Precis of
Coming to Our Senses:
A Naturalistic Program for Semantic Localism

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INTRODUCTION

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 to Our Senses (Devitt 1996; henceforth, "Coming") is to answer these "methodological" questions and to see what semantic program follows from the answers.

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 who disagrees, claiming that semantics should be concerned with, say, warranted assertability or "use"? Furthermore, it is troubling that, in attempting to accomplish the semantic task, we all go in for "intuition mongering," even those of us who are naturalistically inclined and skeptical of the practice (e.g., Jerry Fodor 1990: 169). Broadly, it is troubling that we seem to lack a scientifically appealing method for settling the disputes that bedevil semantics. In Coming Chapter 2 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. I think that applying this methodology will help with all semantic issues. In Coming I use it in the hope of settling some, including some of the most notorious.

A by-product of this methodological discussion is a naturalistic account of the thought experiments characteristic of "armchair" philosophy.

In approaching the methodological questions, I make three important and related assumptions. First, I assume anti-Cartesianism. It is common to think that linguistic-conceptual competence brings "privileged access" to meanings (or contents).[1] One example of this is the widespread view that semantic competence consists in knowledge of truth conditions.[2] Another is

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[1] For example, consider:

It is an undeniable feature of the notion of meaning... that meaning is transparent in the sense that, if someone attaches a meaning to each of two words, he must know whether these meanings are the same. (Dummett 1978: 131)

See also: Loar 1987: 97; McKinsey 1994: 308.

[2] Heidelberger (1980) has shown how widespread this assumption is with references to Wiggins, Strawson, Davidson, Frege, Wittgenstein, Quine, and Carnap. He points out that it seems to be regarded as "uncontroversial...harmless...perhaps unworthy of serious discussion" (p. 402). Evans says, "perhaps no one will deny it" (1982: 106).

the received Fregean view that two expressions that differ in informativeness must differ in meaning. I argue briefly against such Cartesianism in Coming (secs. 1.7, 1.8, 2.2) and have argued against it at much greater length elsewhere (1981a: 95-110; 1983: 674-5; 1991b: 270-5; Devitt and Sterelny 1989). In any case, I think that the onus lies very much on the other side. The supposition that someone who has a thought, or uses an expression, that has a certain meaning thereby has knowledge about that meaning is a strong one requiring much more support than it has ever been given (even if the knowledge is described as only "tacit").[3] I think that we should be skeptical of the supposition that semantic competence alone yields semantic propositional knowledge. My aim is for a semantics that does not make these suppositions.

My second assumption is already obvious. It is naturalism: that there is only one way of knowing, the empirical way that is the basis of science (whatever that way may be). So I reject "a priori knowledge." I do not give a detailed argument for my rejection but I do give two reasons (2.2): Briefly, first, with the recognition of the holistic nature of confirmation, we lack a strong motivation for thinking that mathematics and logic are immune from empirical revision; and, second, the idea of a priori knowledge is deeply obscure, as the history of failed attempts to explain it show.

My third assumption is implicit but nonetheless important. It is realism about the external world: that the physical world posited by science and common sense objectively exists independently of the mental. The chances that discoveries about meaning will cast doubt on this realism are, in my view, just about nil. I have argued for this at length elsewhere (1991b). Coming takes realism so much for granted that it hardly mentions it.

The methodological discussion was one natural way to start Coming. But I had a particular concern with semantic (or meaning) holism, and I did not need either the methodology or the previous three assumptions for my critique of the case for this holism. So I decided to make this critique Chapter 1 and the methodological discussion, Chapter 2.

The aim in Chapter 1 is not to defend an "atomistic" localism like Fodor's according to which no inferential property of a token constitutes its meaning. It is to defend a more moderate, "molecular," localism according to which a few of the inferential properties of a token may constitute its meaning. And I expect that we shall discover that many meanings are indeed constituted by inferential properties. In leaving open this possibility, Coming challenges the conventional wisdom that molecular localism is untenable because there is "no principled basis" for its distinction among inferential properties.

The first use of the methodology is in Chapter 3 to argue a case against semantic holism and for molecular localism. Chapter 1 rejects the arguments against there being a principled basis for distinguishing inferential properties alleged to constitute a token's meaning from its other inferential properties. Chapter 3 argues that, insofar as we need a principled basis, we have one.

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[3]Heidelberger notes that the assumption about knowledge of truth conditions is not "obviously true" (1980: 402). He makes what so far as I know is the first attempt to argue for it. He does not claim success. I attempted to construct an argument based on hints in the literature (in, e.g., McGinn 1980, Wright 1976, and particularly Dummett 1978), but it turned into a travesty (1991b: 270-2). Apart from some brief and inconclusive remarks of Dummett (1981: 310-11), that seems to be all that has been said for the assumption.

The methodology is used next, in Chapter 4, to present a program for a particular localistic semantics. This program is "Representationalist": It holds that the meanings of sentences are entirely constituted by the properties that go into determining their truth conditions and that the meanings of words are entirely constituted by properties that go into determining their references. (So Representationalism is in the spirit of the slogan "The meaning of a sentence is its truth condition.") Arguing for this program requires rejecting "two-factor," functional- (conceptual-) role, verificationist, and "use" theories. The methodology is used finally, in Chapter 5, to reject "narrow"-meaning theories and other forms of revisionism and eliminativism.

Representationalism is, of course, common in semantics. The most notable thing about my program is the argument for it based on the proposed methodology. Also notable is the claim that a token has more than one meaning. With this claim goes partial acceptance and partial rejection of two influential views of singular terms: first, the "`Fido'-Fido," or "Millian," view, recently resurrected by direct-reference theorists, that a term's only meaning is its property of referring to its bearer; second, the Fregean view that a term's only meaning is its "mode of presenting" its bearer. Coming argues that a term has both meanings. I agree with Frege that the meaning that is a mode may be descriptive, involving inferential links to other terms; that is my molecularism. I disagree with Frege, and just about everybody else, in arguing that some meanings are nondescriptive causal modes of reference.

I mostly call what I propose a "program" rather than a "theory" because I do not go into a lot of semantic details; in particular, although I talk about reference all the time, I say very little to explain it. This does not reflect any lack of interest in these details (see my 1981a, for example). Rather, the aim of Coming is to focus attention on the more general question: Which way should semantics go in future?

A CRITIQUE OF THE CASE FOR SEMANTIC HOLISM (Chapter 1)

At its most extreme, semantic holism is the doctrine that all of the inferential properties of a token in language or thought constitute its meaning. Holism is supported by the consideration that there is no principled basis for molecular localism's distinction among these properties. Coming rejects four arguments for this consideration.

The first argument, derived from Quine, starts from the epistemological view, confirmation holism: Put extremely, perhaps too extremely, the justification of a sentence depends on the justification of every other sentence. Combining this with the semantic view, verificationism, yields a simple argument for semantic holism. For, according to verificationism, the meaning of a sentence is its method of justification. So if that justification depends on every sentence then the meaning does as well.[4] Coming dismisses this argument quickly because there is no compelling reason to accept verificationism.[5]

The second argument, from the rejection of analyticity, is more popular. The molecular localist allows a distinction between the inferential properties of a token that constitute its meaning and those that do not. This is alleged to lead to an analytic-synthetic distinction that Quine has discredited in "Two Dogmas" (1953: 20-46). For, Quine argued that no belief, not even a law of logic or mathematics, is immune to revision in the face of experience; the web of belief is "seamless." The contrasting view -- that the web is seamed -- was a major motivation for traditional doctrines of analyticity. According to those doctrines, analytic beliefs have a privileged epistemic status; they are known a priori and are empirically unrevisable. So Quine's "Two Dogmas" stands clearly opposed to the epistemological aspects of traditional doctrines of analyticity.

This argument against molecular localism fails because that localism is not committed to any of these epistemological aspects of traditional doctrines.[6] Localism is strictly a semantic doctrine, making no epistemic claims at all. Insofar as it is committed to a doctrine worthy of the name "analyticity," it is to a nonepistemic doctrine. Suppose that the meaning of a token of `bachelor' is partly constituted by its inferential connections to tokens of `unmarried'. Then `All bachelors are unmarried' will be true partly in virtue of that fact about the meaning of `bachelor' and partly in virtue of the logical truth, `All unmmarrieds are unmmarried'. So it will be analytic in the sense that it can be "reduced by definition" to a logical truth. That is all that the localist would be committed to: viz, a semantic claim that says nothing about the epistemic status of `All bachelors are unmarried'. To give that sentence a privileged epistemic status we must add assumptions about our knowledge of meanings and about our knowledge of logical truths. There is no compelling reason for the localist to make any assumptions contrary to Quine's naturalized epistemology.

Consider logical truths. The localist may think that these, like any other, are true partly in virtue of their meaning and partly in virtue of the way the world is. The logical truths do of course enjoy a privileged epistemic status of some sort, but there need be no more to this than Quine indicated: A sentence earns its place on the list of logical truths by having a certain centrality in our web of belief.

Consider meanings. Cartesianism is still rife in this realm. Linguistic (conceptual) competence is thought to give "privileged access" to meanings. Merely understanding the words `bachelor' and `unmarried' (merely having the concepts **BACHELOR** and **UNMARRIED**) yields propositional knowledge of their meanings, including the relations these meanings have to each

[4] Quine 1960: 12-13; 1969: 80-1; 1981: 70-1. (Quine regrets the suggestion in "Two Dogmas" that the unit of significance is the whole of science: A substantial body of theory is sufficient; see 1991: 268.) There are signs of the argument in Putnam 1983: 144-7. Fodor summarizes the argument (1987: 62-3); so does Loar (1982: 273), attributing it to Harman.

[5] Fodor and Lepore (1992: 37-58) argue that there is another crucial flaw in Quine's argument: Confirmation holism presupposes semantic localism.

[6] Fodor and Lepore seem to think otherwise: 1992: 57-8; 1993b: 309; Lepore and Fodor 1993: 673-4.

other. And this knowledge can be brought before the conscious mind by "analysis." The localist need not go along with this Cartesianism. She can see competence not as semantic propositional knowledge but as an ability or skill: It is knowledge-how not knowledge-that. Not only can she have this view of competence but, as I have already claimed, she should have.

One common version of the analyticity argument takes localism to be committed, unacceptably, to sentences being unrevisable without meaning change.[7] Coming argues that this version is a storm in the teacup. The localist can hold to the only kind of revisability that should concern Quinean epistemology: that any belief is open to disconfirmation and that the empirical way of disconfirming is the only way. Meaning change is beside the point. The only unrevisability that localism might involve is not epistemological but harmlessly "metaphysical" (1.9-1.11).

The third argument, briefly, is one from psychological explanation. Coming discusses a version of it due to Ned Block (1991: 60-1), based on Hilary Putnam's "Ruritania" example (1983: 144-7) and concerned with "narrow" meaning.[8] The argument is rejected because it begs the question (1.14-1.15). The fourth argument, briefly again, is one from functionalism. It needs to be accompanied by a further argument that functionalism is essentially holistic. In any case, it could only establish a very mild holism (1.16-1.17).

To complete the case against semantic holism, we need to present the positive case for localism. That is the task of Coming Chapter 3. But, first, we need to tackle the methodological questions that began this precis. Answering these questions will help not only to rid us of holism but also to address other semantic issues.

THE METHODOLOGY OF NATURALISTIC SEMANTICS (Chapter 2)

To give a methodology for semantics, we must start with the question: 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.[9]

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[7]See, e.g., Lycan 1991: 112; Levine 1993.

[8]Coming returns to Ruritania arguments later when discussing narrow meanings (5.10-5.11).

[9] "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).

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." 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.

In Coming I seek a solution to this problem by focusing on the purposes for which we ascribe meanings (or contents) 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.

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.

Semantics very often proceeds without any attempt to explicate the talk of "meaning" used, implicitly or explicitly, to define the task(s): The talk is simply taken for granted. Given the prima facie unclarity and vagueness of the 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.[10]

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

[10] Similar remarks apply to psychological research into the nature of concepts, for this is, in effect, investigating the meanings of mental tokens: We need an explication of the talk of concepts together with a motivation for the research arising from the role of concepts thus explicated in psychology.

(2.7). But my point is <u>not</u> that the properties that I have called "meanings" are the <u>only real</u> meanings nor that the tasks I have called "semantic" are the <u>only proper</u> 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.[11]

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 might explain one bit of behavior; another, another; and a third might serve as a guide to external reality. <u>Coming</u> argues later that tokens really do have many meanings.

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.

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. <u>Coming</u> 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 <u>real</u> meanings. So I make my <u>second methodological proposal: We should look to the descriptive task for evidence for the normative/basic one</u>. 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.

Turn now to the methodology of the descriptive task. <u>Coming</u> approaches this by considering descriptive tasks <u>in general</u>. 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 have 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 <u>F</u>'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 <u>F</u>'s and non-<u>F</u>'s. Second, we must examine the examples to determine the nature of **being an <u>F</u>**.

The second stage is a straightforwardly scientific one. <u>Coming</u> says nothing about it beyond noting an important feature: The examination can lead us to reject some of the results of the first stage; apparently uncontroversial examples can turn out to be controversial. Thus, we may conclude that some of the things identified as <u>F</u>'s are not; for example, whales are not fish. We may conclude that some things identified as non-<u>F</u>'s are <u>F</u>'s; for example, tomatoes are fruits. We may even conclude that <u>none</u> of the things identified as <u>F</u>'s are <u>F</u>; for example, there are no witches.

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[11]It is worth noting that my view of the semantic tasks leaves it open whether truth has anything to do with semantics. Hence it does not beg the question against deflationary truth in favor of Representationalism's "correspondence" theory.

What about the preliminary first stage. How do we identify the examples that are to be examined in the second stage? In the absence of a well-established theory we must consult the experts about F's and see what they identify as F's and non-F's. Often these experts will be scientists. Sometimes they will be engineers, tradespeople, and the like. And sometimes they may be just plain folk. But, whoever they are, our first stage consists in conducting "identification experiments" with them.

What we elicit in these experiments are the experts' most basic "intuitions" about **being an F**. We can elicit other intuitions, by asking the experts not only to identify F's but also to tell us about them. These other intuitions are clearly richer and less basic than the identification ones: A person may be good at recognizing F's without having much reliable to say about them; this may be the situation of the folk with pains.

So, we have found a place for intuitions. It is important not to exaggerate that place. At best the intuitions are likely to be seriously incomplete. And they may be wrong: They are empirical responses to the phenomena. Our earlier brief consideration of the second stage shows that even the most basic intuitions, expressed in identification experiments, are subject to revision in the face of scientific examination. Intuitions are often needed to identify the subject matter for the descriptive task, and may be otherwise helpful, but nothing ultimately rests on them.

What about thought experiments? These experiments can have a role in identification. Instead of "real" experiments that confront the expert with phenomena and ask her whether they are F's, we confront her with descriptions of phenomena and ask her whether she would say that they were F's. These thought experiments provide valuable clues to what the expert would identify as an F or a non-F. They can do more: The descriptions that elicit the expert's response indicate the richer intuitions that can be a useful guide to the second stage. Valuable and useful as thought experiments may be in practice, they are dispensible in principle: We can make do with real experiments. And the results of thought experiments have the same empirical status as the results of real experiments.

Thought experiments performed in the special situation where theorists count themselves among the identification experts are the characteristic "armchair" method of philosophy, a famous example of which is "the analysis of knowledge." So Coming proposes a naturalistic account of that method. The naturalist does not deny "armchair" intuitions a role in philosophy but does deny that their role has to be seen as a priori: The intuitions reflect an empirically based expertise in identification.[12]

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

[12] Cf Bealer 1992.

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.

Finally, Coming turns to the method of investigating the meanings of attitude ascriptions themselves, "second-level" meanings (2.12). This investigation is dominated by philosophers who talk of "propositions," Platonic objects that are separate from the concrete spatio-temporal world of meaningful tokens. This prompts my fourth methodological proposal: that our investigations in semantics should be guided by the slogan, "Put metaphysics first."[13] It is common, and surely correct, to think that many meanings involve relations to objects. When considering what objects might play that role, we should be guided by general metaphysical constraints. These should make us reluctant to posit objects in semantics that we do not have good independent reasons for believing in. Coming later argues that we should not posit propositions (4.12).

A CASE FOR SEMANTIC LOCALISM (Chapter 3)

It is generally thought that the molecular localist must show that there is a principled basis for distinguishing any inferential properties of a token that she alleges constitute its meaning from its other inferential properties. In responding to this demand, we must distinguish two ways of construing it.

(a) If the demand were making a "descriptive" point, it would require that we distinguish the inferential properties that constitute any property that we do ascribe to a token for semantic purposes from the other inferential properties of the token. A consideration of analogous demands elsewhere shows that this demand should be dismissed. A property may be constituted localistically out of some properties and not out of others. That may be the way the world is and nothing more needs to be said. (b) It is more likely that the demand for a principled basis is making a "basic" point. The demand raises the question: What makes a property that we ascribe for semantic purposes -- a particular set of inferential properties -- a meaning? We must distinguish the inferential properties of a token that are meaning constituters from the other inferential properties of the token. We do need a principled basis here. And the framework of the last chapter provides one. A property -- hence the inferential properties that constitute it -- is a meaning if and only if it plays a semantic role and so is one we should ascribe for semantic purposes.

In sum, the roles played by properties and our purposes in ascribing them can provide a principled basis for ascribing localistic not holistic properties, hence, for there being localistic not holistic meanings.

We are left with an epistemic problem: showing that localistic properties not holistic ones meet this criterion and are meanings. Coming urges three arguments.

1. The argument from the success of our ascriptions: This is a straightforward application of the methodological proposals. First, I use the "ultimate" method to argue for

[13] My 1991b is, in effect, an extended argument for this slogan.

descriptive localism (third proposal): all the properties we do ascribe for semantic purposes are in fact localistic. So, given the success of our current ascriptions in serving those purposes of explaining behavior and guiding us to reality, we have good reason to suppose that the properties we ought to ascribe are localistic (second proposal). So we have good reason to suppose that meanings are localistic (first proposal) (Coming considers challenges to that success in Chapter 5.)

The argument for descriptive localism is as follows. It is clear from the folk's application of t-clauses that, for any putative word meaning, the folk see many different people as having thoughts that include tokens with that property. Thus, the folk ascribe to almost everyone "wheel-beliefs" like that the wheel is an important invention, "atom-beliefs" like that atoms are small, and "Cicero-beliefs" like that Cicero was an orator. Even a cursory examination of tokens that are identified by the folk as sharing a meaning shows them to differ enormously in their inferential properties. Thus, the alleged believers vary from engineers to mechanical ignoramuses, from physicists to morons, from Cicero's acquaintances to those who read about him in Word and Object. As a result there will be great differences in the inferential properties of their tokens, all of which are thought to have the putative meanings **WHEEL**, **ATOM**, or **CICERO**. So, a set made up of a large proportion of the inferential properties of one person's token will include many that are not properties of another person's. So, contrary to holism, it cannot be the case that the shared putative meaning is constituted by any such set: No such set is common and peculiar to the tokens to which the folk ascribe the one putative meaning.

2. The argument from our interest in generality: Consideration of a range of properties outside semantics - for example, **being a planet**, **being an echidna**, **being a capitalist**, **being a pain**, **being a wheel** - shows that, in general, whether our purposes are explanatory, practical, or perhaps even frivolous, we tend to ascribe and name properties that are localistic; only a small proportion of the natural candidates to constitute one of these properties really does constitute it. Why do we ascribe and name localistic properties? Because only such properties have the sort of generality we are interested in; localistic properties are likely to be shared by many things.

Thus, consider economic properties. Economic holism would require the specification of a distinct set of properties for each time slice of the world economy and so would be totally futile. We want to specify properties that play explanatory roles in many different complicated structures. The names for these properties may be parts of theories that are not in competition but are complementary. Thus, capitalists, money, and prices have roles in international trade, in some very different national economies, and in a great range of microeconomic activities. A property that can play the variety of roles of interest to economics - a property with that sort of generality - must be constituted by only a few of the relations of any one token that has that property; it needs to be localistic.

These considerations yield the simplest, least theory-laden, argument against semantic holism: We ought to ascribe localistic properties because only such properties have the generality that will serve our semantic purposes. Hence, only localistic properties play semantic roles. Hence, all meanings are localistic.

3. The argument from Representationalism: The last two arguments make, near enough, no appeal to any available theory in semantics. This is not the case with the final argument which rests on the popular, overarching theory, "Representationalism": the theory that word meanings are entirely constituted by referential properties. This theory will argued for next. Meanwhile, Coming argues, if the theory is correct, meanings cannot be holistic (3.11).

MEANINGS AND THEIR ASCRIPTIONS (Chapter 4)

I have argued that meanings are localistic. This raises the question: Which localist properties are meanings? Applying the methodology again, Coming argues for a program according to which meanings are representational properties.

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,[14] we find support for the descriptive version of Representationalism.

The focus is on definite singular term tokens. In their use of t-clauses, the folk seem to ascribe at least three different sorts of putative referential meaning to such a 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).

For an example of the first sort, consider

(1) Ralph believes that Ortcutt is a spy.

Construed opaquely the beliefs that utterers of (1) identify by "that Ortcutt is a spy" seem to have in common, so far as their singular terms are concerned, only the property of referring to Ortcutt by a certain mode, the mode exemplified by the name `Ortcutt' in (1) itself. A sentence in Ralph's "belief box" that would make (1) true would "contain `Ortcutt'," a token with a property of referring to Ortcutt by that mode.

For an example of the second sort, consider

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

Like (1) this ascribes to Ralph a belief that refers to Ortcutt but unlike (1) not a belief that refers to him by any particular mode. Yet, it is usual to think, not just any mode will do. There are various ways of trying to capture the extra constraint: Ralph must have "Ortcutt in particular in mind"; he must be "en rapport with" Ortcutt; his thought must have "a special sort of aboutness"; the thought must be "referential" and not merely "attributive." What is common and peculiar to the singular terms in beliefs that (2) would apply to is that they have this special rapport reference to Ortcutt.

For an example of the third sort, consider this story. Smith is found dead. Ralph, the famous sleuth, suspects murder. Furthermore, the manner of the slaying makes him think that whoever is responsible is insane. A newspaper reports this:

(3) Ralph believes that Smith's murderer is insane.

The murder was in fact committed by a well-known mobster:

(4) Big Felix is Smith's murderer.

Big Felix's moll knows this. She reads the newspaper and remarks to one of the mob:

(5) Ralph believes that Big Felix is insane.

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

This seems clearly true. Yet, construed opaquely, it is not true because Ralph would not express his belief using `Big Felix'. It is not true construed like (2), as rapport-transparent, because Ralph does not have anyone in particular in mind as the murderer. The truth of (5) seems to require the simply-transparent construal that allows Ralph to have his insanity belief of Big Felix under any mode of referring to him at all. The singular terms in beliefs that would make (5) true need share only the property of referring to Big Felix.

We now take this descriptive account of putative meanings as evidence of meanings. 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 do indeed 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. Exploration of the ways in which these ascriptions serve those purposes confirms this (4.3). (Coming defends this argument from various revisionist and eliminativist considerations in Chapter 5.)

What we most need in order to explain these referential meanings are theories of reference. Coming argues that three sorts of theory of reference are possible.

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. Description theories were once popular for proper names and natural-kind words, but they foundered because of "problems of ignorance and error": Speakers who seem perfectly able to use a word to refer are too ignorant to provide the appropriate descriptions of the 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, Putnam 1975). Aside from that, description theories are essentially incomplete, explaining the reference of some words in terms of the reference of others.

If we are to be naturalistic, some words 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.

Theories of reference of these three sorts will straightforwardly explain one of the referential meanings of a singular term: the transparently ascribed property of referring to an object. What about another, the opaquely ascribed property of referring to the object in a certain way? These may be constituted by descriptive modes of reference: This is a familiar molecularism. But it follows from the above discussion that some such meanings must be causal modes of reference. Coming illustrates this idea, so alien to the semantic tradition, with "IT," a historical-causal theory of names and other singular terms. (I proposed this theory in earlier works: 1974, 1981a, 1981b, 1989b, Devitt and Sterelny 1987.)

Having outlined its program, Coming turns to the rejection of rivals. `Fido'-Fido theories, hence direct-reference theories, are right about one meaning of a name: the transparently described meaning of referring to the name's bearer. Their error lies in denying any other meaning. This error is brought out by well-known problems, particularly the identity problem: The true identities `a = a' and `a = b' differ in meaning. I do not argue for this difference in the usual way, by appealing 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. So the two identities differ in meaning, a difference to be explained in terms of the different modes of reference of `a' and `b', very likely different causal modes. Coming argues against direct reference's contrary view and against its attempt to export the identity problem, and others, to the theory of mind or to pragmatics.[15] Direct reference's export strategies, and its exclusion of causal modes of reference from semantics,[16] are theoretically arbitrary and ad hoc. At bottom, I think that the popularity of direct reference arises from inattention to the methodological issues discussed in Chapter 2, in particular, the issue of semantic purposes.

Next, Coming considers "semi-Representationalist" two-factor theories. According to these theories, one meaning of a token -- what the theory calls "one factor" of its meaning -- is representational but the other is not. This other one is the token's property of having a certain conceptual or, better, "functional" role, a property constituted by its inferential relations to other tokens and by its relations to proximal sensory inputs and/or proximal behavioral outputs. These functional-role meanings are not modes of representation because they do not determine reference.

The evidence of our ordinary ascriptions does not support two-factor theories as descriptive theories. There seems to be no evidence that the folk ever ascribe a non-reference-determining functional-role putative meaning.

The evidence of the folk's (simply-)transparent ascriptions counts decisively against "anti-Representationalist" verificationist, use, and one-factor functional-role theories as descriptive theories. The only property that seems to be shared by the singular terms to which we ascribe the one putative meaning is the property of referring to a certain object, a representational property. Thus, consider the variety of tokens to which we might apply (transparently) `that Big Felix is insane'. These tokens involve `Big Felix', `Smith's murderer', `I', `he', `you', and any other way of referring to Big Felix. It is most unlikely that these share any epistemic or other functional-role property that could be the meaning.

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.

Coming considers the meanings of attitude ascriptions themselves - "second-level" meanings. It argues against the "Hidden-Indexical" theory that these meanings are extremely

[15]For examples of exportation to pragmatics, see Salmon 1986; Soames 1988: 104-5; Fodor 1987: 85-6. Salmon lists many others who have taken this path before (167n). For examples of exportation to the theory of mind, see Almog 1985 and 1986, Lycan 1985, Wettstein 1986, 1989a, and 1989b, and Fodor 1990: 166-72. Kaplan seems to be tempted (1989: 529-40, 562-3).

[16]See particularly, Salmon 1986: 70-1.

context dependent (4.11). It finds support for an "intimate link," usually identity, between the meaning ascribed and a meaning of the ascribing content sentence. Applying the slogan "put metaphysics first" (fourth methodological proposal), it argues that we should take ascriptions to concern concrete thoughts and utterances rather than Platonic propositions (4.12). Finally, it develops the program to handle various puzzles including those due to Mark Richard (1983), Hector-Neri Castaneda (1966, 1967), and Saul Kripke (1979). This leads to the positing of meanings that are even more "fine-grained" than those attributed by ordinary opaque ascriptions (4.14-4.17). The view that these puzzles support direct reference is rejected (4.18).

In effect, Coming argues for correspondence truth, for it argues need that truth because we need meaning. The properties we do, as a matter of fact, ascribe to serve our semantic purposes are truth-referential and nonepistemic. The apparent success of these ascriptions at serving our semantic purposes provides good evidence that we need to ascribe these truth-referential properties to serve those purposes.

ELIMINATIVISISM AND REVISIONISM (Chapter 5)

Revisionism rejects the semantic status quo: We ought not to ascribe the properties that we do ascribe for semantic purposes. Eliminativism is an extreme form of revisionism: We ought not to ascribe any properties for semantic purposes because nothing has a meaning. Revisionism can be less extreme: we ought to ascribe for semantic purposes properties other than the ones we do. Given the arguments above, I take the status quo to be Representationalist.

Coming starts by arguing that eliminativism is an empirical doctrine that is not open to dismissal by popular transcendental arguments to the effect that the doctrine is "incoherent" (5.2). Nevertheless, I think that its evidential support is weak.

The main case against revisionism, harped on already, is that our ordinary attitude ascriptions are apparently successful at serving our semantic purposes. This is evidence that thoughts and utterances have the properties ascribed and that the properties are meanings. And we have already argued that the properties ascribed are Representationalist. In the face of this, anyone who wants to overthrow this Representationalism needs both a powerful argument and a plausible alternative. This is a lot to provide.

Three sorts of evidence would count in favor of revisionism: (1) Evidence that the apparent success of our ascriptions was not real or was greatly exaggerated. (2) Evidence that a naturalistic explanation of Representationalist properties was not available or likely; so we would not be entitled to ascribe those properties. (3) Evidence that we could serve our purposes better in some other way; so we would not need to ascribe Representationalist properties.

After a brief critical look at evidence of sorts (1) and (2), offered by Patricia and Paul Churchland (5.3),[17] Coming focusses on two arguments that mainly offer evidence of sort (3), the argument from the computer analogy and the argument from methodological solipsism. (It should be noted that these arguments only concern the purpose of explaining behavior, not the purpose of guiding us to reality.)

[17] See, e.g., Patricia Churchland 1986; Paul Churchland 1981, 1989; Churchland and Churchland 1983. Paul Churchland 1993 is a nice summary.

Neither argument supports the view that psychology should ascribe only syntactic properties,[18] strictly understood, to mental states. Such properties may be adequate for the explanation of thought processes but they are not for the explanation of thought formation or behavior. The mind as a whole is not purely syntactic at any level even the implementational. In arguing this, Coming emphasizes three distinctions that tend to be overlooked in the debate: first, that between a token's intrinsic fairly brute-physical properties like a shape, which I call "formal" properties, and syntactic properties, which are extrinsic functional properties that a token has in virtue of its relations to other tokens in a linguistic system; second, that between processes that hold only between thoughts, and mental processes in general, which may involve not only thoughts but also sensory inputs and behavioral outputs; and, third, that between syntactic properties, which are constituted only by relations between linguistic tokens, and putative narrow meanings, which involve relations to nonlinguistic entities - for example, sensory inputs and behavioral outputs - as well.

The argument from methodological solipsism may seem to support the view that psychology should ascribe only narrow meanings. To assess this support we need to distinguish two views of narrow meaning. According to one,[19] the narrow meaning of a sentence is a function taking an external context as argument to yield a wide meaning as value. So, on this view, narrow meanings partly determine truth conditions and reference and so are Representationalist; they are intentional wide meanings `minus a bit'; they are `proto-intentional'. The belief that we need only these meanings to explain behavior is, therefore, only moderately revisionist. According to the other, more popular, view of narrow meaning,[20] that meaning is a functional role involving other sentences, proximal sensory inputs, and proximal behavioral outputs. These putative meanings are not Representationalist and differ greatly from the meanings that we currently ascribe. The belief that we need only these meanings to explain behavior is, therefore, highly revisionist.

Coming finds some truth, though not enough, in the argument for the moderately revisionist view, but none at all in the argument for the highly revisionist one.

Narrow meanings as functions must be acceptable to someone who believes, as I do, in wide representational meaning. For, a theory that explains those wide meanings will explain the narrow ones. And these narrow meanings would indeed yield explanations of behavior. Coming argues, however, that the idea that we should proffer these explanations instead of the usual wide ones is mistaken (5.12).[21]

Many narrow meanings (as functions) are likely to be `coarse grained' in that there is not much to them and `promiscuous' in that they can yield any of a vast range of wide meanings as values by changing the relevant external context as argument. These narrow meaning could serve psychological needs if the behaviors that needed to be explained were themselves only narrow and

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[18]Cf Stich 1983; but see also Stich 1991.

[19] To be found, e.g., in White 1982; Fodor 1987: 44-53.

[20] To be found, e.g., in Loar 1981 and 1982, McGinn 1982, Block 1986.

[21]This goes against the revisionism of my 1989a and 1991a.

`proto-intentional'. The objection to restricting psychology to explanations involving narrow meanings is then that we do not need to explain only proto-intentional behavior. For, that behavior is also likely to be coarse grained and promiscuous. The intentional behavior, giving water to Mary involves giving, water, and Mary. So too does the related proto-intentional behavior in one context, but that behavior might, in some other context, involve taking, kicking, or many other acts; XYZ, or many other stuffs; Twin Mary, or any other person. We certainly want to have the more discriminating intentional behavior explained somewhere. No good reason has been produced for thinking that it should not be explained in psychology.

Coming is much more critical of the more popular functional-role narrow meanings urged by two-factor theorists. It argues that these putative meanings are left almost entirely unexplained and mysterious. Even if they were not, we have been given no idea how such meanings could explain intentional behaviors (like giving water to Mary) and it seems very unlikely that they could. If they do not explain these behaviors, then revisionism requires that intentional behaviors be denied altogether; for if there are these behaviors and they are not explained by narrow meanings then it is not the case that psychology should ascribe only narrow meanings. We have been given no reason to deny intentional behaviors (5.11). The argument from methodological solipsism does nothing to solve these problems for functional-role meanings. This is very bad news for the highly revisionist doctrine that psychology should ascribe only these putative meanings. That doctrine has a heavy onus arising from the apparently striking success of our present practice of ascribing wide meanings to explain behavior. Why do these ascriptions seems so successful if they are not really? What reason have we for thinking that the ascriptions that would be recommended by this revisionist doctrine would do any better? The doctrine has hardly begun to discharge its onus.

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