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DEFERENCE AND THE USE THEORY

Michael Devitt

Abstract

It is plausible to think that members of a linguistic community typically mean the same by their words. Yet “ignorance and error” arguments proposed by the revolution in the theory of reference seem to show that people can share a meaning and yet differ greatly in usage. Horwich responds to this problem for UTM by appealing to deference. I give five reasons for doubting that his brief remarks about deference can be developed into a satisfactory theory. But this appeal has an even deeper problem: the appeal is inconsistent with UTM. These problems are not minor ones of details: they strike at the very core of UTM.

I. Introduction

This paper is a criticism of Paul Horwich’s *Reflections on Meaning* (2005) chapter 2, “A Use Theory of Meaning”, which develops a theory, “UTM”, presented in *Meaning* (1998b), and responds to some criticisms, including mine in “Meaning and Use” (2002).¹

It is plausible to think that members of a linguistic community typically mean the same by their words. Yet “ignorance and error” arguments proposed by the revolution in the theory of reference started by Saul Kripke (1980) seem to show that people can share a meaning and yet differ greatly in usage. Horwich responded to this problem in *Meaning* by appealing to deference. I pointed out that the major problem with his appeal is the lack of details about deference. My first criticism of *Reflections* is that it does not provide the necessary details. My second criticism consists in five reasons for doubting that Horwich’s brief remarks about deference can be developed into a satisfactory theory. My final criticism points to an even deeper problem : the appeal to deference is inconsistent with UTM. These problems are not minor ones of details: they strike at the very core of UTM.

1 Horwich’s deflationary view of truth, presented in his influential book, *Truth* (1998a), is an important background to his view of meaning. On a deflationary view, crudely, truth isn’t anything. I have attempted to give a non-crude characterization of what the deflationist should say about *truth* (2010: 155–81).

2. Agreement

Let us start, however, with some important matters on which Horwich and I are in broad agreement.

First: We agree on the need to begin by identifying the meanings that it is the semantic task to explain; and we agree pretty much on how they are to be identified. Why do we have to start by identifying meanings? Because our ordinary way of identifying them, our ordinary talk of “meanings”, is very vague.² So how do we identify them? By their causal role. But here Horwich and I have a somewhat different emphasis. In *Meaning* Horwich puts his view as follows: “the meaning-constituting property should be identified with whatever best explains the word’s overall deployment” (1998b: 6); a version of this is the *first prong* of his “short crude statement of UTM’s two-pronged central thesis” in *Reflections* (2005: 28). The only deployment he mentions in *Meaning* is the acceptance of linguistic sentences containing the word. There is no explicit formulation of the use theory for the mental. Yet, as *Reflections* makes very clear, Horwich intends his theory to apply as much to the mental as to the linguistic.³ We can infer how this application to the mental goes by noting that the mental analogue of accepting a linguistic sentence is believing a mental one, “having it in the belief box”. So Horwich seems to be identifying a mental word’s meaning with that property of the word that best explains the presence of mental sentences containing the word in the belief box. Now this would identify the meaning of a mental word with a property that has a role in explaining why mental sentences containing it *are caused*. But, I argued, doesn’t this very same property also have a role in explaining what the sentences *cause*? For one, these sentences cause *linguistic* behaviors in virtue of their meanings. Thus, it is in virtue of a belief’s meaning IT IS RAINING that an English speaker expresses that belief by asserting “It is raining”. And why restrict ourselves to *linguistic* behaviors? Beliefs, and other thoughts, cause *nonlinguistic* behaviors in virtue of their meanings too: believing that it is raining not only leads me to say “It is raining” but also to take an umbrella. And causing nonlinguistic behaviors

2 “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).

3 In “Meaning and Use” I argued that he should give UTM for the mental *priority* over UTM for the linguistic (2002: 109–12). He rejects this idea (2005: 30–2, 57–62). I think he is wrong to do so, but I shall not argue the matter further.

seems more fundamental (think of animals and babies, for example). So, in “Meaning and Use” I claimed that we should broaden the identification of meaning: a mental word’s meaning is the property that plays a role in explaining not only why sentences containing the word are caused but also why they cause behaviors in general (2002: 112). Horwich clearly thinks along similar lines in *Reflections* (2005: 37–8) but his identification of meanings is still on the cause of sentences, on their acceptance or inclusion in the belief box, rather than on what they cause. In contrast my identification in *Coming to Our Senses* (1996: 2.3–2.6) was in terms of the behavior they cause. I corrected this later (1997), taking account of the role of meanings in explaining the causes of sentences.⁴

Second: Horwich rejects the “over-intellectualization” of linguistic competence (2005: 58), what I have labeled “Cartesianism” (1996). In his view, as in mine, this competence is “a matter of *knowhow*” (2005: 220).⁵

Third: We both seek a naturalistic reduction of meanings. Horwich asks the question:

Are we to suppose, moreover, that meaning-properties *reduce to*, i.e. *are constituted from*—use-properties? Or perhaps even that these properties are *identical*?” (2005: 32)

Horwich thinks so. Yet, strangely, he claims that UTM is “not intended to be part of science” (1998b: 87). I think that we should be seeking a scientific explanation (2002: 112). This will require that meaning-properties be identified with, or supervene on, more basic properties.

3. Disagreement

My main disagreement with Horwich is over the *second* prong of UTM. This prong explains the nature of the meanings identified by the first prong as:

an acceptance-property of the following form:—‘that such-and-such w-sentences are regularly accepted in such-and-such circumstances’ is the idealized

- 4 I also identified the meanings of mental sentences partly with their properties that enable them to inform us about the world.
- 5 Jason Stanley and Timothy Williamson (2001) have argued ingeniously for the surprising thesis that knowledge-how is really a species of knowledge-that. I have responded (2011).

law governing *w*'s use (by the relevant 'experts', given certain meanings attached to various other words). (2005: 28)⁶

So the idea is that our acceptance of *certain* sentences containing *w*—*w*'s "basic acceptance property"—explains our acceptance of *any* sentence containing it. In response I proposed a truth-referentialist view:

The alternative is a "moderate" truth referentialism along the following lines. "Primitives" get their meanings from referential relations explained by some sort of direct causal link to reality, an informational, teleological, or historical-causal link, or some combination of these; proper names and natural kind words are likely primitives. Other words get their meanings from referential relations explained by their "definitional" links; the words are inferentially associated with others that determine their reference; they are covered by "description" theories of reference; 'bachelor' is a likely example. (2002: 114).⁷

So whereas Horwich explains the meaning of a word as an acceptance property, I explain it as a referential property.

4. The Nonprimitives

I pointed out that a consideration of nonprimitive words is unlikely to settle the disagreement:

In assessing the relative merits of the use theory, it is important to note that, for many words, the theory will be hard to distinguish from moderate truth referentialism. The words in question are ones like 'bachelor' that truth referentialism treats as nonprimitive and covered by a description theory. Thus, where truth referentialism explains 'bachelor's' meaning by its reference-determining association with 'unmarried man', the use theory explains its meaning by its appearance in 'A bachelor is an unmarried man'. For such words, the only significant difference between the two theories is in their attitudes to reference: truth referentialism is committed to a substantial reference relation, the use theory, to deflationary reference. According to the use theory, meaning trivially determines reference; according to truth referentialism, meaning strongly determines reference In light of this, when we attend to words like 'bachelor' we are unlikely to find decisive evidence favoring one theory over the other (independent, of course, of evidence for or against a deflationary theory of reference). (2002: 114).

6 I have omitted an 'is' before the parenthetical clause because it is a typo.

7 I urge a view of this sort in 1996 and Devitt and Sterelny 1999. The naturalistically-inclined truth referentialist has to choose between some such moderate view and the extremism of semantic atomism (every word is a primitive).

Note, for example, that the two theories would do equally well at explaining the causal role of meanings for nonprimitives. Horwich thinks that what does the explaining is an acceptance property, “a certain *core* use” of a word, “the acceptance of a certain narrow set of sentences containing it” (2005: 42). Why is that plausible? Because of “a familiar *inferential model*. . . For it is clear how a basic propensity to accept certain sentences in certain conditions might, given environmental circumstances and the deployment of rules of inference, naturally bring about the acceptance of other sentences” (2005: 43). I think he is right. That narrow set of accepted sentences including the word would act like axioms involving the word which, together with other information, would lead to the acceptance of other sentences involving the word. But, of course, moderate truth-referentialism’s description theory will yield just the same axioms for nonprimitives; for example, it will require speakers to associate ‘bachelor’ with ‘unmarried man’ yielding ‘A bachelor is an unmarried man’.

So, anything that is to count decisively against UTM must be found in its handling of primitive words, the place where language ultimately links to the world.⁸

5. The Primitives

I think UTM fails very badly with the primitives. The root of its problem is to be found in the revolution in the theory of reference led by Saul Kripke (1980), Keith Donnellan (1972), and Hilary Putnam (1975). The revolution shows that certain words like proper names and natural kind terms cannot be covered by a description theory and so are, in effect, primitives. They can’t be so covered because competent speakers are often too ignorant or wrong about the referent to associate the appropriate descriptions. Horwich neatly describes the consequent problem for UTM: “members of a linguistic community typically mean exactly the same as one another by a given word, even when their uses of it diverge” (1998b: 85–6).

But how could this be so? Causal theorists propose an answer based on an idea briefly mentioned by Strawson in presenting his description theory of names. It is the idea of “reference borrowing” or, as it later came to be called, “deference”. Horwich also proposes an answer based on this idea.

8 There must be some primitives because not all words can have their meanings explained in terms of the meanings of other terms; not all words can pass the semantic/referential buck (1996: 159).

5.1. UTM's Solution: Deference

What is needed is, first, that there are acknowledged experts in the deployment of the term—experts whose usage is determined by some such regularity; second, that the individual is disposed to defer to the experts—i.e. to accept correction by them; and consequently, third; that his use of the term conforms to that regularity at least to some extent. (1998b: 86)

I criticized this proposal in ways that I will summarize in a moment. But my most important point was that the proposal “requires an *awful* lot more explanation and defense” than Horwich gives it (2002: 118); it is “seriously incomplete” (p. 119). Horwich is strikingly unmoved by this point. His brief discussion of deference in *Reflections* (2005: 51–3) offers nothing more in the way of an explanation or defense of his proposal. He frankly admits that his few remarks “leave many questions unanswered” including the question, “What exactly is the phenomenon of deference?”, but comforts himself with the thought that “*any* account of meaning-constitution will have to confront them, and none is peculiarly well placed to respond” (2005: 53). This is way off base.

(1) I think that causal theorists are in a good place to explain deference or reference borrowing. And I think, immodestly, that *I have gone some way toward actually doing so* in the process of proposing a theory of reference (1981). (2) In contrast, the prospects for a UTM explanation are very dim. The problem for UTM is not a matter of filling in some minor details but one that strikes at the very core of the idea that use explains meaning. When it comes to deference, we are not all in the same boat, as Horwich is suggesting. And his boat is leaking badly and doomed, I think, to sink.

5.2. The Causal Theory of Reference Borrowing

Concerning (1), consider the causal theory for names. As Kim Sterelny and I emphasize, it has two parts, a theory of reference *fixing* and a theory of reference *borrowing*. Reference fixing takes place initially at a dubbing in which the name is causally “grounded” in an object in virtue of that object being the object perceived in the dubbing. Witnesses to the dubbing can thereby gain the ability to designate the object with the name (1999: 66–7). And it is important to note that, typically, reference is also fixed in subsequent perceptions of the object that prompt the use of the name: the name is “multiply grounded” in the

object (pp. 75–6).⁹ What about reference borrowing? We state the basic idea as follows:

People not at the dubbing acquire the semantic ability from those at the dubbing. This acquisition is also a causal, indeed perceptual, process. The name is used in conversation. Hearers of the conversation, if of suitable linguistic sophistication, can gain the ability to use the name to designate the object. The exercise of that ability will designate the object in virtue of a causal chain linking the object, those at its dubbing, and the user through the conversation. (1999: 67)

Those who have borrowed reference can then lend it to others. Thus, our competence with ‘Aristotle’ is the result of centuries of reference borrowings that take us back to Aristotle’s contemporaries who grounded the name many times in the famous philosopher.¹⁰

I have recently (2006b) added some more details in responding to criticisms by Dunja Jutronic (2006). Clearly reference borrowing is in some sense an intentional act. This is not to say that the borrower forms an intention deploying a concept of reference borrowing or deference: that would be far too intellectualized a picture of the process. Still the borrower must process the input supplied by the situation in whatever way is appropriate for gaining, or reinforcing, an ability to use the name to designate its referent. The borrower must intentionally set in motion this particular sort of mental processing even though largely unaware of its nature and perhaps not conscious of doing so. So, reference borrowing is not just any old causal process in the communication situation: it is a special one involving that particular mental process.

It is important to note some things that are *not* required by the theory. If a person’s current use of a name is to designate its bearer then that use must be caused by an ability with that name that is, *as a matter of fact*, grounded in the bearer whether via reference borrowing or directly by the person herself: *the efficacious mental state must have the right sort of causal history*. If it has the right history, that is sufficient. Contrary to what Thomas Blackburn (1988: 184) claims we need not require a borrower to recognize or acknowledge this history. And contrary to what Adele Mercier claims (1999), we need not require that a

- 9 We think that the “*qua*-problem” poses a serious problem for this theory of reference fixing. (1999: 4.5)
- 10 We think that a “pure-causal” theory of reference borrowing is appropriate for names and natural kind terms but contemplate a “descriptive-causal” theory for some others. A descriptive-causal theory requires a competent borrower to be not only appropriately connected causally to the referent but also to associate a certain description, albeit not an identifying description, with the word; for example, requiring the borrower of ‘sloop’ to associate ‘boat’ with it. (1999: 5.5).

borrower' successful use of a name be accompanied by an intention to defer. The speaker intentionally exploits an ability that is, *as a matter of fact*, borrowed but the speaker need not intentionally defer to the lender. Indeed, the speaker need not know who the lender was or even that she *has* borrowed the term. There is no need for her to have any semantic thoughts about the term at all. Use of language does not require any thoughts about language.¹¹

I have always preferred the expression 'reference borrowing' to 'deference' to capture the way in which speaker *x*'s use of a term can depend for its reference on speaker *y*'s. My reason is implicit in the above discussion. If *x* borrows the reference of a term from *y* then that is an intentional act at the time of receiving *y*'s communication. In contrast, if *x* defers to *y*'s use of a term then that is likely to be an intentional act at the time of *x*'s using the term herself to communicate. In my view, *x*'s dependence on *y* *must* involve an act of the former sort but *need not* involve one of the latter sort. A communication involving a borrowed term need not involve any intention that "looks back" to the occasion of borrowing.¹²

Indeed, where did the idea first come from that a causal theory of reference borrowing must involve a backward-looking intention? And where did the talk of "deference" first come from? Kripke follows Strawson in talking of "borrowing the references" (1980: 90). He does not talk of the speaker "deferring" at the time of usage, nor mention any backward-looking intention. Rather, he says, a person in borrowing a name "must, I think, intend *when he learns it* to use it with the same reference" (p. 96, emphasis added). Yet Searle misconstrues this passage as requiring a backward-looking intention: a person who has borrowed the reference of a name must, *at the time of using it*, intend to refer to the same object as the person from who he borrowed the name (1983: 244). Donnellan does not use the terms 'deference' or 'reference borrowing'. He talks of our uses of proper names being "parasitic on uses of the names by other people." He emphasizes that "the history behind the use of a name may not be known to the individual using it" (1972: 373), implying that no backward-looking intention is required. Nor does Putnam talk of "deference" or backward-looking intentions in his introduction of the "division of linguistic labor" (1973: 705). Later, however, he does talk in ways that might be taken to suggest such intentions: "my denotation may be, by general consent, the denotation assigned by persons distant from me in space and even in time, but linked to me by relations of

11 This is a central theme of my book, *Ignorance of Language* (2006a).

12 Jutronic (2008) now accepts that the use of a name in communication need not involve any backward-looking intention and she no longer endorses the criticisms of Blackburn and Mercier. However, she still thinks that the causal theory of names goes too far in not requiring any descriptive element in reference borrowing. Devitt (2008) is a response.

cooperation”; he thinks he is thus “giving up my right to be the authority on the denotation of my own words” (1975: 274). The first talk of “deferring” in this context seems to have been by Gareth Evans (1973) in the process of *criticizing* the causal theory and presenting his own.¹³

The causal theory of reference borrowing was presented as part of a robust theory of reference, just the sort of theory that a deflationist like Horwich thinks unnecessary. Still it could just as well be presented as part of a theory of meaning: simply replace all the talk of reference with talk of meaning.

So, that is the causal theory’s answer to the question that Horwich sets aside: “What exactly is the phenomenon of deference?” The answer is not complete, of course, but I think that it is in good shape so far as it goes.¹⁴

5.3. UTM’s Theory of Deference

Turn now to (2) and Horwich’s theory. His move to save UTM by appeal to deference is similar to moves to save description theories of names (e.g. by Strawson). So I drew on responses to those moves (Devitt and Sterelny 1999: 3.4) in criticizing Horwich:

Consider proper names. To meet Horwich’s second requirement [that the individual is disposed to defer to the experts—i.e. to accept correction by them], the user of a name has to acknowledge her ignorance and hence be prepared to defer. But surely many ignorant users of a name do not acknowledge their ignorance. On Horwich’s theory these users will not be using the name with the same meaning as the deferrers and the experts. My guess is that most users of most names will be in that category. Next, the requirement demands that each deferrer identify experts to defer to. How? They cannot be identified simply *as* experts on the meaning of the name, on pain of circularity. It is surely unlikely that most deferrers will be able to manage the required identification....Finally, Horwich’s first requirement is that there be acknowledged experts. But surely the ignorant will often defer to someone that they think is an expert who in fact is not. (2002: 118–19)

In sum, (i) people will often not defer where they should; (ii) they will often try to defer but fail to identify an expert; (iii) they will often defer to a nonexpert. And these problems arise not only for names but for *all* primitives and

13 Thanks to my students James Dow and Joshua Livingston for scholarly help with this paragraph.

14 Horwich sets aside some other questions that arise for his UTM (1998b: 53). They do not arise for the causal theory.

perhaps even some nonprimitives (Devitt and Sterelny 1999: 5.5). So they arise for natural kind words, perhaps for artifactual kind words like ‘sloop’ and ‘sofa’, medical words like ‘arthritis’, socio-legal words like ‘contract’. Horwich’s use theory places far too heavy an epistemic burden on competent users of these words. The very same considerations that were devastating for many description theories are devastating for Horwich’s use theory. None of these problems arise for the causal theory.

(iv) UTM faces two further problems with names that refer to things that are dead. The first is that the experts that we need to defer to may be dead too. Thus, consider what Horwich says about the name ‘Aristotle’:

“Aristotle”’s meaning what it does consists in the fact that the basic feature of its use is the (conditional) holding *true* of “This is Aristotle” when pointing at Aristotle. (1998b: 129)

This is clearly not an account of *our* basic acceptance property for ‘Aristotle’ because Aristotle is not around for us to point at. So this must be an account of the “*expert’s*” basic acceptance properties. But then none of these experts are with us any more: nobody now alive holds true “This is Aristotle” when pointing at him. So there is nobody for us now to defer to.¹⁵ This is of course a special case of (ii), of people not being able to identify experts to defer to.

(v) The second problem arises even if there are some surviving experts. There are people still around who could once truly have said “This is Kingsley Amis” while pointing at Amis (and presumably could still do so *were* he around to be pointed at). Now according to UTM, people defer to experts in that “they are disposed to alter their basic acceptance properties so as to conform with that of the ‘experts’” (2005: 52). Yet there seems to be no way that deferrers to the Amis experts could meet this requirement. *What change could they be disposed to make?* They clearly cannot change to holding true “This is Kingsley Amis” while pointing at Amis. (And how could they even become able to hold this true *were* he around?)

There seem to be only two ways for UTM’s theory of deference to be modified in the face of these two problems. First, it could give up on the above-quoted “pointing” view of the expert’s basic acceptance property in favor of a “description” view. Thus, in the case of ‘Aristotle’, instead of the acceptance property involving holding true “This is Aristotle” while pointing at Aristotle it would involve holding true a whole lot of sentences about Aristotle. But the revolution has made any such “description” view implausible even for experts. Second, UTM could maintain the “pointing” view for the names of objects that are still alive but

15 I am indebted to my student Jeremy Ginsburg for drawing my attention to this problem.

adopt the “description” view for the names of objects that are dead. This has less commitment to the “description” view, but still too much. And it is implausibly committed to a name changing meaning when its bearer dies.

In sum, Horwich needs to appeal to deference to explain how we can all mean the same by words that we use very differently. My first criticism is that he has offered almost nothing in the way of a theory of deference. In light of problems (i) to (v), I think that the prospects of his doing so successfully are very dim. That is my second criticism. I conclude that Horwich’s theory of deference, though scarcely born, should be abandoned.

UTM faces a deeper problem: this appeal to deference seems to be incompatible with UTM. So even if UTM could be supplemented with a satisfactory deference theory, that would not save UTM. That is my final criticism.

5.4. *UTM and Shared Meanings*

The deeper problem for UTM arises from the following dilemma: either the members of a linguistic community typically share their meaning of a primitive word or they do not. Horwich’s appeal to deference seemed to arise from his grasping the first horn of the dilemma, as we noted: “members of a linguistic community typically mean exactly the same as one another by a given word, even when their uses of it diverge” (1998b: 85–6). So it looks as if he thinks, plausibly, that words like ‘elm’ *do* mean the same out of our mouths as out of the experts’. The difference between us and the experts over ‘elm’ is simply that we are disposed to defer to them in that we are disposed to change our basic acceptance properties to theirs. But then, from a UTM perspective, this is surely a disposition to *change the meaning* of ‘elm’: our actual meaning differs from that of the experts but we are disposed to change to the experts’ meaning. After all, our meaning of ‘elm’ is identified with whatever explains our overall deployment of ‘elm’ and UTM should surely require that this meaning be *our* basic acceptance property. For, the experts’ basic acceptance property of ‘elm’ could hardly explain *our* deployment. This is most strikingly apparent with our *mental* deployment, with the role of the mental word ‘elm’ in causing certain of our behaviors and being caused by certain of our inputs. It has to be some property of the word in *our* heads that explains this causal role. Our disposition to defer might explain why we are prepared to change our meaning of ‘elm’ but could not explain the meaning it now has for us.

So, we seem to have a *reductio*: briefly, (i) UTM and shared meanings; (ii) deference; (iii), therefore, not both UTM and shared meanings.

In the face of this, perhaps UTM should grasp the second horn of the dilemma: members of a linguistic community *do not* typically share their meaning of a primitive word. There are signs that Horwich may be tempted by this. Thus he says: “there is such a thing as the meaning of a word within a group of speakers—where many of the members of the group *do not fully grasp* that meaning” (p. 52; emphasis added). And he earlier claimed that “understanding is a matter of degree...the degree of similarity between the explanatorily basic use property...that determines the word’s overall deployment in the community and the use property that determines its deployment by the individual” (1998b: 17–18).¹⁶ But the second horn is a disaster.

Note first that a UTM that grasps the second horn has the advantage of not needing deference to explain sameness of meaning. It can accept that deference is not part of the theory of the meaning of primitives but simply a theory of a tendency to change meanings. But the trouble with the second horn is that it is false. On this view, we nonexperts with ‘elm’ and other primitives will not share meanings with the experts. Indeed, applying UTM, we will often not share meanings with each other. Each nonexpert’s meaning will be determined by her basic acceptance property and there is likely to be a great deal of variation in that property in the community. That is what the ignorance and error arguments show. It is as hard for UTM to bite this bullet as it was for description theorists of names and natural kind terms to bite the analogous bullet. For, our intuition is that people *do* share meanings. We allow, of course, that a word’s meaning for a certain person *might* differ from its meaning in her community; she may be eccentric like Mrs. Malaprop, not participating in her community’s convention for the word. Still, intuitively, the meanings of nearly all primitive words for nearly all users are not eccentric in this way.

We can do better than appealing to intuition:¹⁷ the view that we share meanings is supported by the methodology I urged in *Coming* (1996; also in Devitt 1994). Horwich and I agree that the meanings of thoughts and utterances are to be identified with the properties of thoughts and utterances that explain their causal roles. Now, day in and day out, folk, and social scientists, use (opaquely

16 But then he goes on: “Just as long as the individual has acquired the word from the community and has a minimal understanding of it, the communal language meaning may be correctly attributed to him—he means by it what everyone else means” (p. 18). This is very puzzling. If the individual really has only a minimal understanding of a word because his basic use property for it diverges greatly from that in the community then on Horwich’s view its meaning in his idiolect differs from the communal language meaning. How then could it be correct to attribute that communal meaning to him?

17 Indeed, the ubiquitous appeal to intuitions in the philosophy of language should be a cause for concern (Devitt 2010: 292–302).

construed) 'that' clauses (and the like) to ascribe meanings to thoughts and utterances for that very purpose, in particular to explain the role of thoughts and utterances in causing behavior. We can start the semantic task by discovering what is common and peculiar to the thoughts and utterances that are ascribed the *same* meaning by a 'that' clause. Then if folk and social scientists are *right* in their ascriptions, the discovery of what is common and peculiar to these thoughts and utterances is the discovery of what really is their meaning. And we have good reason to believe that the folk and social scientists are mostly right because their ascriptions are mostly *successful*: they really do explain behavior, for example. Now apply that methodology to the current issue. Does a person, Mary, mean the same as experts by a name '*a*' and a primitive general term '*F*' despite being ignorant or wrong about *a* and *F*s and hence having a usage very different from the experts? We find that people who are *as expert as you like* with those terms, are normally *as* prepared to use the clauses 'that...*a* ...' and 'that...*F*...' to describe some thoughts and utterances of the ill-informed Mary as they are to describe those of another expert. Thus, suppose Mary is largely ignorant or wrong about Catiline and elms. Experts may nonetheless be prepared to describe one of her beliefs as 'that Catiline was denounced by Cicero' and one of her utterances as 'that elms are deciduous trees'. They may be as prepared to do this for her as for another expert. And, as I have noted, such descriptions of thoughts and utterances are mostly successful in explaining the causal role of thoughts and utterances. So we have very good reason to think that those who are ignorant or wrong can nonetheless share the meanings of primitive words with experts. And a similar argument would show that those who are *differently* ignorant or wrong can share their meanings with each other.

For the most part, experts differ from the ignorant not in their meanings for primitive words but in their ability to combine those words with others to form truths; they differ not in *concepts* but in *conceptions*. And our theoretical interest is almost entirely in explaining the shared conventional meanings of our words rather than any meanings in an eccentric idiolect. For, those shared meanings are the main communicative route to the thoughts that explain behavior and guide us to reality (2006a: 10.5).

6. Conclusion

It is plausible to think that members of a linguistic community typically mean the same by their words. Yet the ignorance and error arguments proposed by the revolution in the theory of reference seem to show that people can share a

meaning and yet differ greatly in usage. Horwich responds to this by appealing to deference, but offers only some brief remarks to explain deference. I have given five reasons for doubting that these remarks can be developed into a satisfactory theory of deference. (i) People will often not defer where they should. (ii) They will often try to defer but fail to identify an expert. (iii) They will often defer to a nonexpert. (iv) When the bearer of a name has been long-dead—for example, Aristotle—there will be no experts around to defer to. (v) Where there are surviving experts about a dead person, there seems to be no change that a deferrer could be disposed to make to conform to the experts' basic acceptance properties.

But UTM's resort to deference to explain shared meaning seems to have a deeper problem: the appeal is inconsistent with UTM. So even if Horwich's theory of deference could be satisfactorily developed it would not save UTM. UTM identifies a word's meaning with whatever explains its deployment. An expert's basic acceptance property could hardly explain the word's deployment by a nonexpert with a very different basic acceptance property. For UTM, deference might explain why a nonexpert is prepared to change her meaning but could not explain the meaning it now has for her. And UTM cannot be saved by abandoning the assumption that we do typically share the meanings of primitives because there are powerful reasons in favor of that assumption.

These problems are not minor ones of details. The problems strike at the very core of UTM. At the very least, Horwich owes us an account of how UTM can deal with them. In my view, the best hope for UTM would be to make it part of a hybrid account: combining UTM for the experts with the causal theory of deference. So the meanings of nonexperts will be determined by the meanings of the experts by means of deference as explained by the causal theory. At bottom meaning would be explained by use but otherwise not. It would be interesting to explore the problems for this hybrid.¹⁸

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18 The first version of this paper was delivered at a conference, "Deflationism: Paul Horwich's Minimalist Theory of Meaning and Understanding," in Pecs in May 2006. Antonio Rauti (2010) responds to a version of the paper in the course of proposing a way that a use theory can accommodate deference. I do not think that the proposal works but discussion of this must be left until another time.

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