Still against direct reference

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Abstract

According to direct reference ("DR"), the meaning or "semantic content" of a name is simply its referent. I have argued against this in "Against Direct Reference" and many times elsewhere. In this paper I do so again from the methodological perspective of Coming to Our Senses. This leads to the view that a name's causal mode of reference is (at least one of) its meanings. This view, ignored by Soames, is the core of my argument against DR. I look critically at Braun's explanationist defense, and Soames' pragmatic defense, of DR. Finally, I argue, again, that DR faces an insurmountable difficulty: the problem of identity statements. The magnitude of this problem is demonstrated by Soames' own discussion.

According to the doctrine of *direct reference* "the utterance of a simple sentence containing names or demonstratives normally expresses a "singular proposition" – a proposition that contains as constituents the individuals referred to, and not any descriptions of or conditions on them" (Crimmins and Perry 1989: 686). I have criticized this doctrine many times, including in a paper called "Against Direct Reference" (1989). In brief, I have argued that the doctrine is theoretically unmotivated and faces an insurmountable difficulty, the well-known problem of identity statements. My criticisms have had sadly little effect. But I am still against direct reference and so I shall try again. I shall consider some of the latest from direct-reference theorists, particularly from Scott Soames. My case against direct reference is in section 3. It rests on considerations I have presented in arguing for a naturalistic truth-referential theory in *Coming to Our Senses* (1996).[1] I shall summarize that argument in sections 1 and 2.

1. The Methodology of Naturalistic Semantics

Three important questions get insufficient attention in semantics. What are the semantic tasks? Why are they worthwhile? How should we accomplish them? The central purpose of *Coming* is to answer these "methodological" questions and to see what semantic program follows from the answers. I summarize.

It is troubling that much semantic theorizing proceeds with inexplicit reliance on apparently *ad hoc* views of the semantic tasks. Thus it is common to take for granted that semantics is concerned with truth and reference. I think that this view is right, but *why* is it right? What can we say to someone like Paul

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[1] See also the following responses to critics: 1997a,b,c. I defend naturalism at greater length elsewhere (2010a: 253-91).

Horwich (1998a,b, 2005) who disagrees, claiming that semantics should be concerned with use?[2] Furthermore, it is troubling that, in attempting to accomplish the semantic task, we all go in for "intuition mongering". Broadly, it is troubling that we seem to lack a scientifically appealing method for settling the disputes that bedevil semantics. In *Coming* (ch. 2, and in Devitt 1994) I propose a view of the semantic tasks by looking at the purposes we attempt to serve in ascribing meanings. And I propose a way of accomplishing the tasks. This methodology has a place for intuitions, but it is the same limited place that they have elsewhere in science.

1.1 Semantic Tasks

So, what are the semantic tasks? What should semantics be trying to do? There seems to be a simple answer: The "basic" semantic task is to say what meanings *are*, to explain their *natures*. It is thus analogous to such tasks as saying what genes, atoms, acids, echidnas, or pains *are* but not, we should note, to such tasks as saying what genes and so forth *do*, stating the laws that advert to them. However, we start the semantic task in rather worse shape than we do its analogues. With them, the subject matter of investigation is already identified relatively uncontroversially. This reflects the fact that we have clear and familiar theoretical or practical purposes *for which* we identify the subject matter. Semantics does not start out like that. It is far from clear what counts as a meaning that needs explaining. Indeed, the intractable nature of semantic disputes largely stems from differing opinions about what counts.[3] (And we should note that the problem is not simply with the *expression* 'meaning'. Those who prefer the expressions like 'semantic content', 'semantic value', or 'propositional content' for what has to be explained have an analogous problem: What counts as a content or value?)

We start semantics in the unusual position of having to specify a subject matter. We should not insist on great precision about this in advance of theory, but we do need some explication of our vague talk of "meanings" ("semantic content", "semantic value", etc.)[4]. Furthermore, we must specify a subject matter *worthy* of investigation; we need an explication that is *not ad hoc*. Finally, the semantic task of interest to philosophers should be not only worthwhile but *fundamental*.

I seek a solution to this problem by focusing on the purposes for which we ascribe meanings using 'that' clauses ("t-clauses") in attitude ascriptions: in particular, the purposes of explaining intentional behavior and of using thoughts and utterances as guides to reality. I call these purposes "semantic". I say further that a property plays a "semantic" role if and only if it is a property of the sort specified by t-clauses, and, if it were the case that a token thought had the property, it would be in virtue of this fact that the token can explain the behavior of the thinker or be used as a guide to reality. We are then in the position to add the following explication to the statement of the basic task: A property is a meaning if and only if it plays a semantic role in that sense. And the basic task is to explain the nature of meanings in that sense (secs. 2.3-2.6).

Consider another task, that of explaining the nature of the properties that we *ought to ascribe* for semantic purposes. It is easy to see that this "normative" task is closely related to the basic one. Indeed, my *first methodological proposal is that we should tackle the basic task by tackling the normative one*.

There is an obvious contrast between the normative task and what I call "the descriptive task". This is the task of explaining the natures of the properties we *do ascribe* in attitude ascriptions for semantic purposes; it is the task of explaining the semantic *status quo*. It is what most people working in semantics -- philosophers, linguists, and psychologists -- are in effect doing. Yet it is very different from the normative and basic tasks. Note particularly that the "putative meanings" it investigates really are *meanings* only if ascribing them really does serve our semantic purposes, only if they really play semantic roles. Once the descriptive task is sharply distinguished from the others, the question of its bearing on them arises. We shall consider this question in a moment.

Semantic tasks are often defined using terms like 'meaning' with either no attempt to explicate this

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[2]   Devitt 2002 and 2010b are in fact responses to Horwich.

[3]   "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 $=_{\text{def}}$ Whatever aspect of linguistic activity happens to interest *me now*" (Lycan 1984: 272).

[4]   Hereinafter I shall mostly omit such parenthetical additions but they should always be taken as read.

talk or with an explication using terms like 'proposition' and 'information' that are equally unclear. Given the prima facie unclarity and vagueness of all this talk, this practice is surely unacceptable: It leaves us both with no firm idea of what the issue is and with the likelihood of being at cross purposes. Once an explication is offered, there is little point in a verbal dispute about the appropriateness of using 'meaning' and hence 'semantics' in the way proposed. However, there is a lot of point in asking *why a task defined in terms of 'meaning' thus explicated is worthwhile*. We are, of course, free to study anything. But if semantics is a genuine science, as the naturalist thinks it must be, we should be able to say why it is interesting. A definition that does not say this will be *ad hoc*.

My discussion of the semantic tasks is an attempt to meet these demands for explication and worth. 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.[5]

I have four comments on this discussion, some that go a little beyond what is in *Coming*.[6]

(a) The focus of this discussion is on the meanings of thoughts rather than of utterances. I argue that it is *because of* our interest in the meanings of thoughts that we are interested in the meanings of utterances. It is by ascribing meanings to thoughts that we *directly* explain behavior and are guided to reality. But a person's utterances are our main route to her thoughts. If we take a speaker's utterances as sincere expressions of her thoughts then we will ascribe the same meanings to her thoughts as to her utterances. So ascribing meanings to utterances is a way to ascribe meanings to thoughts and hence *indirectly* to explain behavior and be guided to reality.

(b) Paul Grice (1989) has drawn our attention to the distinction between the conventional meaning of an utterance and its speaker meaning. A speaker may mean something by the sentence he utters that is different from what that sentence conventionally means on the occasion of the utterance. In doing semantics, we are primarily interested in *conventional meanings on occasions of utterance* because those are the meanings that are the main routes to speaker meanings and hence thought meanings. It is because sentence tokens have those meanings, largely by convention but partly as a result of the "pragmatic" fixing of indexical reference and removal of ambiguities, that uttering the sentences is so worthwhile.[7] For, on hearing an utterance, a person who participates in the conventions for the type of sentence it involves can, with the help of "pragmatic" abilities at detecting indexical references and removing ambiguities, immediately grasp the thought that the utterance expresses. So the basic task for utterances is to explain the nature of properties that utterances have largely by convention and that play semantic roles.

(c) This task concerns the meanings of tokens and the earlier explication should be understood accordingly: a property *of a (thought or expression) token* is a meaning if and only if it plays a semantic role. Yet many focus their semantics on the meanings of types. An interest in types is already implicit. Although our theoretical interest in meanings starts with tokens, that immediately leads to an interest in types. For, as we have just noted, an utterance token has a meaning we are interested in partly in virtue of a conventional meaning of tokens of that type in the speaker's language. So our basic task for tokens leads to an interest in those conventional properties of types which, along with pragmatic features on an occasion of utterance, determine the properties of tokens that play semantic roles. And the basic task for expression types is to explain the nature of those conventional properties.

(d) The discussion opens up the possibility that a token has more than one meaning. For, it opens up the possibility that a token might have a variety of properties playing semantic roles: One meaning might explain one bit of behavior; another, another; and a third might serve as a guide to external reality. If so, there would be no theoretically interesting reason to insist that one rather than another is *the* meaning (2.8). *Coming* argues that this is in fact the case.

My first methodological proposal was that we should tackle the basic task by tackling the normative

one. Turn now to the methodology of the latter task.

1.2 Methodology for the Normative Task

What should be the basis for our choosing to ascribe one rather than another of the many properties of a token that are candidates to be meanings? The answer is implicit in the task. We should ascribe the ones that play semantic roles and hence that it serves our semantic purposes to ascribe: the ones that play the roles outlined in explaining behavior and informing us about reality.

It is clearly not easy to tell which properties to ascribe nor to say how to tell. *Coming* makes a modest proposal about the bearing of the descriptive semantic task on this normative task. Our ordinary attitude ascriptions attribute certain properties for semantic purposes. Given the apparent success of the ascriptions it is likely that these putative meanings are *real* meanings. So I make my *second methodological proposal: We should look to the descriptive task for evidence for the normative/basic one*. Further, I suggest, although I do not argue, that the main, if not the only, justification for the usual focus on the descriptive task is the bearing of that task on the normative one (2.9).

1.3 Methodology for the Descriptive Task

Turn now to the methodology of the descriptive task. *Coming* approaches this by considering descriptive tasks *in general*. How do we tell what is the nature of some property, **being an F**, that we ascribe? Sometimes we already have a well-established theory; for example, thanks to molecular genetics, we had one for **being a gene**. But suppose that we do not have a theory and are starting pretty much from scratch. How then do we tell what is common and peculiar to *F*'s "in all possible worlds"? What is the "ultimate" method?

The answer breaks into two stages. First, we must identify some apparently uncontroversial examples of *F*'s and non-*F*'s. Second, we must examine the examples to determine the nature of **being an F**. The second stage is a straightforwardly scientific one. The preliminary first stage may not be. It involves using "identification experts" who may be scientists but may be just plain folk (2.10).

Coming goes on to apply this general discussion to the descriptive task in semantics. In semantics we surely start from scratch: Any theories we have are as much in contention as anything is. So, I make my *third methodological proposal: We should use the "ultimate" method on putative meanings to accomplish the descriptive task.*

To apply the "ultimate" method to a putative meaning, we must first identify some apparently uncontroversial examples of tokens with that property and of tokens without it; and second we must examine the examples to determine the nature of the property. Who are the experts to be consulted in the first-stage identifications? The folk, in their frequent use of t-clauses in attitude ascriptions, are as expert as anyone. And the folk are in an advantageous position to become experts because they themselves are competent to produce token thoughts and utterances to which the properties are ascribed. Finally, theorists can count themselves among the expert folk. So, in the first stage, semantic theorists have two advantages: no need to consult others and ready access to data.

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 (2.11).[8]

The next step is to use this methodology to argue for a truth-referential view of meanings.

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[8] For more on intuitions and their role, see Devitt 2010a: 292-302.

2. A Truth-Referential Theory

Coming argues for the view that the meanings of a token are entirely constituted by "representational properties". These properties include any property that plays a role in determining what the token represents. So the meaning of *a sentence* token is exhausted by the properties that determine its *truth condition*.[9] And the meaning of *a word* token is exhausted by properties that determine its *reference*.[10]

2.1 The Descriptive Task

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,[11] we find support for the descriptive version of our representationalism.

The focus is on definite singular term tokens and their mental correlates. I shall talk of these mental correlates as if the controversial "language-of-thought hypothesis" were true. Thus, I shall call the part of a thought that is expressed by a proper name "a proper name". I do think that the language-of-thought hypothesis is true (4.4), but even if it is not I can see no harm in this convenient way of talking.

As Quine pointed out, the most common attitude ascriptions are opaque. In these, substituting a co-referential term for a singular term in a t-clause cannot be guaranteed to save truth; the rule of "substitutivity of identity" does not hold. (The proponents of direct reference disagree; see sec. 3.) Thus, consider Quine's famous example. Ralph observes a man in a brown hat behaving suspiciously and as a result it is true to say,

(1) Ralph believes that the man in the brown hat is a spy.

But, as typically understood, it is not true to say,

(2) Ralph believes that Ortcutt is a spy,

even though Ortcutt is that very man in the hat. For, Ralph does not know this identity. In light of this, what property do singular terms in the t-clauses of opaque ascriptions ascribe to thoughts? I argue that they ascribe the property of (purportedly) referring to a specified object under a specified mode. All token beliefs that would make (1) true have in common, so far as their singular terms are concerned, only the property of (purportedly) referring to Ortcutt under the mode exemplified by 'the man in the brown hat' in (1); similarly, (2), and the mode exemplified by 'Ortcutt' in (2). (1) is true because Ralph believes spyhood of Ortcutt under the former mode; (2) is false because he does not believe it of him under the latter mode. Since these modes are ascribed to explain behavior and guide us to reality they are *putative* meanings at least (4.2).

2.2 The Normative/Basic Task

But are they *really* meanings? If my argument is right then we have learnt what folk ascribe to explain behavior and guide us to reality and hence what they are, in effect, treating as a meaning. It describes the semantic *status quo*. We have solved the descriptive task. But we need to solve the normative/basic task.

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[9]   Not quite. A sentence token 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 shall overlook these forces.

[10]   Any truth-referential theory of meaning gives an explanatory role to truth and reference and so is at odds with deflationist views. For a discussion, see Devitt 2010a: 155-81.

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

Perhaps the folk are wrong. Perhaps the property of a mental word that really does play a role in explaining behavior, and hence is its meaning, involves a stereotype or a non-reference-determining functional role. Or perhaps the eliminativist is right and there are no thoughts: something else altogether explains behavior. Of course, if the *status quo* is as we have described it, such positions are all revisionist. But that does not show that they are wrong.

Showing this is, of course, a lengthy business involving criticism of revisionism and its arguments. I attempt to tackle this in *Coming* (ch. 5). Now I just want to emphasize one simple yet powerful argument for the *status quo*: briefly, it works. Day in and day out the folk use ordinary thought ascriptions to explain behavior and guide us to reality. For example, they say "Oscar believed that Mary was thirsty" to explain his giving water to Mary and inform us of Mary's thirst. And the folk are not alone in this habit: social scientists do it all the time too. Furthermore, these ascriptions appear to be, by and large, *successful*; the ascription to Oscar really does seem to explain his behavior and inform us of Mary's thirst. This is evidence that thoughts really do have whatever properties the folk and social scientists ascribe to them, and that those properties really do serve semantic purposes and so are meanings. And, I have argued, those properties are modes of reference. Given the explanatory success of this *status quo*, overthrowing it needs both a powerful argument and a plausible alternative semantics (4.3).[12]

2.3 Other Meanings

Now, in this paper, I shall be focusing on these common opaquely ascribed meanings. But I think that there is a very good case that there are others. First, using an example due to Stephen Schiffer (1979: 67), I argue sentences like (1) and (2) also have a "simply-transparent" reading according to which they ascribe the property of simply referring to a specified object. And the literature makes it plausible that sentences like

(3) Ortcutt is such that Ralph believes him to be a spy

are "rapport-transparent" ascribing the property of referring to an object that the speaker has particularly in mind (4.2). Furthermore, I argue that these other ascriptions also serve our semantic purposes, particularly the purpose of guiding us to reality. So, these putative meanings are also really meanings (4.3). And that is not the end of it. Attention to various ingenious "puzzles" (Richard 1983; Perry 1993; Castaneda 1966, 1967; Kripke 1979) complicate the story. The explanation of behavior sometimes demands that a demonstrative has a "finer-grained" meaning than may be ascribed by a normal opaque ascription (4.14). First-person pronouns have a similar finer-grained meaning ascribed by a special form of attitude ascription (4.15). The meaning ascribed by a proper name in a t-clause is normally "coarser-grained" than my discussion of (2) suggests: thus 'London' in a t-clause normally ascribes a "disjunctive" mode that includes the mode of 'Londres' and all other translations of 'London'. Yet, the explanation of behavior sometimes demands that a name has a finer-grained meaning than may be ascribed by a normal opaque ascription (4.17).

If this is all right, the earlier-mentioned possibility that a token belief or utterance has more than one meaning property turns out to be actual.

2.4 Explaining Meanings

We need to explain the nature of these various meanings, of course. What we most need for that are theories of reference. *Coming* argues that three sorts of theory of reference are possible (4.5). According to "description" theories, the reference of a word is fixed by certain of the descriptions that speakers associate with the word; it refers to whatever those descriptions, or a weighted most of them, apply to. The received view for decades was that proper names are to be explained by a description theory. But then came the revolution. Such theories were seen to have serious problems, particularly the problem of

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[12] So Braun is wrong to say that I *assume* that t-clauses "ascribe only semantic properties to tokens" (2000: 491). The properties they ascribe are semantic at all, on my explication, only if they really do explain behavior or guide us to reality. I *argue* that these properties do have these "semantic" roles. A central point of my methodology is to avoid tendentious assumptions about semantics.

"ignorance and error": speakers who seem perfectly competent with a word are too ignorant to provide the appropriate descriptions of its referent; worse, speakers are often so wrong about the referent that the descriptions they would provide apply not to the referent but to other entities or to nothing at all (Kripke 1980; Donnellan 1972). Aside from that, description theories in general are *essentially* incomplete, explaining the reference of some words in terms of the reference of others, a reference which then needs to be explained.

If we are to be naturalistic, some singular terms at least must be covered by "causal" or "descriptive-causal" theories, explaining reference fully or partly in terms of direct noninferential relations to reality. These theories must appeal to one or more of three types of causal relations: historical, reliablist, or teleological. Although much progress has been made with theories of these ultimate referential links, I don't think that we yet have any completely satisfactory theory.

Theories of reference of these three sorts will explain the referential meanings we have posited. Consider the meanings that are our focus, opaquely ascribed properties of referring to an object in a certain way. Some of these – for example, that for 'the man in the brown hat' - might be descriptive modes of reference but it *simply follows* from the above discussion that some must be nondescriptive *causal* modes of reference.[13] *Coming* illustrates the latter idea with "IT", a historical-causal theory of names and other singular terms (4.6).[14] On this view, the opaquely ascribed meaning of a name is a property of referring by a certain sort of causal chain. For example, the mode for 'Mark Twain' is the property of referring by means of causal chains grounded in Mark Twain and involving the sounds, inscriptions, and so on, that constitute the history of the name's use to designate Mark Twain; and the mode for 'Samuel Clemens' is similar but involves the sounds, inscriptions, and so on, of this different name. If another sort of causal theory is right for a word, its mode will be its property of referring by some other sort of causal relation to external reality. The commitment to meanings as causal modes does not rest on IT or any particular theory of these modes.

3. Direct Reference

3.1 Ignoring Causal Modes

Having outlined its program, *Coming* turns to the rejection of its rivals. The first of these is the direct-reference view of proper names, "DR" (4.8, 4.18).[15] DR has its roots in the just-mentioned refutation of description theories for proper names. DR responds to this refutation with the surprising claim that the meaning – DR's preferred term is "semantic content" - of a name is simply its referent, and the claim that assertions containing the name semantically express only "singular Russellian propositions", propositions containing that very referent.[16] Haven't we known for more than a century that such a Millian view faces

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[13]   Note that the view is not that such a meaning is a property of *being associated with a description* of a certain causal way of referring to the object: that would be what is known as "causal descriptivism", a *description* theory that is parasitic on a causal theory; see Devitt and Sterelny 1999, p. 61, for a discussion. The view is that the meaning is the property of *having* a certain causal way of referring to the object. So the name is rigid (pp. 79-81); c.f. Braun 2000.

[14]   I proposed this theory in earlier works: 1974, 1981a, 1981b, 1989.

[15]   I am indebted to David Braun for a very helpful exchange about DR, lasting over many years and including lengthy comments on a draft of the present paper. He has tried to answer my criticisms of DR with great patience but not, it seems to me, much success. He has also been an invaluable guide to the literature.

[16]   Given my scruples about treating meanings as objects, particularly as propositions, and preference for treating them as properties, I would express the DR view as that a name's meaning is simply its property of referring to its referent. But this preference is beside the point of my disagreement with DR and so I shall (mostly) go along with DR's expression. DR applies also to indexicals and perhaps some

overwhelming problems with identity statements, empty names, and the role of names in the content sentences of attitude ascriptions? Proponents of DR think not. They acknowledge the problems, of course, but seek to explain them away because they find DR so *prima facie* plausible. Thus, in his much-discussed recent book, *Beyond Rigidity* (2002), Scott Soames claims that "if names don't have descriptive semantic contents, then it would seem that their only semantic contents are their referents" (2002: 5); "the semantic theses [of DR] I have adopted are highly motivated; it is not easy to see how they could be wrong" (p. 142). What is the source of this complacency? The answer is clear. In support of the DR view that 'Carl Hempel lived on Lake Lane in Princeton' semantically expresses the proposition made up of Carl Hempel and the property of living on Lake Lane in Princeton, Soames has this to say: "Since there seems to be no better candidate, it is reasonable to identify the proposition semantically expressed" with this singular Russellian proposition (p. 66). In brief, he thinks that, once description theories are abandoned, DR is "the only theory in town".

This is curious. DR is obviously not the only theory in town. I have briefly described another in the last section. It is the theory that the meaning of a name is its property of (purportedly) referring to a specified object in a certain causal way; the meaning is a causal mode of reference. Neither this theory nor any of the many works in which I have argued for the theory and against DR (*inter alia*, 1974, 1980, 1981a, 1989, 1996, 2001) is even mentioned by Soames. This is disappointing, even a bit depressing.[17] What are we to make of it? Soames cannot be unaware of the theory; indeed he gave me helpful comments on a draft of "Against Direct Reference" (1989). The charitable thing to suppose is that he finds the view too preposterous to be worthy of discussion. This is pretty much the view of Nathan Salmon, another "hard man" of DR. Citing my *Designation* (1981a), Salmon calls the view "ill conceived if not downright desperate...wildly bizarre...a confusion, on the order of a category mistake" (1986: 70-1).[18] Indeed, the received view, even among those who do *not* subscribe to DR, seems to be that DR is simply a *consequence* of accepting a nondescriptive causal theory of names.[19]

Why would anyone think that the idea that the meaning of a name is its causal mode of reference should not be taken seriously? The idea that the meaning is a mode of reference ("presentation") of *some* sort has, of course, been familiar since Frege. It was proposed precisely because of the now-familiar problems for a Millian theory, particularly the problem of identity statements. According to the Fregean tradition this mode is expressed by the descriptions competent speakers associate with the name. But then the revolution showed that the reference of a name is not determined in this descriptive way. So where do we go from there? Well, clearly, the name's reference must be determined *somehow*, presumably causally in something like the way that Kripke sketched. So the name must *have* some sort of causal

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other terms but I shall discuss only proper names. It should be noted that "direct reference" is used quite loosely and does not always refer to what I am here calling "DR". I have elsewhere summarized the various "direct reference" theories and their histories (1989: secs. 1-2).

[17]   But I don't take it personally for I am far from alone in being thus neglected. For example, even though Soames rests his defense of DR on appeals to pragmatics, the only work in the extensive pragmatics literature that he mentions is Grice 1989. And I must acknowledge that Soames devotes a whole chapter (2002: ch. 7) to criticizing the views of Richard Larson and Peter Ludlow (1993) and Mark Richard (1990). These views, which take attitude ascriptions to ascribe "linguistically enhanced propositions", have some similarities to mine, as I note (1996: 166n, 202, 237n). But the differences are important and none of Soames' criticisms seem to me to undermine my view. Thus, (i), my argument that the meaning ascribed by a proper name like 'London' in a t-clause is normally a "disjunctive" mode that includes the mode of 'Londres' and all other translations of 'London' (1996: 232-4) avoids some criticisms Soames makes of both views (2002: 156-7, 165-7, 174). (ii) I do not embrace the Davidsonianism of Larson and Ludlow that leaves them open to one of Soames' criticisms (pp. 150-1). (iii) Most of Soames' criticism of Richard is of views that arise from Richard's "hidden-indexical" theory of attitude ascriptions. I also reject hidden-indexical theories. Indeed I have criticized them extensively (1996: 4.11; 1997b), sometimes along lines rather like Soames'.

[18]   Note that my view is not the genuinely preposterous view that the meaning of a name is a particular *token* causal link (c.f. Yagisawa 1993: 144) and so is not open to Salmon's "argument from subjectivity" (p. 70).

[19]   Some examples: Loar 1976; Ackerman 1979a: 58; 1979b: 6; McGinn 1982: 244; Baker 1982: 227; Lycan 1985; Block 1986: 660, 665; Lepore and Loewer 1986: 60; Wagner 1986: 452; Wettstein 1986: 187; Katz 1990: 31-4.

mode of reference. So perhaps *that* mode is the meaning. This surely should be a *candidate* for being the meaning, perhaps not the right candidate, but still a candidate. Yet the idea that this causal mode is a meaning is clearly alien to the semantic tradition; it is, as I have said in the title of a paper, "A Shocking Idea about Meaning" (2001). Why is it so shocking? I think the main cause is a ubiquitous Cartesianism about meaning. It is a truism that competent speakers of a language "know the language". The Cartesian assumption is that this involves (tacitly) knowing *facts about* meanings: if an expression has a certain meaning in the language then speakers know *that* it does.[20] Then, since the typical speaker knows nothing about causal modes of reference, those modes cannot be meanings. Yet this popular Cartesianism is almost entirely unsupported and is, I have argued, undermined by the revolution. We should embrace the much more modest view that linguistic competence is an ability or skill, a piece of knowledge-how not knowledge-that (1996: 51-4, 172-6; 2001: 479-81).[21] In any case, this Cartesian explanation should not appeal to the proponents of DR. We would expect them to be skeptical of semantic Cartesianism, and Soames, for one, seems to be (2002: 70-1). Without Cartesianism, the cursory dismissal of a causal mode as a candidate meaning is unwarranted: the dismissal needs an *argument*. And Salmon and Soames need more than that. For, they have another candidate - the meaning of a name is its referent – and so they need an argument that their candidate is *better*. To my knowledge, no argument of either sort has ever been attempted by Salmon, Soames, or any other proponent of DR. Instead of argument, we get special pleading: DR is "reasonable" (Soames 2002: 65), "highly motivated" (p. 142), "highly plausible" (2004: 102), "appealingly simple" (Braun 2001: 256), and so on.

3.2 The Argument for Causal Modes as Meanings

Of course, the view that a name's meaning is a causal mode of reference also needs to be argued for and shown to be better than DR. This I have attempted in *Coming* and the other works cited. I have outlined the argument in the last section. Let me now give a snappy summary to sharpen the issue with DR:

1. A name in a t-clause of an apparently opaque attitude ascription conveys information about a mode of referring to the name's bearer.
2. A name's mode of referring to its bearer is causal not descriptive.[22]
3. Apparently opaque attitude ascriptions explain behavior in virtue of what they convey.
4. So, the causal mode is the name's meaning.

Given DR's revolutionary ancestry, it surely must accept (2). And Salmon (1986: 70-1) and Soames (2002: 19-20) do seem to accept it (whilst remaining noncommittal about the precise details of these causal modes).

What about (1)? I start with a preliminary point about its talk of "apparently" opaque ascriptions. This is to avoid begging the question against DR. Salmon and Soames claim that attitude ascriptions are not *really* opaque - substitutivity of identity does not *really* fail – but they, like just about everyone else, agree that some ascriptions *seem* to be opaque.

(1) goes back at least to Frege, is much discussed by Quine, and is very hard to deny. Salmon does seem to accept it (1986: 116). But, whereas a common view is that conveying the mode of reference is a semantic matter, for Salmon it is merely a "pragmatic function" of the ascription (p. 117). We will get to this difference in a moment (3.4. What about Soames? So far as I can see, he does not explicitly discuss (1) in *Beyond Rigidity*. He thinks that apparently opaque ascriptions pragmatically convey more than singular Russellian propositions but what he principally has in mind is information captured by non-reference-determining descriptions that the speaker and her audience associate with the name (2002: 210-14). He does not seem to have in mind that these ascriptions also convey modes of reference. However, he earlier accepted that they do (1987: 68; 1988: 117-25) and he offers no reason for going against this

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[20] An analogous Cartesianism is rife in linguistics. My *Ignorance of Language* (2006) is an extended critique.

[21] 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).

[22] Perhaps I should be a bit more cautious: the mode is *largely if not entirely* causal rather than descriptive. This allows for the fact that the revolution did not show that *no* descriptive element plays a role in determining the referent of a name. And, as Soames points out nicely, a case can be made that names like 'Princeton University' are "partially descriptive" (2002: 51-2).

near-irresistible view.

Now consider (3). The view that apparently opaque attitude ascriptions explain behavior in virtue of what they convey is a pillar of folk psychology, accepted one would have thought by everyone bar a few eliminativists. Yet it is not as obvious as one would like that either Salmon or Soames accepts it. Salmon thinks that it serves "our normal purpose in attributing belief… to convey *how* the believer stands with respect to a proposition" (1986: 116) but he says nothing to suggest that an important "normal purpose" is to explain behavior. So far as I can see there is no mention of explaining behavior in Soames' book. Still, in a response to Maite Ezcurdia, he does seem prepared to go along, "for the sake of argument", with her emphasis on the importance of attitude ascriptions in explaining behavior (2004: 103; see also 1987: 68-9).

In sum, (2) must be accepted by any proponent of DR. (1) and (3) are widely accepted and Salmon and Soames do not attempt to throw any doubt on them. (4) follows easily. From (1) and (2) it follows that an apparently opaque attitude ascription conveys information about a name's mode of referring and that mode happens to be causal. Then from (3) it follows that it is partly in virtue of conveying this mode that the ascription explains behavior. So that mode plays a semantic role and is a meaning: it is in virtue of having the property of referring to its bearer in a certain causal way that a name contributes to causing behavior.

Now a proponent of DR is likely to respond that semantics, *properly conceived*, is not concerned with this property of causing behavior. Soames does in effect respond in this way to Ezcurdia (2004: 103-4) and Salmon surely would, given his resistance to connections between semantics and psychology (1986: 174, n. 2). This sort of response raises two questions. First, if the response is not to be a merely verbal point about what to call "semantic" or a "meaning" (or "semantic content") then it needs to be accompanied by an argument that the property of a name that causes behavior *is not* theoretically interesting, that it *is not* adverted to in a theory that explains some phenomena. Second, the response needs to be accompanied by an argument that a name's property of simply referring to its bearer - what DR attends to – *is* theoretically interesting, that it *is* adverted to in a theory that explains some phenomena. And it is not sufficient to argue that this property of a name is interesting because it is what a name contributes to the truth conditions of sentences containing it. For, we need an argument that a sentence's truth condition, its property of representing a certain situation, is theoretically interesting. Perhaps, rather, its property of representing that situation *in a certain way* (or, indeed, its property of having a certain use) is what is theoretically interesting. We need these arguments if the DR's preference for a name's property of referring to its bearer over its property of referring to its bearer *in a certain way* is not to be theoretically arbitrary, a mere *ad hoc* stipulation. I made this point in *Coming* (4.8). I had made it before (1989) and have made it since (2001). Yet, so far as I know, no attempt has been made to supply the needed arguments.

This is not to say that the property of referring to the name's bearer is *not* theoretically interesting nor to say that an argument for its interest *cannot* be given. Indeed, I claim to have given something close to this in *Coming*, attending particularly to the role of names in guiding us to reality (4.2-4.3). But I also claim to give an argument for the theoretical interest of modes of reference in explaining behavior *and* in guiding us to reality. So, contra DR, these properties are the most theoretically interesting and certainly deserve to be called "meanings" (1996: 150-4). The most objectionable feature of DR is not what it includes in semantics but what it excludes.

So my conclusion is that there are good reasons for the view that a name's causal mode of reference is its meaning and no good reason has been presented against this view. I shall now go on to say more against DR, but the above argument is the core of my case.

3.3 Braun's Defense of DR

I start with a defense of DR offered by David Braun (2001). He takes critics like me to argue that if DR were true, "then attitude ascriptions could not explain (certain sorts) of behavior" (p. 254). Yet these ascriptions obviously do explain that behavior and so DR must be false. He responds by arguing ingeniously that ascribing a singular Russellian proposition to a person *does contribute* to explaining a person's behavior: it "provides a substantial portion of the information contained in some ideal explanation" (p. 267). But Braun has missed the main point of my argument. That point is *not* that if DR were true the ascriptions could not explain behavior. The point is that ascribing *non*-Russellian

propositions involving causal modes of reference does, as a matter of fact, explain behavior. This point is quite compatible with the claim that ascribing a singular Russellian proposition can *contribute* to the explanation in the way Braun illustrates. Indeed, in effect, I explicitly endorse that claim (1996: 153). But ascribing modes of reference provides a *more complete* explanation. As Braun states nicely: "Clearly, the way in which an agent believes or desires a [singular] proposition can make a difference to that agent's behavior" (p. 257). So that way, involving in the case in question a name's causal mode of reference, is the meaning. There is no principled basis for Braun's view that the referent is theoretically interesting in a way that the mode of reference is not. Braun is taking as obvious precisely what is in question.

3.4 The Pragmatic Defense of DR

I turn next to "the pragmatic defense" of DR. At the beginning of his book, Soames says: "A central feature of this account is the explanation it provides of how sentences containing names or indexicals may be used to convey, and even assert, propositions the contents of which exceed the semantic contents of the sentences uttered" (2002: v). Now, everyone should accept something along these lines; in particular, in a situation Soames describes (p. 63), we should probably all accept that 'Carl Hempel lived on Lake Lane in Princeton' conveys the information that *the well-known philosopher of science* Carl Hempel lived on Lake Lane in Princeton even though the information that Hempel is a well-known philosopher of science is not part of the semantic content of the sentence. And we should all accept that it is often hard to know precisely where to draw the line between what is semantically and what is pragmatically conveyed by a sentence.[23] Still any claim that an utterance semantically expresses that *p* but pragmatically conveys that *q* needs to be supported by an argument. And the argument needs to show not only how an utterance that semantically expresses that *p could*, in the circumstances, pragmatically convey that *q*; it needs show that it is more plausible to think the utterance *does* pragmatically convey that *q* whilst semantically expressing only that *p* rather than semantically express that *q*. I don't think that DR comes close to providing the arguments we need here.

   The key issue is whether a name conveys information about its mode simply in virtue of a semantic convention. If it does then that information is semantically not pragmatically conveyed. Here is evidence that it does. Let us attend to simple predicative utterances of the form '*a* is *F*'. First, we note that it is highly relevant to the explanation of the behavior of a person that utters '*a* is *F*' to know that the thought she expresses is under the mode of reference of '*a*'. Thus, whether Mary's belief of Twain/Clemens that he is sitting at a nearby table is under the mode of 'Twain' or 'Clemens' might make a big difference in her behavior; for example, given her other beliefs, if the belief is under the former mode she might rush up to Twain/Clemens for an autograph but if it is under the latter mode she certainly will not. So we *need* people regularly to convey information about modes of thought and we would *expect* there to be a semantic convention to facilitate their doing so. Second, the fact that people regularly – indeed always? – convey this information in uttering '*a* is *F*' is evidence that doing so is a semantic convention. When a person has a thought under a certain mode she *standardly* uses an expression *under that very mode* to express the thought. Soames states a nice requirement for semantic content: "the semantic content of *s*…should consist of information that a competent speaker who assertively utters *s* asserts and intends to convey in any context in which *s* is used nonmetaphorically" (2002: 57). Information about modes fits this requirement perfectly. Finally, as we have already noted, our standard way of ascribing meanings to utterances to explain behavior– opaque attitude ascriptions – conveys information about modes.

   This case is persuasive that information about a name's mode of reference is semantically conveyed. Even if the contrary view that this information is pragmatically conveyed faced no insurmountable difficulty, it is hard to see any reason for preferring that view. And the view does seem to face an insurmountable difficulty: the problem of identity statements.

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[23]   *Coming* throws sadly little light on how to draw this line in general. I think I have done better in some later works (1997b, 2004, 2008).

3.5 Identity Statements

That the true identities '$a = a$' and '$a = b$' differ in meaning is about as powerful a semantic intuition as we have. Still, we should not rest with an intuition: we need an argument. I do argue for this difference, but not in the usual way that appeals to the difference in informativeness of the identities. For, what have *epistemic* issues about informativeness got to do with *semantic* issues about meaning? People think that the epistemic issues are relevant because they take something like the following for granted: '*S1*' and '*S2*' mean the same only if all competent speakers know that they do. This is a dramatic example of the sort of Cartesianism that *Coming* rejects. I argue for the difference in meaning by applying the methodology: the folk normally distinguish the meanings of '$a = a$' and '$a = b$' in using t-clauses to serve their semantic purposes (descriptive), and they are right to do so (normative); the t-clauses are opaque and ought to be so to serve the purposes, particularly those of explaining behavior. So the two identities differ in meaning. And, of course, the semantic view I am urging accounts for this difference easily: 'a' and 'b' have different modes of reference (4.7; see also 1997c, pp. 382-6 for a correction).

DR has to start its treatment of identity statements with the highly implausible claim that '$a = a$' and '$a = b$' do *not* differ in meaning. So the differences between these statements have to be explained away pragmatically. And so too do differences in *ascriptions* of the attitudes that $a = a$ and that $a = b$. The dimension of this problem for DR has been demonstrated countless times but Soames demonstrates it as well as anyone has (2002: 230-2). Suppose that Tom says "$a = b$" to Mary and that as a result Dick says "Mary has just learned that $a = b$". Set aside any propositions Dick thereby pragmatically conveys that Mary has just learned and consider the proposition that, according to DR, he semantically expresses that Mary has just learned. That proposition is a triviality. For, Dick semantically expresses just what he would have had he said "Mary has just learnt that $a = a$". Now had he said that he would have expressed something *false*: Mary has known the triviality that $a = a$ ever since she first heard of a. Yet *nobody* (except of course proponents of DR) would accept that Dick's actual utterance falsely asserts that this triviality is what Mary has just learnt. Soames unflinchingly bites the bullet: we are all wrong. This is not plausible (to put it delicately).

In sum, I have argued that DR is theoretically unmotivated. And it has no convincing answer to the well-known problem of identity statements.

4. Conclusion

I started by summarizing my argument in *Coming to Our Senses* (1996) for a certain semantic methodology and truth-referential theory of meaning. This explanation of meaning is far from complete, of course. What it mostly lacks are completely satisfactory theories of reference.

Central to this truth-referential theory is the view that a name's causal mode of reference is at least one of its meanings. This is the core of my argument against direct reference, DR. I went on to dismiss Braun's defense of DR based on the correct claim that ascribing a singular Russellian proposition can contribute to the explanation of behavior. Finally, I considered the pragmatic defense of DR. Even if the DR view that information about a name's mode of reference is pragmatically conveyed faced no insurmountable difficulty, there seems to be no reason for preferring it to the view that this information is semantically conveyed. And the DR view does face an insurmountable difficulty: the problem of identity statements.[24]

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[24] Parts of this paper were delivered at the Australasian Association of Philosophers at Armidale, July 2007, and at a conference on John Perry's Work, XVIII Inter-University Workshop on Philosophy and Cognitive Science in Madrid, April 2008.

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