*. For example, consider 'that sheep' in Smith's mental token, 'That sheep is about to nibble my shoe.' It has the fine-grained meaning of referring to the sheep by the general demonstrative mode (referring by some demonstrative or other) but is not Unified with many other of Smith's tokens of 'that sheep.' So being Unified is certainly not common to all tokens that have that meaning and can be no part of the nature of the meaning.

In sum, a meaning is a property we should ascribe in certain circumstances to explain behavior. We explain the circumstances in which we should and should not ascribe it for that purpose in terms of matching and unification. This talk of

matching and unification has nothing to do with explaining the nature of the meaning, a meaning that may explain behavior where there is matching and uni-fication but otherwise does not. Explaining the circumstances in which a meaning will do an explanatory job and explaining the nature of that meaning are different matters. Talk of unification contributes to the former not the latter explanation and so could supply no evidence one way or the other on the two-factor theory of the nature of that meaning.

5   Contextualism

My Quinean view of attitude ascriptions like

(1) Ralph believes that Ortcutt is a spy

allows that they are mildly context dependent in that the context determines whether they are transparent or opaque. Richard, and others, claim that these ascriptions are much more radically context dependent. I argue for two con-straints on such a theory, the second of which is: "The theory must explain how hearers use linguistic conventions together with accessible context to understand ascriptions" (p. 199). Speakers usually, although not of course always, under-stand ascriptions. So if something in the context partly determines the meaning of an ascription it must be something that is accessible to the hearer in normal cir-cumstances. Richard wonders whether unvoiced speaker's intentions are, on my view, accessible (pp. 103–4). And the answer is: "Some are and some aren't." I think that intentions often play a role in determining meaning. In particular, they disambiguate names; thus, my uses of 'Mark' in delivering this paper referred to Mark Richard not Mark Anthony because I intended them to. And it would be very easy to spell out the features of that context that made this intention acces-sible to the audience. In contrast, Humpty Dumpty's intention to mean **THERE'S A NICE KNOCK-DOWN ARGUMENT** in saying "There's glory for you" was not accessible to Alice. A consequence of my constraint is that a contextualist theory must not claim that the meaning of an ascription is determined by inten-tions that are like Humpty Dumpty's.

One criticism I make of contextualism is that known versions of it do not show how they meet this constraint. And it seems unlikely that they can. Another crit-icism is that contextualism is unmotivated. If the theory is right, then, as Richard agrees, "a use of (1) might be false *even though* Ralph has a belief he expresses with 'Ortcutt is a spy'" (p. 101). I was unable to imagine such a case (p. 204). In presenting criticism (3), Richard produces an ingenious example involving a slightly different ascription that seems to be a case. And his discussion suggests how contextualism might meet my constraint.

In his example, Ralph and Mary observe, but do not recognize, a person later discovered to be Ortcutt acting as if he was about to set a factory on fire. Ralph calls the police. Mary partly explains this action by saying,

(1a) Ralph believes that Ortcutt is an arsonist.

Richard claims that Mary's use of 'Ortcutt' in the circumstances specifies the visual mode of referring to Orcutt not the mode of referring to him by the name 'Ortcutt.' So, even if Ralph did have a belief that he would express, 'Ortcutt is an arsonist,' (1a) would be false if he did not also have a belief prompted by sight of the person in the bushes that he would express, 'That's an arsonist.'

In this example, a name in an attitude ascription seems to specify a demonstrative mode. And the example meets my constraint by providing some "stage setting" for (1a) that makes the visual mode salient. This may seem to be persuasive evidence for contextualism. I have two comments on it.

First, we should note that this sort of contextualism seems not to be the radical departure from my Quinean view that Richard suggests. On my view, the mode specified by a singular term is a mode of that very term; for example, the mode specified by 'Ortcutt' is the mode of referring by that very name. And, so far as I can see, the same is true also on this contextualism *unless there is some special stage setting to make another mode salient* (cf. p. 104). So, very likely, the two views will agree in their assignment of truth conditions in the great majority of cases.

Second, it is not as obvious as it may seem that examples like this are evidence for contextualism. The situation is complicated, in interesting ways. To see this it helps to consider an analogous dispute.

This dispute was generated by Keith Donnellan's famous article about definite descriptions (1966). This article influences some philosophers to conclude that descriptions are "ambiguous," sometimes having a Russellian "attributive" meaning in which they function like a quantifier and sometimes a very different "referential" meaning in which they function like a demonstrative. Other philosophers, notably Kripke (1979a), draw on a Gricean distinction between what a token expression literally or conventionally meant—the proposition expressed—and what a speaker meant by the expression—the proposition meant—to reject the ambiguity thesis. Thus, consider the following passage from Stephen Neale:

Suppose it is common knowledge that Smith is the only person taking Jones' seminar. One evening, Jones throws a party and Smith is the only person who turns up. A despondent Jones, when asked the next morning whether his party was well attended, says,

(7) Well, everyone taking my seminar turned up

fully intending to inform me that only Smith attended. The possibility of such a scenario, would not lead us to complicate the semantics of 'every' with an ambiguity; i.e., it would not lead us to posit semantically distinct quantificational and referential interpretations of 'everyone taking my seminar.' (1990: 87–8)

The moral Neale draws is that the possibility of Donnellan's scenarios should not lead us to complicate the semantics of 'the *F*.' He goes on to argue that Grice's pragmatic theory of "conversational implicature" (1989) with its Cooperative Principle and Maxims of Quantity, Quality, Relation and Manner can explain all these scenarios.

Similarly, we might draw the moral that the possibility of Richard's scenarios should not lead us to complicate the semantics of attitude ascriptions; conversational implicature will do the explanatory work. Just as 'every' is a quantifier even though, given a suitable stage setting, a speaker can successfully use it to refer to a particular person in mind, similarly, the singular term in an attitude ascription specifies the mode of that very term even though, given a suitable stage setting, a speaker can successfully use it to specify a different mode. Although what Mary meant by (1a) was that Ralph had a belief prompted by sight of the person in the bushes that Ralph would express, 'That's an arsonist,' what (1a) literally or conventionally meant was that Ralph had a belief he would express, 'Ortcutt is an arsonist.' Sentences can convey meanings that they do not themselves have.

Have we then shown, contrary to what Richard is suggesting, that his examples are not really evidence for contextualism after all? Not so fast. Return to the dispute over definite descriptions. Many philosophers have found reasons for not giving up the ambiguity thesis in the face of the Gricean point. Perhaps the contextualist can do likewise.

I am in an interesting position here because I subscribe to the ambiguity thesis about descriptions *despite* the Gricean point and yet I am inclined to go against Richard's contextualism *because of* the Gricean point.

Here is my response to the Gricean point about descriptions.[9] The basis for the ambiguity thesis is not simply that we *can* use a description referentially, it is that we *regularly* do so. When a person has a thought with a particular *F* object in mind, there is a regularity of her using the physical forms of 'the *F*'—for example, the sound /the *F*/—to express it. This is strong evidence that there is a convention of using 'the *F*' to express a thought about a particular *F*. This convention is semantic, as semantic as the one for an attributive use. In each case, there is a convention of using the physical forms of 'the *F*' to express a thought with a certain sort of meaning.

A similar contextualist response to the Gricean point about ascriptions might run as follows. When a person wishes to ascribe a thought about an object under a certain mode of referring, and that mode has been made salient by stage setting, then there is a convention that allows her to use any singular term that refers to that obect to specify the mode. This convention is semantic.

I doubt that this contextualist response works because I doubt that the convention is semantic. But then in what respect does it differ from the convention for descriptions that I claim is semantic? What makes a convention semantic not pragmatic? What is the theoretical significance of this question? These are diffi-

cult matters. This is clearly not the place to go into them thoroughly, but here are some quick thoughts.

(i) Gricean discussions do not cast nearly enough light on the questions. These discussions have much to say about the pragmatic conventions of conversational implicatures but little to say about what makes other conventions semantic and not pragmatic. The view that they are semantic seems to be based on intuition rather than on theory.[10] This is particularly worrying given that the Gricean account of pragmatic conventions seems to fit some cases that we would surely want to treat as semantic conventions. Think of metaphors, paradigms of conversational implicature. It is common for an expression to have once had a certain meaning metaphorically and then, as a result of regular use, now to have that meaning conventionally. The Gricean account of it as a metaphor—including the constraints of cancellability and derivability—will *still* apply when it is no longer one. It has ceased to be a metaphor not because that account *does not* apply but because another account—the correct one for a semantic convention, whatever that may be—*does* apply to it. Grice's "Modified Occam's Razor," "Senses are not to be multiplied beyond necessity" (1989: 47), pushes us in the wrong direction here.

(ii) *Coming* is not much help with the problem either. Its focus is on speaker meaning, because that meaning is immediately indicative of thought meaning which directly plays the semantic roles. I do say that the conventional meaning of an utterance is of interest to us because it is indicative of the utterance's speaker meaning and hence of its underlying thought meaning (p. 226). But this clearly is no help to us here because it would apply equally to the allegedly pragmatic conventions. I point also to the ordinary locution that folk use to distinguish the conventional from the speaker meaning of an utterance: "says that...but...means that" (p. 58n). This does seem to put the folk against the Richard contextualist: it seems likely that the folk would say: "Mary said that Ralph called the police because he thought that Ortcutt was an arsonist but she meant that he did it because he thought that the person in the bushes was an arsonist." Still, even if people would say this, it is far from sufficient to reject contextualism: we need theoretical support for this application of the distinction.

(iii) It is noticeable that the description of the convention in the contextualist's response, unlike in my response in the descriptions case, mentions the reference of a word ("a convention that allows her to use any singular term that refers to that object to specify the mode"). So the convention rests on a prior semantic convention. Does this provide a principled basis for ruling that the former convention is not semantic? I cannot say so because I am committed to treating the following convention as semantic: the convention of using a term that refers to a certain object to ascribe a thought about that object under the mode of that term. One semantic convention can be built on another.

(iv) I think that the difference between semantic and pragmatic conventions lies in differing roles of the context. In the case of the semantic convention for a

referential description, the context plays an immediate role in determining the meaning of the underlying thought, and communication succeeds because that role is salient. But that is *all* the context does. So there is no need to mention the context in stating the convention: it is the convention "to use the physical forms of 'the *F*' to express a thought with a particular *F* in mind." The convention is not one of using the context in some way to convey the (context-determined) meaning. In contrast, pragmatic conventions essentially involve using the context in some way to express a thought meaning; thus, in our contextualist example, the convention is of using the fact that a certain mode *has been made salient in the context* to convey the meaning that a thought is under that mode.[11]

This is what inclines me to think that Richard's nice example does not, after all, support contextualism. It is indeed an example of using a name to specify a demonstrative mode but this is a pragmatic matter not a semantic one.

6   The Meanings of Name Types

In criticism (4), Richard rejects my claim that "nothing very significant about meaning is at stake" in my disagreement with a contextualist. He claims that a view of the meaning of name types is at stake (sec. 12). He rightly takes my view of name types to be a compromise between direct-reference and Fregean views: a name type has two meanings, one the property of referring to its bearer and the other of referring to its bearer by the mode of that very name. He rejects this compromise in favor of the direct-reference view. I have two comments.

First, the contextualism he uses to support his claim that a view of names is at stake is different from the one that my claim is about. My claim concerns a contextualism that agrees with my Quinean view that the opaque attitude ascription

(M) Mary believes that Sterelny is a philosopher

ascribes a property of referring to Sterelny by a certain mode. So it agrees with my Representationalism about meanings. This contextualism disagrees with my view that (M) always ascribes a property involving the mode of 'Sterelny,' claiming instead that the ascribed property might involve any mode of referring to Sterelny. The contextualism Richard uses is much more extreme: the ascribed property might not involve *any* mode of referring to Sterelny and might involve some non-reference-determining functional role; for example, the property of being involved in Mary's hitting Sterelny. This disagrees with Representationalism and so there is of course something very significant at stake in a disagreement with it. And I recognize this significance in arguing vigorously against it (pp. 206–7; secs. 4.9 and 4.10).

Second, and more important, a key assumption in Richard's argument for his direct-reference view of name types is that "on an extreme contextualism there is *no end* to the properties we might ascribe, in the course of attitude ascription, to 'Sterelny' tokens naming Sterelny" (p. 109). So, he argues, we should identify the

meaning of the name type 'Sterelny' with what all these tokens have in common, the property of referring to Sterelny.[12] I think that this final step should be rejected as theoretically *ad hoc*, but my present comment is on the key assumption. That assumption reflects a mistaken view of contextualism, confusing meaning ascriptions with the meanings they ascribe:[13]

> On an extreme contextualist view, for pretty much any property P a mental token of 'Sterelny' might have, we might ascribe...*referring to Sterelny and having P* to a mental token of 'Sterelny' in the course of ascribing an attitude to someone. And we *ought* to do this, given that our purpose in ascribing attitudes is to explain or rationalize behavior. (p. 108)

So, according to Richard, it *follows* from contextualism that some mental token *of 'Sterelny'* has such a property and that it really does explain behavior. *But this does not follow at all: it is a gratuitous addition to the characterization of contextualism.* Take (M) as our example and assume that it is true. Extreme contextualism is the view that 'Sterelny' in the t-clause of (M) ascribes, for the purposes of explaining behavior, the property of referring to Sterelny and having the property P, where the value of 'P' varies from context to context. Let us fix the context and hence the property that is the value. *It does not follow that any token of 'Sterelny' has that explanatory property. Indeed, nothing follows at all about which sort of token might have the property and make (M) true.* Contextualism is a thesis about what properties t-clauses ascribe not about which tokens have those properties. A thesis about the latter requires further argument (that Richard does not give). Thus, so far as contextualism is concerned, if it were the case that a mental token explained Mary's behavior in virtue of having the property of referring to Sterelny and being involved in hitting Sterelny, that token might not be of 'Sterelny' but of 'that Aussie bastard,' 'the hairy wine drinker in the corner,' or whatever. Furthermore, the examples that usually motivate contextualism give no support to what Richard is suggesting about the meanings of names. Thus, according to Richard himself, the mental token of Ralph that makes

  (1a) Ralph believes that Ortcutt is an arsonist

true in the earlier story is a token of 'That's an arsonist,' which does not involve 'Ortcutt' at all (pp. 101–2).

   In brief, contextualism is a theory about what is ascribed not about what has what is ascribed. The key assumption in Richard's argument is false.

   In conclusion, I think that my general Representationalist theory of meaning comes unscathed through Richard's four criticisms. However, I think that his defense of contextualism in criticism (3) raises a genuine doubt about my Quinean view of attitude ascriptions. Even so, I expect that a closer look will show that the phenomena here are pragmatic not semantic.

Notes

[1]This paper is a modified and expanded version of a response to Richard I made at American Philosophical Association Convention in Seattle, April 1996. Many thanks to Georges Rey for comments on the penultimate version of this paper.

[2]Not quite. A sentence represents some situation that would make it true, complied with, or whatever, but it also asserts that the situation obtains, requests that the situation be brought about, or whatever; a sentence has "a force." I overlook these forces.

[3]This chapter is based on my 1994.

[4]"The chief problem about semantics comes at the beginning. What is the theory of meaning a theory of?" (Higginbotham 1991: 271). "Meaning is notoriously vague" (Block 1986: 615). Lycan has brought out the problem wittily with his "Double Indexical Theory of Meaning": "MEANING $=_{def}$ Whatever aspect of linguistic activity happens to interest *me now*" (Lycan 1984: 272).

[5]See particularly, Quine 1953: 139–59; 1960: 141–51, 166–9; 1966: 183–94; Kaplan 1968.

[6]Richard claims that this case "is very much on all paws with cases in which a believer fails to unify distinct proper names of the same object." He goes on to imagine a case where the sheep smelled is dubbed "Stinky" and the sheep sighted "Lubby" (pp. 111–112n). When we consider how fine-grained the meaning must be that does the explaining, the cases are indeed on all paws. But the cases differ in that in the latter case but not the former the folk have an easy way of specifying that meaning.

[7]See particularly pp. 239–40.

[8]I include the "ultimately" because coarse-grained properties of simply referring to an object, ascribed by ordinary transparent ascriptions, can explain behavior (p. 153).

[9]One might generously see this response as implicit in my 1981a: 50–4; 1981b: 516–20.

[10]See, for example, Neale 1990: 74.

[11]This will not support treating Grice's "generalized" conversational implicatures—like those associated with 'and' and 'or'—as not semantic, because these do not appeal to particular details of the context.

[12]Note that Richard's argument is *not* that we should identify the meaning of 'Sterelny' with what is common to all its tokens *in attitude ascriptions*. That identification is wrong for other reasons (and also gets no support from my methodology).

[13]The analogous confusion of ascriptions of beliefs with the beliefs ascribed, reflected particularly in the use of the terms '*de dicto*' and '*de re*,' is very common. For discussion see *Coming*, sec. 3.9.

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