

AGAINST DIRECT REFERENCE

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It is easy nowadays to get caught up in direct-reference mania. (Salmon 1986: 82)

1. The Theories

The origins of the theory of "direct reference" for proper names are alleged to be found in the works of Saul Kripke, Keith Donnellan, and David Kaplan. Sometimes what may seem to be the same theory of names is called "new"; sometimes, "Millian"; sometimes, "causal"; sometimes, "historical."

Despite appearances, there is not one theory covered by these various names but many. My aim is to distinguish these theories and their origins, and to argue against one of them. My own views have the same sources as the views of direct-reference philosophers but differ sharply in concern and content. The implications of this disagreement stretch way beyond the theory of names. Time and again the disagreement comes back to questions about the nature of semantics.

In this section, I set out the theories and some of the relationships between them. In the next section, I focus on the history, examining particularly the extent to which these theories are correctly attributed to Kripke, Donnellan, and Kaplan. In the following sections, I get on with the argument.

The 'Fido'-Fido Theory

The theory that I shall be arguing against is prominent in the discussion of direct reference. It is:

All there is to the meaning, semantics, information value, or linguistic significance of a name is (standing for)¹ its referent. A name is purely designative or denotative; it is just a tag; it merely labels.

The 'Fido'-Fido theory has problems that have been familiar since Frege and the early Russell:² the differing "cognitive values" of ' $a = a$ ' and ' $a = b$ ' which I shall call "the Identity Problem"; the nontriviality of true positive existence statements and the meaningfulness of true negative ones, which I shall call "the Existence Problem"; the meaningfulness of empty names, which I shall call "the Emptiness Problem"; the failure of substitutivity of identicals in thought, or propositional attitude, ascriptions, which I shall call "the Opacity Problem." These problems seem so massive that it is startling to find the 'Fido'-Fido theory revived by able philosophers who are thoroughly aware of the problems.

¹ The distinction between the theory with the words in parentheses and without them is unimportant for this paper and will be ignored.

² Though Russell rejected the 'Fido'-Fido theory for ordinary proper names, he did of course hold it for logically proper names.

The main reasons for the revival come from the other theories appearing under the banner of direct reference and set out below. My first point against the 'Fido'-Fido theory is that these other reasons are insufficient.

The Nondescription Theory

One of these other theories, often presented in the same breath as the 'Fido'-Fido theory as if it were the same, is as follows:

A name is nonconnotative. It does not have a Fregean sense determining its reference. It is nondescriptive.

Although the Nondescription theory is entailed by the 'Fido'-Fido theory, it is not the same as that theory. Why might someone think otherwise? Because of the following semantic presupposition:

SP: The meaning of a name is either descriptive or else it is the name's referent.

SP presupposes that there are no other possible candidates for a name's meaning. So, if the Nondescription theory is right, the meaning of a name must be the name's bearer: the 'Fido'-Fido theory. I shall argue against SP. I think that a name has a certain sort of nondescriptive, hence non-Fregean, sense.

To argue against SP is to take it as a substantive thesis about meaning. However, the discussion of direct reference sometimes proceeds as if SP were true by definition. This would collapse the 'Fido'-Fido theory into the Nondescription theory and leave us without a complete *nontrivial* theory of meaning for names. So, I shall be arguing that 'Fido'-Fido is either false or the result of a trivial addition to Nondescription.

Why do some people behave as if SP were trivial? My diagnosis is that they pay too little attention to what we need a theory of meaning *for*.

The Rigid Designation Theory

A third theory associated with direct reference is as follows:

A name refers to the same object in every possible world.

We can derive the Rigid Designation theory from the Nondescription theory as follows. First, we need an explanation of "reference in a possible world." Consider a singular term, *T*, in a sentence, *S*, in a particular context of utterance. The referent of *T* in a possible world, *W*, is whatever object *T* makes relevant to the truth evaluation of *S* in *W*.³ Second, we need an assumption about what makes reference vary from world to world. Suppose that *T* were a definite description like 'the President of America in 1989'. It seems obvious that *T* would vary its reference because different objects would be picked out by the description in different circumstances. The needed assumption is that *only if T* is descriptive can its

³ The apparent commitment to possible worlds in this explanation could be removed. Let '*D*' abbreviate a total description of *W*. To say that *x* is the referent of *T* in *W* is to say that, were it the case that *D*, *x* would have to have the attribute specified by *S* for *S* to be true.

reference vary. So, given the explanation and this plausible assumption, the Nondescription theory implies that a name refers to the same object in every possible world.

Rigid Designation does not entail either Nondescription or 'Fido'-Fido. So far as Rigid Designation is concerned, a name can have any meaning at all provided only that that meaning does not make any object other than the name's actual referent relevant to evaluations in other possible worlds. This proviso will be met if the name's meaning is the referent, as 'Fido'-Fido holds. But we need further argument to show that this is the *only* way to meet the proviso. Indeed, Alvin Plantinga has shown, ingeniously, that a descriptive name could meet it (1978). Even if we use the Nondescription theory to rule out Plantinga's suggestion, we still need to rule out the possibility of other ways of meeting the proviso. Perhaps a term with a nondescriptive sense would meet it. That possibility could be ruled out by the semantic presupposition, SP, but then if we had SP as well as Nondescription, we wouldn't need Rigid Designation to establish 'Fido'-Fido.

There is no fast and clean route from Rigid Designation and Nondescription to 'Fido'-Fido. I shall examine a slow route later (section 3).

I have deliberately presented these three theories without using two terms that are prominent in the discussion of direct reference: 'proposition' and 'content'. These technical terms are open to various interpretations some of which are appropriate to one of the theories, some to another. The main reason that the discussion is so confusing is that the terms are often inadequately explained and thus tend to blur distinctions between the theories. I will introduce the terms later (section 3).

The Causal Theory

The final theory associated with direct reference is as follows:

A name designates an object solely in virtue of a certain sort of causal or historical chain connecting the name to the object.

The Causal theory clearly entails the Nondescription theory, because it entails that reference is not determined by a Fregean sense. Further, if we accept the earlier explanation of "reference in a possible world," we can derive Rigid Designation from the Causal theory. A name's causal links to an object in the actual world of its utterance—the links that determine its reference—remain fixed however we vary the world of evaluation. Indeed, the Causal theory *explains* why a name has the semantic property of being rigid.

I am enthusiastic about the Causal theory.⁴ So I am very concerned to reject the common assumption that it entails the 'Fido'-Fido theory. Clearly, as it stands,

⁴ Though I think a few qualifications are necessary. One is needed to allow for "attributive" names; 1974: 196; 1981a: 40–2. Another is needed to allow for the *qua*-problem; 1981a: 60–4; Devitt and Sterelny 1987: 63–5.

it does not, for it says nothing about the meaning, information value, etc., of a name. Indeed, I shall argue that the Causal theory provides the means to reject the 'Fido'-Fido theory, because it provides an explanation of the nondescriptive sense of a name. I have argued this several times before, with no apparent success.⁵ In this paper, I hope to do better.

My main aim in this section has been to emphasize that neither the Nondescription theory, the Rigid Designation theory, nor the Causal theory entail the 'Fido'-Fido theory. The most important additional premise to establish that theory is SP. If my argument is good, SP is false.

2. The History

Kaplan introduced the term "direct reference" in the mid-seventies in works that were mainly on the semantics of demonstratives and indexicals (1973; 1975; 1979a, 1979b, 1988a).⁶ Neither Kripke nor Donnellan used the term. And neither they, nor Kaplan, called their theories "new."

The 'Fido'-Fido Theory

Insofar as the theory of direct reference is this theory, it is remarkably inappropriate to call it "new" as some do (e.g., Wettstein 1986):⁷ 'Fido'-Fido is the oldest theory in the book, going back at least to Plato. Calling it "Millian" is much more appropriate, though strictly it seems not to be what Mill held. The theory has been unpopular for most of this century, but it was by no means dead when the direct-reference philosophers revived it recently.⁸

Who did revive it? Kripke and Donnellan are often mentioned in discussions of direct reference as if they endorsed the 'Fido'-Fido theory. Yet, interestingly enough, nobody makes a serious attempt to cite convincing evidence that they do.⁹ Kripke clearly does not endorse it (1980: 20-1). I have been unable to find any decisive evidence that Donnellan does. In one place, he flirts with a view that might seem to come close (1974), but the view seems more appropriately construed as Rigid Designation. Kaplan does, somewhat tentatively, endorse the theory (1988a: 591), although it is not what he *means* when he says that names are directly referential. And names are not his primary concern.

⁵ 1980: 271-4; 1981a: 152-7; 1981c: 217-8; 1984a: 388, 403-5; 1985: 222-3. I have also argued a similar line to do with natural-kind terms: 1983: 675-7.

⁶ Kaplan 1988a, which seems to have made the introduction, circulated widely in an unpublished form from 1977 on. On the semantics of demonstratives and indexicals, see also Perry 1977, 1979.

⁷ Stephen Schwartz, who seems to have been first to talk of the "new theory of reference," did not include the 'Fido'-Fido theory as a "main feature" of the new theory (1977b: 20-32).

⁸ See, e.g., Smullyan 1947: 140; Marcus 1961: 309-10; 1981.

⁹ E.g., Loar 1976 (cf. my 1980); Ackerman 1979a: 58; 1979b: 6; Schiffer 1979 (cf. my 1981c); Marcus 1981: 502; Almog 1984: 482; Baker 1982: 227; Wettstein 1986: 187. So far as I know, Loar was the first to attribute the 'Fido'-Fido theory to Kripke and Donnellan.

'Fido'-Fido has flowered in the work of a "new wave" of philosophers who sail under the banner of direct reference: Joseph Almog, Nathan Salmon, Scott Soames, and Howard Wettstein.¹⁰

The Nondescription Theory

This theory is entailed by the 'Fido'-Fido theory and it is just as inappropriate to call it "new." It is, however, appropriate to call it "Millian," because Mill is its most famous exponent. There is no doubt that Kripke (1980), Donnellan (1972), and Kaplan (1988a) do subscribe to this theory. Furthermore, Kripke and Donnellan did something importantly new in relation to it. At a time when its opposite, the Description theory, had been the received view for decades, they produced powerful arguments for the Nondescription theory.¹¹ The Nondescription theory has been enthusiastically embraced by all the direct-reference philosophers.¹²

The Rigid Designation Theory

The Rigid Designation theory has much more claim to be considered "new" though it is, in effect, to be found in Ruth Barcan Marcus (1961).¹³ Kripke is famous for urging the theory (1971, 1980). When Kaplan says that names are "directly referential," Rigid Designation is part of what he means.¹⁴ The other part is Nondescription (1988a: 512-6, 521-6). Donnellan did not discuss the Rigid Designation theory in his early articles. However, his idea of a "referential use" of definite descriptions (1966, 1968) was suggestive of the idea, as Kaplan points out (1979a: 383-5). Donnellan did embrace the theory later (1979: 50).

The main interest of the Rigid Designation theory was for logic and formal semantics; in particular, for intensional logic and possible-worlds semantics. The new wave are very interested in the theory.¹⁵

¹⁰ Salmon 1981: 11; 1986; Almog 1984: 482; 1985: 615-6*n*; Wettstein 1986: 185, 192-4. I take the theory to be implicit in Soames 1985, 1987, 1988. John Perry is a direct-reference theorist and is often cited as if he subscribed to 'Fido'-Fido, but I can find no clear evidence that he does; but see, e.g., Barwise and Perry 1983: 165. Though the terms 'proposition' and 'content' often make the discussion confusing (section 1), I do not mean to suggest that the new wave is confused.

¹¹ The argument that has had the most attention is a modal one, derived from the Rigid Designation theory, and found in Kripke. The other argument, which strikes me as more powerful, is found in Kripke and Donnellan. It points out that people mostly do not have the knowledge of a name's bearer required by the Description theory. I call it "the argument from ignorance and error."

¹² Salmon 1981: 16; 1986: 65-6; Wettstein 1986: 185-6; Almog 1986: 220; Soames 1988: 100.

¹³ See also Smullyan 1947; Fitch 1949.

¹⁴ Kaplan has a subtle discussion of a difference between his formulation of Rigid Designation and Kripke's (1988a: 521-2). I have used Kaplan's formulation.

¹⁵ Salmon 1981: ch. 1; 1986: chs. 1-2; Soames 1985, 1987, 1988; Wettstein 1986: 186; Almog 1986.

The Causal Theory

This theory is the one that really is “new.” It was discovered by Kripke (1980) and Donnellan (1972) in the late sixties¹⁶ Kaplan seems always to have been impressed with the theory but, in the end, gives it no semantic significance (1988a: 587–92).

The new wave of direct-reference philosophers typically associate their views with the Causal theory but, of the four theories, it clearly *interests* them least.¹⁷ Their concern with the question, ‘What determines the reference of a name?’, seems to evaporate once they have concluded that the reference is not determined by a Fregean sense.

Consider now the link between the ‘Fido’-Fido theory and the Causal theory in the works of the philosophers mentioned. Kaplan and the new wave embrace the ‘Fido’-Fido theory but show little interest in the Causal theory. Kripke and Donnellan hold the Causal theory, but the ‘Fido’-Fido theory is not to be found in any of their works. So the historical link between the two theories is rather tenuous. It is striking then that the received view seems to be that the Causal theory is a ‘Fido’-Fido theory.¹⁸ I am most concerned to break the link between the two theories.

Because of the above history, my term, “the direct-reference theory of names,” should be taken as referring to the conjunction of the ‘Fido’-Fido theory, the Nondescription theory, and the Rigid Designation theory, but not to the Causal theory. And by “the direct-reference philosophers,” I mean Kaplan and the new wave, not Kripke and Donnellan.¹⁹

In earlier works, I have proposed a theory that developed ideas of Donnellan and Kripke in two ways.²⁰ First, I drew a distinction at token level, based on Donnellan’s distinction at type level, between referential and attributive descriptions,²¹ and then I applied this new distinction across the board, covering names, demonstratives, and pronouns, as well as definite descriptions. Second, I gave a

¹⁶ See also Chastain 1975, an interesting but neglected article.

¹⁷ Salmon 1981: xiii, 11; Almog 1984; Wettstein 1986: 192–3; 1988: 420.

¹⁸ See references in note 9; also, McGinn 1982: 244; Lycan 1985; Block 1986: 660, 665; LePore and Loewer 1986: 60; Wagner 1986: 452.

¹⁹ I might have included others among the direct-reference philosophers. For example, Lycan subscribes to direct reference and his “paradox of naming” starts from SP (1985). Fodor proposes a “denotational theory” which is, in effect, a direct-reference theory, though he does not mention any of the above literature in his discussion (1987: 72–95). The philosophers I have included are closely related to one another. The detailed discussion of a larger group would have been unmanageable.

²⁰ I became interested in the semantics of singular terms in discussions with Charlie Martin in Sydney in 1966. Martin had a rigidity thesis for names and demonstratives and a view of definite descriptions that was similar to the view that Donnellan was about to publish (1966, 1968). I first heard the causal theory of proper names from Kripke at Harvard in 1967. I proposed my own theory in my Ph.D. thesis (1972). The following later works were largely based on that: 1974, 1976, 1981a.

²¹ This treatment gives Donnellan’s distinction a semantic significance that he may not have intended. Kripke thinks that the distinction does not have semantic significance (1979a). My 1981b is a response to Kripke.

causal theory of reference for all the referential tokens.²² The theory explained their reference in terms of causal chains—I called them “d-chains”—generated by “groundings” in an object²³ and by “reference borrowings” in communication.

In the course of theorizing about the reference of names, I talked also about their meaning. I claimed, in effect,²⁴ that Frege was right in thinking that a name had a sense which determined its reference—it had a mode of presentation—but wrong in thinking that this sense was descriptive. The sense was a certain type of d-chain (1974: 203–4; 1981a: 153–7). Given the new popular idea that the Causal theory is a ‘Fido’-Fido theory, my view about a name’s meaning has turned out to be much more radical than I expected. I adopted the view for two reasons. First, a name must have *some* property that determines its reference—it does not refer by magic—and, according to the Causal theory that property is the relational one of being causally linked in a certain way to the referent. In brief, an interest in the *explanation* of reference pointed to a sense of the sort I was proposing. Second, if a name had no sense, it would be impossible to solve the familiar problems that had originally driven people away from the ‘Fido’-Fido theory. I took solving those problems to be a requirement on a theory of names (1981a: 6).

The solution to these problems, briefly, is as follows. The Identity Problem: ‘*a* = *a*’ and ‘*a* = *b*’ have differing cognitive values because they have different senses; for underlying ‘*a*’ and ‘*b*’ are different types of d-chain. The d-chains are of different types in virtue of being parts of different “networks” of d-chains. The Existence Problem and the Emptiness Problem: the meaningfulness of a name does not depend on it having a referent; it is meaningful if it has an appropriate network underlying it even if that network is not grounded in a referent. The Opacity Problem: substitutivity does not hold for a name in an opaque thought ascription because the ascription depends for its truth on the sense of the name not on its referent. I shall return to these solutions in section 7.

It follows from my view that SP is false. Fregean senses and referents are not the only candidates for a name’s meaning: its meaning is a non-Fregean sense explained in terms of a causal network.

I have already noted the lack of interest of direct-reference philosophers in explaining how names relate to the world. Indeed, though they trace their views

²²Donnellan does not offer a theory of reference for his referential descriptions. In particular, he does not extend his causal theory of names to cover those descriptions (though he does remark in a footnote that such a description is a “close relative” of a proper name; 1972: 378n).

²³It is important to the plausibility of the Causal theory that it allows a name to be *multiply* grounded in its referent, not simply grounded in an initial dubbing (1974: 198–9; 1981a: 56–7).

²⁴The qualification is necessary because of my caution with the *word* ‘sense’ (and the *word* ‘meaning’). Initially, I was anxious to emphasize the difference between the Causal theory and the Description theory. This led me to use ‘sense’ as if it applied only to *Fregean* descriptive senses and hence to deny that names had senses (1974). Later I allowed, tentatively, that we might think of causal modes of presentation as non-Fregean senses (1981a: 236). Had I anticipated the rise of direct reference, I would have emphasized the difference between the Causal theory and the ‘Fido’-Fido theory by not being at all tentative about this. I am not tentative now (Devitt and Sterelny 1987: 56–8).

back to Russell, they set aside almost without comment the part of Russell that was an attempt to solve this problem: the theory of acquaintance. Their lack of interest in this ultimate question seems to reflect a narrower view than mine of the scope of semantics.²⁵

This difference over the nature of semantics is related to another one. My concern is always with language as a natural phenomenon and so the guide for its investigation is empirical science. The direct-reference philosophers tend to be more concerned with language as an abstract system and so the guide for its investigation is formal logic.

3. Motivating the 'Fido'-Fido Theory

Given the well-known, and apparently overwhelming, problems for the 'Fido'-Fido theory, why has it been revived? The above discussion provides some ideas. In this section I shall develop these ideas in the process of discussing four possible motivations for the theory. Motivations 3 and 4 are certainly influencing the direct-reference philosophers; 1 and 2 probably are too.

1. Suppose that a person starts with the common assumption:

- (1) The meaning of a sentence is the proposition it expresses.

The person adds, perhaps under the influence of some reflections on "ordinary language":

- (2) The proposition expressed by a sentence is what the sentence says.
 (3) A name's contribution to what a sentence containing it says is simply the name's bearer.

From these three premises, it follows that a name's bearer is its contribution to a sentence's meaning. Add in the uncontroversial,

- (4) The meaning of the name is its contribution to the meaning of a sentence containing it,

and the person has arrived at the 'Fido'-Fido theory.

The problem with this argument is its first three premises. (1) and (2) identify the meaning of a sentence with what it says. Suppose we accept that. What *does* a sentence say? Our answer must be guided by the truth conditions of indirect speech reports. But then as Quine and others have shown, these truth conditions are tricky. Consider:

Tom said that Cicero is an orator.

²⁵A lack of interest in this ultimate question has been common among philosophers of language. Consider the long period of rule of the Description theory. Even if the theory had been right, it would not have answered this question. It is essentially incomplete, explaining the reference of one term in terms of the reference of others. But how do *they* refer? Description theories simply pass the referential buck. The buck must stop somewhere with a different sort of theory. This point relates to Putnam's famous arguments for the slogan "Meanings just ain't in the head" (1975: 223-7); see Devitt in press a.

Suppose that the actual words Tom uttered were, "Tully is an orator." Is the report true? According to one popular view, yes and no. It is true if construed transparently. On the basis of that construal, we might feel justified in saying that what is said by "Tully is an orator" is the same as what would have been said by "Cicero is an orator," thus confirming (3). However, if the report is construed opaquely it is false. On the basis of that construal, what is said by the two utterances is not the same, thus falsifying (3). So, for the argument to go through, we have to identify the meaning of a sentence with what is said in the transparent sense. But why should we do that? The traditional problems for the 'Fido'-Fido theory show that this would be a mistake. Perhaps we should identify meaning with what is said in the opaque sense, or with something else altogether. There is no compelling argument here for overlooking the traditional objections.

2. Suppose that a person starts with the popular slogan: "the meaning of a sentence is its truth conditions." This gets interpreted as follows:

- (5) The meaning of a sentence is a possible state of affairs: an arrangement of objects, attributes, and so on.

The meaning of a singular term is simply its contribution to this meaning. Applied to names, this yields (4). But what *does* a term contribute? The easy answer is, its referent: an object, attribute, or whatever, that partly constitutes the state of affairs. The person rejects this answer as too easy, because some terms are complex, picking out their referent partly in virtue of their structure. This structure and the referents of its parts seem relevant to meaning. Thus the meaning of 'the father of Annette' involves the referent of 'father' and 'Annette', and not simply Harry, who *is* the father of Annette. Nevertheless, the person thinks, the answer is on the right track. It is right for a term that is simple, having no structure:

- (6) The meaning of a simple term is its referent.

The Nondescription theory shows that names are simple. The person has reached the 'Fido'-Fido theory.

The traditional problems for the 'Fido'-Fido theory strongly suggest that there is something wrong with truth-referential semantics of the sort reflected in (5) and (6): meaning is not simply a matter of assigning entities to expressions—for example, states of affairs to sentences, and objects to names.²⁶ Note that this semantics does take *some* account of modes of presentation. *If the term is complex*, then the way in which it presents its referent is important to its meaning. However, if it is simple, only its referent matters. This view could be summed up in a generalization of SP: the meaning of term is either a descriptive structure or else it is the term's referent. The problems suggest, in my view, that modes of presentation are *always* important to meaning; they are important for simple terms as well as complex. The slogan should be understood not as (5) but as: the meaning of a sentence is a *mode of presenting* a possible state of affairs.²⁷

²⁶See Wagner 1986 for a sustained criticism of this sort of semantics.

²⁷I am dissatisfied with the attempt at making this point in Devitt and Sterelny 1987: 33.

3. Kaplan came tentatively to the 'Fido'-Fido theory via an argument in possible-world semantics (1988a: 590-1). Kaplan developed this semantics for indexicals and demonstratives. It yields only three candidates for the meaning of an expression: its referent, its "content," and its "character." Applying this to names,

- (7) The meaning of a name is either its referent, its content, or its character.

In Kaplan's semantics the Rigid Designation theory and the Nondescription theory yield,

- (8) The content of a name is its referent.

Kaplan argues further that

- (9) The character of a name is its content.

The 'Fido'-Fido theory follows. "Because of the collapse of character, content, and referent, it is not unnatural to say of proper names that they have no meaning other than their referent" (1988a: 591). This argument is the slow route from Rigid Designation and Nondescription to 'Fido'-Fido mentioned earlier (section 1).

Before assessing this argument, it is worth mentioning a fast, and very dirty, route from possible-worlds semantics to 'Fido'-Fido. The route establishes (8) one way or another, and then simply identifies content with meaning. The problem with this is that "content" is a technical term in the semantics. We need an argument to show that content, *understood in that way*, is appropriately identified with meaning. To suppose that no further argument is needed is to treat the identification as a matter of definition. This would make the 'Fido'-Fido theory trivial.

What is Kaplan's "content"? Consider a sentence in a particular context of utterance. Its content, or the proposition it expresses, is the aspect of it that is evaluated for truth in each possible world. It can be represented by a function from possible worlds to truth values. The content of a singular term in the sentence is its contribution to what is evaluated; it is the aspect of the term that determines its reference in each possible world. It can be represented by a function from possible worlds to objects. According to the Rigid Designation theory, a name refers to the same object in every possible world. So we can represent its content by a *constant* function; it is "fixed" or "stable." A descriptive name *could be* rigid in this way, as Plantinga showed, but the Nondescription theory rules this out. In these circumstances, rather than represent the name's content as a constant function with the actual referent as its value, Kaplan prefers to follow Russell in thinking of the name's content as the referent itself. Hence (8). The content of the sentence containing the name is then a "singular" proposition (pp. 529-31).

What is Kaplan's "character"? The character of an expression "is that which determines the content in varying contexts" of utterance (p. 534). "Indexicals have a *context-sensitive* character" (p. 535). Thus the character of 'he' determines different contents, because it determines different referents, as we vary the context of utterance. "Nonindexicals have a *fixed* character" (p. 535). Thus the character

of 'cat' determines the same content, cathood, in all contexts. Kaplan goes on to identify the fixed character of a word with its content (p. 536). All that remains to reach (9) is an argument that names have fixed characters (pp. 587–91).

Kaplan's notions of content and character yield an elegant and plausible semantics for indexicals. And there can be no objection to his applying the notions more widely to yield (8) and (9). The controversial move is (7). The traditional objections to 'Fido'-Fido show that the referent of a name is not a plausible candidate for its meaning. Since the content and character of a name, according to Kaplan's semantics, are identified with the referent, they are not plausible candidates either. We need an argument for (7) that Kaplan does not provide. (7) simply "falls out" of Kaplan's semantics. But that semantics was designed for another purpose. It remains to be argued that it is appropriate for names.²⁸

One way of modifying Kaplan's semantics so that the 'Fido'-Fido theory can be dropped would be to broaden the notion of character to cover nondescriptive modes of presentation. Thus, even when the character is fixed, determining the same content in all contexts, it is identified not with the content but with the mode of presenting that content. Character in this broad sense could then be identified with meaning. Another way²⁹ would be to return to the picture that Kaplan discards: the content of a name is not the referent but a function which always has the referent as value. This function is a mode of presentation and can be identified with meaning.

4. What most motivates the direct-reference philosophers to accept the 'Fido'-Fido theory is that they think that there is no viable alternative. The Nondescription theory has ruled out a descriptive meaning for a name. SP is assumed, and so the name's meaning must be its bearer.

I have proposed that a name has a nondescriptive sense, or mode of presentation, which is identified with the type of causal chain that determines the name's referent. What response do the direct-reference philosophers have to this sort of solution to the problems for the 'Fido'-Fido theory?

(i) Wettstein constructs a view along these lines, which he claims to be based on ideas in Perry and Kaplan. He objects to the view because it has the consequence

that everyone who uses 'Aristotle' to refer to the ancient Greek philosopher must be thinking of him as "the individual who stands in the appropriate historical relation. . . ." Most competent users of that name have never heard of the Donnellan-Kripke account of names and do not think of the referents of names in such terms. (1986: 194)³⁰

The objection is beside the point because the view does not have this consequence. The objection assumes that competent speakers must have propositional knowledge

²⁸Perhaps Kaplan would agree, for his approach to names is tentative. I think that the semantics, as it stands, is also inappropriate for 'cat'.

²⁹Which I owe to Georges Rey.

³⁰Searle makes a similar point in defending the conclusions he draws from his Chinese-room example (1980:452).

of linguistic rules. This assumption is popular, but nonetheless false. Briefly, a competent speaker's behavior is *governed by* linguistic rules without her being (mostly) *aware of* those rules, just as a pocket calculator's behavior is governed by arithmetic rules without its being aware of those rules. I have argued against the propositional view of competence at length elsewhere³¹ and will say no more here.

(ii) Another objection to the proposal runs as follows. "The causal chains that constitute a name's meaning, according to the proposal, are external to the mind. So how could differences in such meanings possibly explain the differing cognitive values of ' $a = a$ ' and ' $a = b$ '?"³² It is indeed common for people to think that the Causal theory puts meanings entirely outside the head.³³ But this is simply false. A great deal of the network of d-chains for a name consists in mental processing and functioning. Indeed, it is absurd to think otherwise. For subjects to think about an object as a result of its causal action on them, they must first be appropriately stimulated, and second *must process the results of that stimulus appropriately*. The role of the mind in meaning will be discussed more below.

(iii) Almog rejects something close to my proposal in the context of discussing the problem of ambiguous names (1984: 483–4).³⁴ His rejection is based solely on his argument that historical chains have, in general, the "presemantic" role of preserving the meanings of words. Thus, he claims that we mean by 'you' what we do because our ancestors meant that and there is a causal chain from them to us. In the case of a name, what is preserved by the chains is reference, because its meaning is its referent (pp. 479–82). The chains solve the ambiguity problem by determining which meaning, hence which referent, a particular token has.

Aside from the 'Fido'-Fido theory, I agree with these claims.³⁵ Indeed, as Almog says, they are "compatible with a Fregean semantics" (p. 486) and "relatively uncontroversial" (p. 487). I claim that the chains *also* explain what it is for a name to mean what it does. They answer the question: In virtue of what do we, our ancestors included, refer to Aristotle by 'Aristotle'? Almog is uninterested in explaining reference. His story of the preservation of meaning takes the meaning preserved for granted.

That the chains feature in some uncontroversial explanations is not, of course, any reason for thinking that they should not feature also in some other, possibly controversial, explanations.

³¹1981a: 95–110; 1983: 674–5; 1984a: 206–11; Devitt and Sterelny 1987: 146–8; in press.

³²Some remarks of Evans suggest this objection (1982: 83). I respond to the remarks in my critical notice (1985: 221–3). I have often heard objections along these lines.

³³See, e.g., Block 1986: 665. Wettstein says that "reference . . . has little to do with the head of the speaker" (1988: 415).

³⁴Almog says that an unpublished lecture of Kaplan's in 1971 anticipated the major ideas of his paper (p. 489*n*). See also Kaplan 1988a: 587–92.

³⁵I agree also with Almog's criticisms of Fregean attempts to absorb the Causal theory (pp. 484–6). Searle has recently provided a complicated example (1983: ch. 9) which I have criticized (in press a: sec. 5).

(iv) Salmon makes the most striking response to my proposal. He describes it as “ill conceived if not downright desperate . . . widely bizarre . . . a confusion, on the order of a category mistake” (1986: 70–1). He says almost nothing in support of this. David Lewis has remarked that an incredulous stare is hard to argue with (1973: 86). So is an incredulous Salmon. However, I hope that what follows is an argument.

Salmon is also motivated by his criticisms of what he calls “the Generalized Frege Strategy,” which I shall discuss later (section 7).³⁶

4. The Semantic Task

In the sections that follow I shall sharpen the objections to the ‘Fido’-Fido theory (section 5), assess direct-reference strategies for avoiding the problems that generate the objections (section 6), and develop my own solutions to those problems (section 7). All of these discussions depend, to an extent, on a view of what the task is in semantics. So we need to discuss that matter first.

The need is particularly pressing in assessing the strategies for avoiding the problems. For, the main strategies move the problems outside semantics. In considering these export strategies, it is easy to fall into a “merely verbal” disagreement about what we shall *call* “semantics” and “meaning.” Thus suppose we ignore empty names (thus ignoring, of course, one of the major problems). Everyone (involved in this debate)³⁷ will agree that it is semantically significant that ‘Cicero’ refers to Cicero, ‘Reagan’ to Reagan, etc. So if ‘semantics’ is simply *defined* so that everything about a name other than its referent is irrelevant to its meaning or semantics (cf. the fast and dirty argument in section 3), there is no room for substantive disagreement.³⁸ However, defining away one’s problems is clearly too easy an approach to intellectual life. And it has the disadvantage of making the ‘Fido’-Fido theory trivial. What the theory needs to avoid this triviality is a justification for export strategies that is based on an independent view of the semantic task. So far as I know, a plausible justification of this sort has not been offered. Indeed, the discussion proceeds with little attention to what semantics is *for*. The view I shall now sketch³⁹ will be the basis for rejecting the export strategies.

³⁶Fodor’s motive for adopting his denotational semantics is completely different from any of the above (1987). He thinks, rightly in my view, that meaning holism threatens Life As We Know It. He thinks, wrongly in my view, that functional-role semantics leads inevitably to meaning holism. He sees his semantics as the only possible savior.

³⁷The qualification is necessary because there are many, most notably those in the French structuralist tradition, who think that reference is irrelevant to meaning. On this see Devitt and Sterelny 1987: ch. 13.

³⁸An example of such an approach—not taken by the direct-reference philosophers—would be to appeal to an old division of the study of language according to which syntax deals with expressions alone, semantics deals with expressions together with their referents, and pragmatics with the users of expressions and possible contexts of use. Cf. note 59.

³⁹For more details, and some argument, see Devitt and Sterelny 1987.

Philosophers do not approach semantics with virgin minds. They already think about language using the familiar notions of folk theory: *meaning*, *truth*, *reference*, and so on. Furthermore, they have been educated to use many further notions: *sense*, *proposition*, *possible world*, and so on. In thinking about the task in semantics, it is important to set aside as much of this rich theoretical machinery as possible. Otherwise we are in danger of feeding into the description of the task much that is part of an attempted solution and should perhaps be controversial.⁴⁰ We need to get back to basics. What are the phenomena that prompted all this theorizing in the first place?

The phenomena are certain sounds and inscriptions which play strikingly important roles in our lives: people produce them in many circumstances and respond to them in a variety of ways. These token linguistic symbols are not abstract entities: they are datable, placeable parts of the physical world. The initial, and fairly theory-neutral, view of the task is: to describe and explain the properties of linguistic symbols that enable them to play the roles we have indicated.

Early on in our theorizing about linguistic symbols, we are likely to start talking about the mind. Indeed, I think that it is obvious that the role of language in people's lives comes from its relation to their minds.⁴¹ I think that we should follow the folk in our theory by ascribing thoughts to people and in seeing linguistic symbols as the expressions of thoughts. And it is because language expresses thought that people are interested in it. Indeed, it is because it expresses thought that it exists at all.

Why then are we interested in thoughts? I think there are two reasons. First, because we are interested in explaining the behavior of the thinker. Second, because thoughts convey information about the way the world is.⁴² So people produce and respond to, say, 'It is raining', partly because of what it shows about the likely behavior of the speaker and partly because of what it shows about the weather. We have arrived at a much less vague, but more theory-laden way, of specifying the roles of linguistic symbols in our lives.

Because of the link between thought and language, we should expect to follow the folk in ascribing many of the same properties to thoughts as to linguistic symbols. I agree with the many philosophers⁴³ who think that we should ascribe a syntax to thought, thus subscribing to a "language-of-thought" hypothesis. So we can now give a broader, though more theory-laden, description of our semantic task: to describe and explain the properties of linguistic

⁴⁰ I doubt the need for abstract entities in semantics, particularly propositions; 1976:404-5, 414-6; 1984a: 385-6.

⁴¹ In emphasizing the relevance of the mind to linguistics, I am emphatically not endorsing the view, found for example in Chomsky and Dummett, that the theory of language is a theory of linguistic competence. I have argued against that view elsewhere (1981a: 92-5; Devitt and Sterelny in press). So has Soames (1984a, b).

⁴² This is what indicatives do. Interrogatives show what the thinker would like to discover about the way the world is. Imperatives show how the thinker would like the world to be. For convenience, I shall ignore nonindicatives.

⁴³ Including Chastain 1975: 197.

symbols and thoughts that enable them to play their roles in the explanation of behavior and as guides to reality.

It is convenient to use the term 'meaning' as a generic term for the properties of language and thought that are the concern of semantics. The semantic task is then to explain meaning so understood.

5. Sharpening Objections to the 'Fido'-Fido Theory

Against this background, the traditional objections to the 'Fido'-Fido theory seem very powerful.

The Identity Problem is that ' $a = a$ ' and ' $a = b$ ' have strikingly different roles whether in language or in thought.⁴⁴ People seldom think and seldom produce tokens like the former, and there are few signs of consequences or interest when they do. In contrast, people often think and produce tokens like the latter, and there are lots of signs of consequences and interest when they do.⁴⁵ The reason for these differences is that the two tokens differ both as guides to reality and as explainers of behavior. I suggest that the strong conviction of the folk that these two differ in meaning—I have never met a beginning student who did not think that they do differ—is the response of folk semantics to these facts.

The Opacity Problem is even more severe. It arises once *truth* has been introduced into the theory of language. Consider, for example, ascriptions of belief in the above identities. It is hard to resist the claim that though Ralph's utterance, 'Flora believes that $a = a$ ', is certainly true, his utterance, 'Flora believes that $a = b$ ' may well be false. Yet, according to the 'Fido'-Fido theory if the two names are coreferential the ascriptions have the same meaning and so much have the same truth value; substitutivity of identicals must hold. We shall see that some direct-reference philosophers have managed to convince themselves, against all intuition, that the two ascriptions do have the same truth value (section 6). Yet still the problem does not go away. The two ascriptions differ radically as sources of information about Flora and as explainers of the behavior of Ralph. So they must differ in meaning. Differences in truth values are sufficient for a difference in meaning here, but they are not necessary.

The Opacity Problem makes the Identity Problem worse. If a name's property of referring to a certain object were the only property that was important to its role in identity beliefs and utterances, then it should be the only property of a name relevant to an *ascription* of an identity belief or utterance. If only the referent matters when we are *confronted with* Flora's belief or utterance, then it should be all that matters when we are *informed about* her belief or utterance. But it is not all that matters when we are informed, as the failure of substitutivity shows in the most dramatic way.

⁴⁴Salmon (1986: 12) demonstrates neatly that the problem is not peculiar to identity statements; compare the informativeness of 'Phosphorus is a planet if Phosphorus is' and 'Hesperus is a planet if Phosphorus is'.

⁴⁵This picture is too simple; see section 7.

Similarly, if Flora's mode of representing an object often matters when we are informed about her belief or utterance, as I think it does, then that is good evidence that the mode matters when we are confronted with the belief or utterance; it is good evidence that the mode is a property of the name that enables it to play its specified roles.

Ralph's ascriptions of beliefs and utterances to Flora have the same dual roles as his other utterances; explainers of Ralph's behavior and guides to reality. Set aside the first role. What is significant for this debate about Ralph's ascriptions is that the reality they are an immediate guide to is Flora's beliefs and utterances (which may be about anything from quarks to Quakers). Ralph, like everyone else, is interested in the beliefs and utterances of others as guides to reality and explainers of behavior. So his ascriptions to the likes of Flora show what properties of beliefs and utterances are relevant to those interests. So his ascriptions show what properties go into meaning.

That modes matter to our interest in beliefs and utterances as guides to reality is not initially obvious: we need the traditional problems to bring this home to us. That modes matter to our interest in beliefs and utterances to explain behavior does seem initially obvious.⁴⁶ The behavior flowing from a belief involving 'Phosphorus', given a certain stimulus, may well be very different from that flowing from a belief involving 'Hesperus', given the same stimulus. This is particularly striking if the stimulus is a verbal one involving one of the names. I suspect that the stubbornness with which the 'Fido'-Fido theory is maintained is partly the result of an exclusive interest in semantic properties as guides to reality rather than as explainers of behavior.

Finally, there are the Existence Problem and the Emptiness Problem. Empty names in existence statements and elsewhere undoubtedly have roles in our lives just as nonempty ones do. So a theory must not deem them meaningless.

6. Avoiding Problems for the 'Fido'-Fido Theory

Direct-reference philosophers have three strategies for avoiding the traditional problems for the 'Fido'-Fido theory. The first is the easiest but has the least to recommend it: ignore the problems. I shall call this "the Ostrich Strategy." The other two are of the export sort already mentioned (section 4): move the problems out of semantics. One, "the Mind Strategy," moves them into the theory of the mind. The other, "the Pragmatics Strategy," moves them into pragmatics.

The Ostrich Strategy

The Ostrich Strategy is bad but underlying it is a sound idea. *All* theories have unsolved problems. Yet it is all right to maintain some theories despite this.

⁴⁶See, e.g., Fodor 1980. I think that, for the purpose of explaining behavior, we can abstract from that part of the mode that is outside the skin: only "narrow" meaning matters (in press b). If this is right then, so far as the explanation of behavior is concerned, the referent of a name is not only *not all* of its meaning, it is *not any* of its meaning.

Newton's theory is a striking example: it triumphed for more than two centuries despite many unsolved problems. However, what this shows is not that it is all right to *ignore* problems—the Newtonians did not do so—but rather that in appropriate circumstances it is all right to maintain a theory despite *failing to solve* problems. Briefly, when a theory is justified by an inference to the best explanation, it is all right to be a little bit complacent about it in the face of unsolved problems.

Such complacency about the 'Fido'-Fido theory must rest on the strength of the case for it. I have argued that the case is weak (section 3). This argument rests in part on a rival theory which, I claim, solves the problems and is a better explanation of meaning. That theory will be discussed in more detail in the next section.

It would be unfair to accuse a direct-reference philosopher of following the Ostrich Strategy simply because he has not yet confronted a problem; Rome wasn't built in a day; perhaps he is about to confront it. Nevertheless, it is worth noting that none of the direct-reference philosophers have confronted the Existence Problem or the Emptiness Problem, both of which seem catastrophic for a 'Fido'-Fido theorist who is not prepared to adopt a Meinongian or phenomenalist ontology.⁴⁷

The Mind Strategy

The Mind Strategy exports problems from semantics. The interesting and substantive question in assessing export strategies is whether the very same considerations that make us think that the referent of a name is relevant to its meaning in the first place should also make us think that other factors are relevant. Against the background of my earlier sketch of the semantic task (section 4), I think that the answer, overwhelmingly, is that they should.

The Mind Strategy has been popular for dealing with the Identity Problem. Wettstein has been most explicit.⁴⁸ According to him, Frege formulated

a condition of adequacy for a semantic account of singular terms . . . any such account must provide an answer to a crucial question concerning the cognitive significance of language: the question of how identity sentences in which proper names flank the identity sign can both state truths and be informative. (1986: 185)

Wettstein rejects this "epistemic" condition of adequacy, urging a "radically different conception" of semantics (p. 186). On this conception, cognitive matters are not the concern of semantics.

Wettstein sharply distinguishes between singular propositions and the ways in which those propositions are cognized. He prefers to call those propositions

⁴⁷Salmon expresses the hope that the Pragmatic Strategy will work for these problems (1986: 127–8).

⁴⁸See also Almog 1986: 233–5. Kaplan seems to be tempted; 1988a; 558–69, 591–2. Lycan follows the Mind Strategy in treating the computational roles of thoughts, which are relevant to their role in explaining behavior, as not part of their semantics; 1985.

"states of affairs" because they are made up of objects and properties "out in the world." In contrast, the cognizing of them is a mental activity (pp. 197–9). Semantics should be concerned with the former not the latter; with "the uncovering of the semantic rules that govern our linguistic practices" (p. 200), the "institutionalized conventions" (p. 201), and not "with matters cognitive" (p. 201). In the case of names, we should be concerned to "specify the conditions under which an utterance of a name counts as a reference to an individual" (p. 202) and not with the "ways in which speakers think about their referents" (p. 201), not with their "cognitive perspectives" (p. 202).

(1) I have linked the semantics of language closely to the semantics of thought and given the latter a certain priority (section 4). So if linguistic symbols have such semantic properties as being true or referring, they have them because thoughts do also. On this view there can be no question of divorcing cognitive matters from the semantics of language. Wettstein must reject the view, and he does: he denies that "the first step towards understanding how words refer is to understand how thoughts do so" (1988: 421). What are the alternatives to this view? (a) One might accept that thoughts share those semantic properties with linguistic symbols but deny that this is explanatorily significant. This view seems absurd. (b) More promising, but in my view quite mistaken (Devitt and Sterelny 1987: ch. 10), is the view that the direction of explanation should be reversed: language is prior to thought. (c) One might have a generally eliminativist view of the mind, denying that there are any thoughts or, at least, that there are any with those semantic properties. This faces the problem that all eliminativism faces: explaining behavior without minds. Eliminativism in this case faces an even more difficult problem: explaining language in a thoughtless world *without eliminating reference*. For, if reference goes, so does direct reference. In my view, no eliminativist has offered even sketches of a plausible solution to these problems. Wettstein does not say enough about thoughts⁴⁹ to make it clear which alternative he favors, though his appeals to Wittgenstein suggest some version of (c).

(2) According to Wettstein, the semantic task for names is to describe, perhaps explain, the convention or rule that links a name referentially to an object. But *why* is that the task? Why is the reference of a name interesting and why is only its reference interesting? An answer might be: we are interested in reference because of its contribution to the truth conditions of sentences (cf. 2 in section 3). But why are we interested in the truth conditions of sentences and only in those? Our discussion of the phenomena that need explaining show that properties of symbols *other* than their reference or truth conditions, namely their modes of presentation, are important to their specified roles in our lives.

⁴⁹Wettstein is atypical in saying as much as he does. Direct-reference philosophers typically ignore the semantics of thoughts altogether. For example, Salmon's recent book (1986) does not discuss mental representation or functional- (conceptual-) role semantics, nor does it mention any of the many recent works on this topic (including those by Fodor, thus returning the compliment).

The difference between ' $a = a$ ' and ' $a = b$ ' is not to be set aside as epistemic, cognitive, and unsemantic. We have seen that it is crucial to the role of the two sentences in the explanation of behavior and as guides to reality; it is crucial to what makes us interested in the meanings of the two sentences in the first place.

In the face of this, one can of course insist on applying 'meaning' only to the referential role of a name, but this is an unmotivated verbal manoeuvre that makes the 'Fido'-Fido theory true by definition and uninteresting. What is needed is some account of the point of semantics that justifies the restriction to reference. Wettstein does not provide this.⁵⁰

(3) Wettstein claims that

there is no reason to suppose that, in general, if we successfully uncover the institutionalized conventions governing the references of our terms, we will have captured the ways in which speakers think about their referents. (p. 201)

What is involved in uncovering these conventions? Presumably we have not done this for names when we say simply that a name designates an object. Even if we overlook the fact that this is false for some names—the Emptiness Problem—surely semantics should tell us in virtue of what a name refers to a particular referent. That explanation must frequently involve the mind, for there is nothing other than minds and their relations to the external world that *could* establish the conventions of reference. In my view, the mental facts alluded to in this explanation will explain the required difference in cognitive values (section 7).

Wettstein wants to divorce the mind from semantics partly because of his view of the role it would play if it were not divorced. On his view, the meaning of a name would involve the descriptions or concepts that a competent user of the name must associate with it. This association amounts to propositional knowledge of the referent sufficient to discriminate the referent from other objects (1986:201–4; 1988). But this is to assume that only the Description theory can provide a "cognitive fix" on the referent. In my view, the Causal theory provides that fix: the fix is a network of d-chains generated by conceptual-role links from thoughts to peripheral stimuli and by links from stimuli to the external world. Little if any of this need be conscious knowledge.

The Pragmatic Strategy

The Pragmatic Strategy is another export strategy. It cannot be dismissed out of hand because there clearly are some linguistic phenomena that are rightly treated as matters for pragmatics not semantics; Gricean conversational implicatures are examples. I suspect that there is not, in general, a theory-neutral way of drawing the line between semantics and pragmatics (1981a: 197–8).

⁵⁰Nor does Almog, who seems to apply the Mind Strategy to the Opacity Problem as well as the Identity Problem. Certainly, he thinks that failures of substitutivity in "epistemic contexts" are of no concern to the 'Fido'-Fido theory (1985).

The Pragmatic Strategy has been popular for dealing with the Opacity Problem. Salmon has used it also for the Identity Problem (1986: 77–9).⁵¹

Salmon's line on the latter is that once we have made the distinction between information that is semantically encoded and information that is pragmatically imparted, it is not obvious that the informativeness of ' $a = b$ ' does not come from what is pragmatically imparted. In the absence of a reason to believe the contrary, Salmon feels justified in putting the Identity Problem aside.

I have argued that when we consider why we ascribe meaning at all—to explain behavior and as a guide to reality—it is obvious that the informativeness of ' $a = b$ ' is semantic and not merely pragmatic (section 5). (This is not, of course, to say that the informativeness is not pragmatic at all. Soames demonstrates nicely that it is partly pragmatic; 1988: 104–5.)

Salmon's claims about pragmatics seem to be disastrous for his position that the information semantically encoded by ' $a = b$ ' and by ' $a = a$ ' are equally valuable. He points out that an utterance of S typically pragmatically imparts the information that S is true (1986: 59). Thus the utterance of ' $a = b$ ' imparts the information that that sentence is true and hence, Salmon continues, that the names ' a ' and ' b ' are coreferential (p. 79). To bring out the disaster, first we express Salmon's view of what is imparted by the identity utterances as follows:

- (I1) ' $a = b$ ' is true, hence the referent of ' a ' is the same as the referent of ' b '
- (I2) ' $a = a$ ' is true, hence the referent of ' a ' is the same as the referent of ' a '

Next, consider what Salmon must similarly claim is imparted by the utterances, "Ben is as tall as Saul," and "Ben is as tall as Ben":

- (T1) 'Ben is as tall as Saul' is true, hence the referent of 'Ben' is as tall as the referent of 'Saul'
- (T2) 'Ben is as tall as Ben' is true, hence the referent of 'Ben' is as tall as the referent of 'Ben'

These claims are precisely analogous to (I1) and (I2). What they show first is that, to someone competent with S,⁵² the pragmatically imparted information that S is true *matches in value* the information that S semantically encodes. Indeed, it is *because* S has a certain value that 'S is true' has that value. Further, the value of the pragmatically imparted information about the reference of terms in S reflects the value of the information about the truth of S from which it is derived (with the help of some elementary semantic knowledge), and so is also determined by the value of what S semantically encodes. It is *because* it is interesting to know that Ben is as tall as Saul that it is interesting to know that the referent of 'Ben' is as tall as the referent of 'Saul'. It is *because* it is uninteresting to know that Ben is as tall as Ben that it is uninteresting to know that the referent of 'Ben' is as tall as the

⁵¹ So have Soames (1988: 104–5) and Fodor (1987: 85–6). Salmon lists many others who have taken this path before; 1986: 167n.

⁵² Salmon rightly points out that the Identity Problem is properly posed only of speakers competent with the names in question (p. 60).

referent of 'Ben'. So the additional value of (I1) over (I2) is evidence of precisely what 'Fido'-Fido denies: that ' $a = b$ ' has more information value than ' $a = a$ '.

Salmon takes the Opacity Problem more seriously. He describes it carefully in a way that demonstrates its enormity for the 'Fido'-Fido theory (pp. 80–81, 87–92).⁵³ He notes that we always entertain a singular proposition under some "guise." Part of this guise is our "mode of acquaintance" with the object the thought is about (pp. 107–9).⁵⁴ He acknowledges that these modes are "similar in some respects to Fregean senses" (p. 120). So Salmon has provided the motivation and the means to adopt a view like mine. But his faith in direct reference does not waiver:

The ancient astronomer agrees to the proposition about the planet Venus that it is it when he takes it in the way it is presented to him through the logically valid sentence 'Hesperus is Hesperus', but he does not agree to this same proposition when he takes it in the way it is presented to him through the logically contingent sentence 'Hesperus is Phosphorus'. The fact that he agrees to it at all is, strictly speaking, sufficient for the truth of both the sentence 'The astronomer believes that Hesperus is Hesperus' and the sentence 'The astronomer believes that Hesperus is Phosphorus'. (p. 116)

Whatever she says, *Lois Lane really does know that Clark Kent is Superman* (p. 83)!⁵⁵

If modes are not relevant to the truth conditions of belief attributions, what is their significance? According to Salmon, they have a "pragmatic function" (p. 117),⁵⁶ which makes

the first [way of attributing the belief to the astronomer] better than the second, given our normal purpose in attributing belief. Both sentences state the same fact . . . but the first sentence also manages to convey *how* the astronomer agrees to the proposition. Indeed, the second sentence, though true, is in some sense inappropriate; it is positively misleading. . . . (p. 116)

Salmon accepts that the astronomer has his belief under a certain mode of acquaintance and not under others. He accepts that the best belief attribution conveys which mode the belief is under. He accepts that conveying this is important "given our normal purpose in attributing belief."⁵⁷ *What better evidence could we have that conveying this is part of the meaning of the attribution?* The apparent difference in meaning between the first and second attribution would remain even if they had the same truth value (section 5). However, if conveying information about a mode is part of an attribution's meaning, what better way is there to

⁵³See also Soames 1988: 105–6.

⁵⁴See also Soames 1988: 125.

⁵⁵See Baker 1982 for a demonstration of the rank implausibility of this view.

⁵⁶See also Soames 1987: 67–9; 1988: 104–5, 117–25.

⁵⁷Wettstein inclines toward this view also. He accepts that substitutivity often does fail for these attributions. He finds the subject "difficult and messy" but seems to think that a context-relative account of these attributions will leave the 'Fido'-Fido theory unscathed (1986: 204–9). See below.

explain the apparent difference than to make the modes relevant to truth conditions? This has the happy result that the first attribution is true and the second false, as everyone always thought. Salmon will have none of this:

[I]t is no part of the semantic content of the sentence to specify the way the astronomer takes the proposition when he agrees to it. The 'that'-clause is there only to specify the proposition believed. (p. 117)

Salmon offers so little support for these claims that we should suspect that the 'Fido'-Fido theory is being made true by stipulation and hence trivial.

Salmon is well aware that his view flies in the face of ordinary intuition (pp. 83–5). However, his problems are much more serious than that. He agrees that we are interested in modes. Why are we? For the same reason that we are interested in thoughts and utterances at all: to explain people's behavior and to gain information about the world. *The very same considerations that motivate meaning motivate modes.* And if we are interested in the modes of thoughts and utterances, we should *expect* to find attributions of thoughts and utterances informing us about modes (sections 4 and 5). Salmon's theory is guided neither by folk opinion nor by scientific methodology.

On my view, an opaque attribution with a name in the content clause conveys information about modes via the mode (= sense) of the name: the mode in the thinker or utterer must be the same as the mode in the attribution. For singular terms in general, I see a systematic relationship between the mode of the term in the content clause and the mode of the term that would make the attribution true. (For example, roughly, one demonstrative requires another, though not usually the same one.) This is one way for the semantics of an attribution to convey information about modes.

Another view has found favor.⁵⁸ On this view, attributions have an implicit indexical reference to some feature of the context—for example, the speaker's intention—which determines which mode makes the attribution true. The view gives no special role to the mode of the term in the content clause in determining truth conditions and so is not as strikingly at odds with the 'Fido'-Fido theory as my view. Nevertheless, it is still at odds because it acknowledges the importance of modes to the significance of thoughts and utterances and hence to their meaning.

Soames's approach to the Opacity Problem is similar to Salmon's, as the notes to the above discussion indicate. He has a further argument for treating the problem as nonsemantic.⁵⁹ In the course of a lengthy and subtle discussion of thought and utterance attributions (1985, 1986, 1988), drawing on a paper of Mark

⁵⁸E.g., Schiffer 1979. I criticize the view in 1981c.

⁵⁹In a paper defending Tarski and not on direct reference, Soames proposes a division within the study of language between semantics and pragmatics that is reminiscent of the old one mentioned in note 38 (1984c: 425–6). If it were applied here, it would trivialize the disagreement. On Soames' proposal, a language is an abstract object that has its semantic properties essentially; it is a triple consisting of a set of expressions, a domain of objects, and a function assigning objects to expressions. These abstract objects are the concern of semantics.

(contd.)

Richard (1983), Soames offers some reasons for maintaining substitutivity, hence for denying that the attributions are opaque. I have argued that maintaining substitutivity is insufficient to save the 'Fido'-Fido theory (section 5): the evidence will remain that, for example, Salmon's first and second attribution differ in meaning. Nevertheless substitutivity is certainly necessary to save the theory and so I shall consider Soames' reasons.

First, Soames points to some cases where, despite appearances, substitutivity holds, and claims that we can generalize from these (1987: 66–7). However, his discussion overlooks some observations that Quine made long ago.⁶⁰ Quine noted that though some thought attributions are opaque, some are transparent; substitutivity holds for them. Transparent attributions can be *obviously* transparent; for example, of the form,

b is such that *a* believes it to be *F*.

However, Quine noted further that the most common forms of attribution are ambiguous between an opaque and a transparent reading; for example,

a believes that *b* is *F*

is ambiguous. In the light of these Quinean observations, the response to Soames's argument is simple: substitutivity does indeed hold in his cases because it is appropriate to construe the attributions transparently. But there is no basis here for Soames's generalization that substitutivity *always* holds.

Quine's view that there are both transparent and opaque attributions is not only supported by our intuitive assignments of truth values to attributions, it is also supported by a consideration of the purposes of these attributions. Take Ralph's attribution of a belief to Flora only as a guide to the reality of Flora's beliefs (not as an explainer of *Ralph's* behavior). If our interest in that reality is for the purposes of explaining Flora's behavior, then an opaque attribution will always be appropriate. On the other hand, if our interest in that reality is as a guide to another reality—what the belief is about—then the transparent form will often better suit our purposes.⁶¹

Second, Soames has a range of cases involving identity where, he claims, certain substitutions are irresistible. Yet the cases yield the same counter-intuitive

Pragmatics is concerned with the contingent question of which language a person or population speaks. Soames' proposal would make the 'Fido'-Fido theory trivial once the Nondescription theory had been adopted for, on this conception of a language, there would then be nothing that could be a name's meaning except its role of referring to its assigned object. The traditional problems for 'Fido'-Fido would not disappear, however. They would become problems for this way of dividing up the subject. Why suppose that the empirically interesting question posed by linguistic phenomena is: Which of *these abstract objects* does *x* speak? The problems suggest that *x's* use of a name—part of speaking a language—is *not* adequately explained by relating *x* to a pair consisting of the name and an object—part of the abstract object that is supposed to be the language. If language is to be conceived of as an abstract object, the empirically interesting conception must be richer than Soames's.

⁶⁰ So does Wettstein's (1986: 205–6) and Salmon's (in press: *n* 12).

⁶¹ For more on this see my 1984a: 394–6; 1984b: 99–101.

results as does the general substitutivity that is so problematic for the 'Fido'-Fido theory. If we are prepared to put up with the results in these cases, why not do so in general?

Soames's most interesting cases are of attributions involving demonstratives. The truth conditions of these are undoubtedly tricky. Soames also has some cases involving general terms. I think that these cases, particularly the demonstrative ones, do provide some solace to direct reference, but nowhere near enough to save it in the face of the considerations I have adduced. However, I must set their discussion aside until another time.⁶² I postpone a discussion of Soames's case involving a name until the next section.

7. Solving the Problems

In this section I shall expand on my solutions to the Identity Problem and the Opacity Problem. However, the discussion must still be briefer than the problems deserve. And I shall have nothing more to say about my solutions to the Existence Problem and the Emptiness Problem.⁶³

I have argued that the strategies adopted by direct-reference philosophers to avoid the traditional problems for the 'Fido'-Fido theory are all mistaken. If my solutions to those problems are along the right lines, the strategies are also unnecessary. The problems can be solved in semantics by abandoning 'Fido'-Fido.

The Identity Problem

I shall start with a paradigm example of an informative identity statement, 'Hesperus = Phosphorus' soon after the discovery of its truth.

My solution begins with the claim that the two names in this statement have different senses in that underlying them are different types of d-chain.⁶⁴ Note that the claim concerns types not tokens. If we located the difference between the names in the different token d-chains that make up their networks, we would be driven to the unfortunate conclusion that every name token differs in sense from every other name token.

How do the d-chains differ in type? They involve different types of groundings: in the one case a set of events in the evening, including the sound of 'Hesperus'; in the other, a set of events in the morning, including the sound of 'Phosphorus'. (I talk of sounds, ignoring other media, for convenience.) They involve different types of reference borrowings: in the one case, communications including the sound of 'Hesperus'; in the other case, communications including the sound of 'Phosphorus'. However, these differences cannot be essential.

Consider two look-alikes who are both named 'George'. Clearly the groundings of these names may be indistinguishable aside from the fact that they involve different objects. But now remove that difference: there is one person leading a

⁶²There is little discussion of them in my 1981a and 1984a either.

⁶³But see 1981a: ch. 6.

⁶⁴For a lot more detail on some aspects of this discussion, see my 1981a: 129–57, 239–40.

double life with such success that everyone wrongly thinks that there are two look-alikes. Intuitively, these two uses of 'George', generating two distinct networks, differ in sense as much as 'Hesperus' and 'Phosphorus'. Certainly they can be part of a very informative identity statement: '(This) George = (that) George'. (So in this highly abnormal situation, a statement of the form ' $a = a$ ' is informative.)⁶⁵ So different types of grounding cannot be essential to a difference in sense.

This example shows also that different types of reference borrowings cannot be essential to a difference in sense. Indeed, all reference borrowings involving the sound of 'George' are, in the relevant respect, the same.

What is essential to a difference in sense is that members of the speech community process the input involving the names differently and hence keep the networks distinct; the names are associated with different "files." Evidence of this is that the two names are involved in distinct sets of beliefs.

Suppose that Charles already has the ability to use the sound type 'Gail' to designate a certain object, Gail. So he has a "file" consisting of a set of thoughts that include tokens that dispose him to speak the sound type 'Gail' and that have underlying them d-chains that are grounded in Gail. Suppose now that Charles is in the position to borrow reference from Kate. He hears Kate using tokens of 'Gail' which, in fact, have underlying them d-chains grounded in Gail. If he is to take advantage of Kate's utterance, amending that 'Gail' file, he must process Kate's 'Gail'-sounds so that they are brought to bear on that file. He must process the input as if he had formed the identity belief, 'Gail (the subject of this conversation) = Gail (the subject of these thoughts)'.⁶⁶ This processing task may not be easy because he may know several people named 'Gail'. Similarly, if he is to amend his file in a grounding situation as a result of experience of Gail herself, he must process the input as if he had formed the identity belief, 'That person = Gail'. It is in this way that Charles' thoughts prompted by current experience are unified with pre-existing thoughts involving a name. D-chains are of the same type for Charles if his inner processing links them together in this way.

This is the story of sameness of type *for Charles*; of his *personal* network. It is the story of *speaker* sense. We need also the story of sameness of type period, of the community's network. We need the story of *conventional* sense.

When Charles borrowed from Kate, his processing linked his network to hers. And *everybody's doing it*, not always with Kate, but with other members of the community. The union of all these personal networks is the community's network. For two d-chains to be of the same type, and hence for the names they underlie to have the same (conventional) sense, is for them to be linked together by the inner processing of members of the community into the one network and for them to involve tokens of the sound type conventionally part of the network. (Unions can be imperfect, as we shall briefly see.) D-chains are of different types when they have not been so unified.

⁶⁵Cf. Salmon's nice example of Aristotle; 1986: 75.

⁶⁶In my 1981a, pp. 134-6, I required that the subject actually from the identity belief. I now think that this yields an over-intellectualized account of the processing.

D-chains of different types normally arise from groundings in different objects. Where they do not, they will almost always involve differences in sound type and different circumstances of use; the case of 'Hesperus' and 'Phosphorous' is an example.⁶⁷ Very rarely, differences in d-chain type may arise despite sameness of object and sound type because of different circumstances of use; the case of 'George' above is an example.

The solution to the Identity Problem falls straight out of this. Because 'a' and 'b' differ in sense, 'a = a' and 'a = b' differ in role and in the "cognitive value" that Frege drew attention to. The difference in sense is a difference of d-chain type brought about by differences in the processing of tokens of 'a' and 'b'. These processing differences affect our understanding of the two identity statements: understanding the first involves accessing the one file twice whereas understanding the second involves accessing two files. Because of these facts about inner processing, and a mastery of identity, a person knows that the first statement must be true and that the second may be true or false.⁶⁸

I solve the Identity problem by ascribing different meanings to 'a' and 'b' but I do not "give" the meanings. This solution will be a disappointment to those who seek a traditional *a priori* "analysis." In my view, the semantic task does not require such an analysis. The task is to *explain* meaning not give it. Furthermore, I don't think that the meaning of 'a' can be given in other terms. 'a' gives the meaning of 'a' as well as it can be given.⁶⁹

On my view, what is important about a name token, what enables it to play its specified roles, is that it is part of a certain sort of network that links the token to other tokens of a certain sound type (and of a certain inscription type, etc.). Its meaning (information value, etc.) is its property of being so linked. The referent gets into the picture because, if the name is nonempty, the network will be grounded in an object which is the referent.

We can use this discussion to provide a further argument against the 'Fido'-Fido theory. According to that theory the only thing essential to understanding

⁶⁷Note that in such cases knowledge of the identity does not lead to amalgamation of files. Thus we process some input to our 'Superman' file and not our 'Clark Kent' file, and vice versa. So the names have different modes of presentation; cf. Schiffer 1987, an interesting criticism of Salmon, to which Salmon in press is a reply.

⁶⁸The story for demonstratives is different. If 'a' and 'b' are such terms, 'a = b' is usually informative. However, this is not to be explained by the differing senses of the terms, for example the differing senses of 'she' and 'you', but rather by what is common to the senses of all such terms: those senses make the reference of a token depend on a d-chain grounded by the speaker in producing that very token (1981a: 42-3). The possibility of tokens of different terms having different referents is intrinsic to the senses of the terms. Indeed, the possibility of tokens of the same term having different referents is intrinsic to the term's sense with the result that 'a = a' can be informative.

⁶⁹For more on the place of analysis in semantics, see Devitt and Sterelny 1987, particularly: 231-5. I suspect that the "analytical" view of philosophy lies behind motivation 2 in section 3. Ackerman's view is like mine in that she ascribes different "non-descriptive connotations" to coreferential names (1979a, b). However, she *does* offer analyses of these connotations as a solution to the Identity Problem. I think the solution will not work for similar reasons to those I offered (1983: 676-7) against her similar view of natural-kind term (1980).

a name token is assigning it the right referent. The case of 'George' shows that this is not so. In that case the name seems to have two meanings but only one referent. Understanding a token requires assigning it the right meaning—in my terms, linking it to the same network that underlies its production. A person who gets this wrong will have misunderstood the token, which may have important consequences for explaining behavior and gaining information about the world. Yet the person may still have assigned the right referent. So assigning the right referent is insufficient for understanding. So having that referent is insufficient for meaning.

We have seen that the 'Fido'-Fido theory is open to refutation by finding a pair of names that share a referent and yet yield an informative identity statement. Salmon calls this "Frege's Strategy." Salmon thinks that if this strategy is good so also is "the Generalized Frege Strategy." Consider the theory that the meaning of a name is its *F*. The Generalized Strategy is to find a pair of names that share *F* and yet yield an informative identity statement. Salmon thinks that if the strategy were good, it would work against any plausible theory. So it can't be good. So Frege's Strategy is not good. So the 'Fido'-Fido theory can be retained.⁷⁰ I claim that my theory is not open to the Generalized Frege Strategy: it is not possible to find two names that share my sort of senses and yet yield an informative identity statement, for any such names will have underlying them the one causal network.

In assessing this claim, two distinctions are important: first, that between *conventional* senses and *speaker* senses; second, that between what is informative *in general*, and what is informative *to a particular speaker*. I have been primarily discussing the first half of each distinction. I explain informativeness in general by appealing to the differing conventional senses of the names; for the appeal is to the typing of d-chains in the speech community as a whole. However, some individuals may be partly at odds with the community, with the consequence that a name with one conventional sense has two speaker senses for them. They process tokens of the one name as if they were of two names. Then even a thought of the form '*a* = *a*' would be informative for them, although the names involved share conventional senses. Consider, for example, Ralph, who does not know that Russell the logician is one and the same as Russell the peace marcher; as a result, he does not unify his input from the two sources into one file. 'Russell (the logician) = Russell (the peace marcher)' is informative for him because the names have two speaker senses (though only one conventional sense).⁷¹

⁷⁰Salmon 1986: 73–6. I have adapted the Strategy a little to suit my purposes.

⁷¹If Ralph's situation were general in a community, the case would be like that of 'George': one referent, one sound type, and yet two conventional senses.

Ralph might make a more serious mistake: treating tokens from two names as if they were from one. The earlier mistake is failing to unify where he should; this one is unifying where he should not. This mistake leads to a network grounded in two objects and thus to indeterminacy of speaker reference. I discuss this in 1981a: 138–52.

The Russell case is of the sort that generates Kripke's "Puzzle about Belief" (1979). I have urged a solution making use of the distinction between conventional and speaker senses 1984a: 407–12). I would offer a similar solution to Salmon's case of Elmer (1986: 92–101).

The Opacity Problem

Sometimes substitutivity holds for ascriptions of thought and utterance and so they are transparent. In such cases, the ascriptions pose no *additional* problem for a theory of names. The additional problem comes from the many cases where substitutivity fails and the ascription is opaque: something other than the name's referent is relevant to the truth of the ascription.

The key to my solution is simple: the truth conditions of the ascription depends on the sense of the name in the content clause.⁷² The ascription requires that the subject's thought or utterance uses a name with the same sense as the name in the content clause. This talk of senses is to be understood in the causal way set out above. The solution captures the intuitive idea that the ascription requires the subject to use the same name as the ascriber.

This solution demands that we say more about what counts as sameness of sense, and hence sameness of name.

My convenient restriction to the spoken language encourages a simple view: tokens of the same name must sound the same. So the ascription of an utterance to Flora could not be true unless the name she used sounded the same as the one in the content clause of the ascription. Immediately we remove the convenient restriction, we see that this cannot be true. Flora might have been writing, not talking. Indeed, the ascription might be made true by an utterance in any medium at all. Furthermore, what if the ascription is of a thought not a communication? It can be made true by a mental token.

The name in the content clause of an ascription requires that Flora use a token in the same network but not a token in the same medium. However, if Flora's token is not mental, being in the same network is not alone sufficient for the truth of the ascription, because Flora's token might not be in accord with the conventions that have formed the network; it might be aberrant. The truth behind the simple view is that if Flora's token is a sound, it must be of the same sound type as those in the network; if it is an inscription, it must be of the same inscription type; and so on through all the media of communication.

I have written as if tokens of what is intuitively only one sound type, inscription type, and so on, are conventionally part of a network. This may require modification. Consider Soames's example: the names 'Ruth Barcan' and 'Ruth Marcus' (1987: 67). Suppose that the ascriber used a token that sounded like the former, whereas Flora's utterance contained a token that sounded like the latter. In assessing this ascription, it may seem as if these tokens have the same sense. Yet, intuitively, they are of different sound types, at least partly. Let us look at the history. For many years, there were sounds of the type 'Ruth Barcan', but none of the type 'Ruth Marcus', in a network grounded in Ruth Barcan Marcus. When there came to be sounds of the latter type referring to her, were they conventionally in the *same* network? To some degree, probably yes: some people probably treated those sounds as if they were tokens of the

⁷²For many more details, see my 1981a, chs. 9 and 10, and 1984a.

old name; their processing brought these tokens into the old network. Others almost certainly did not. Doubtless, the same practices go on to this day. So sounds of the two types are partly unified into the one community-wide network and partly not. Unification is always a matter of degree and in this case the degree is well short of what is required for a confident claim that Flora's token and the ascriber's are of the same name type.

So according to the theory, there is some vagueness about sameness of sense. I don't think that this matters to the theory because there is a matching vagueness about the circumstances in which we ordinarily think ascriptions true.

Foreign names pose a similar problem. Tokens of 'London' and 'Londres' normally count as the same for the purposes of thought ascription. Intuitively, they are of different sound types, inscription types, and so on, though not so very different. At first sight, it may seem as if the networks underlying the two types are not unified at all: one network involves English speakers, the other, French speakers. This is a mistake. It overlooks the many situations where an English speaker processes a French speaker's 'Londres' to a 'London' file, and vice versa. If a foreign name has not been unified with an English name in this way, then I think that we would not count an English ascription of a thought to a foreigner true on the strength of a thought involving the foreign name. At least we would not count it true *construed opaquely*. Of course, it might well be true construed transparently.

Soames's example enables us to construct a case of the sort that he likes, discussion of which I postponed (section 6):

- (a) Flora believes that Ruth Barcan = Ruth Barcan.
- (b) Flora believes that Ruth Barcan = Ruth Marcus.

Soames thinks that the substitution of 'Ruth Marcus' for 'Ruth Barcan' is irresistible: they must have the same meaning. Yet, he claims, (b) still seems much more informative than (a). If everyone has to tolerate this counter-intuitive result, why not tolerate the similarly counter-intuitive results of general substitutivity, thus allowing the 'Fido'-Fido theory to stand?

I bite the bullet here. I think that the substitution is quite resistible. Tokens of 'Ruth Marcus' and 'Ruth Barcan' are not thoroughly unified in the one network. The tendency to favor substitution arises from the partial unification of the networks for the names. Identity of sense is a matter of degree.

8. Conclusions

The theory of direct reference for names is a combination of three distinct but related theories: the 'Fido'-Fido theory, the Nondescription theory, and the Rigid Designation theory. It does not include the Causal theory. I have argued that the 'Fido'-Fido theory does not follow from the three other named theories, lacks adequate motivation, and is false.

The argument against it is a traditional one: it fails to solve the Identity Problem, the Existence Problem, the Emptiness Problem, and the Opacity Problem. Direct-reference philosophers typically try to avoid this argument by exporting the problems into the theory of the mind or pragmatics. A consideration of our purposes in attributing semantic properties to linguistic symbols—explaining behavior and learning about reality—shows that these export strategies fail. The very same considerations that make us think that the reference of a name is semantically significant, make us think that *more than* reference is significant.

A major reason for thinking that the 'Fido'-Fido theory lacks motivation is that there is an alternative. This alternative ascribes to a name a nondescriptive sense, or mode of presentation, that is explained in terms of the reference-determining causal network for the name. Thus, I argue that the Causal theory not only explains a name's reference, it also points to an explanation of the name's meaning that solves the traditional problems for the 'Fido'-Fido theory. Contrary to popular opinion, the Causal theory supplies not an example of the 'Fido'-Fido theory but a way of replacing it.

In my argument, I take the 'Fido'-Fido theory to be a substantive addition to the Nondescription theory. That is, I take its notion of meaning to be the one we need in an empirical theory attempting to explain linguistic phenomena. However, the discussion often proceeds as if 'Fido'-Fido follows from Nondescription by stipulation: nondescriptive meaning is identified with reference *by definition*. This is not a theoretically useful notion of meaning. The move has the further disadvantage of making 'Fido'-Fido the result of a trivial addition to Nondescription.

Why is the 'Fido'-Fido theory maintained in the face of apparently overwhelming objections? Partly, I think, because far too little attention is paid to the question of what we need semantics *for*. As a result, standard views of the nature of semantics, and of the range of alternative theories, are taken for granted. In my opinion, these standard views are mostly unhelpful, if not mistaken.^{73,74}

⁷³Shortly after this paper was sent to the publishers, David Kaplan sent me a copy of his "Afterthoughts" (1988b), in which proper names are discussed at some length. He still favors the 'Fido'-Fido theory (draft pp. 13–9). However, in contemplating a change in his view of a name's character (similar to those I proposed above on his behalf; sec. 3, motivation 3), he seems prepared to bring a name's mechanism of reference into its meaning (draft pp. 30–3). I am in broad agreement with many of his other remarks about names (draft pp. 58–76): on the relation of thought to language (1981a: 83–6; Devitt and Sterelny 1987: 124–8); on being *en rapport* (1981a: ch. 9; 1984a); on naming future objects (1974: 199–200; 1981a: 59–60).

⁷⁴Earlier versions of this paper were given at the University of Sydney (July, 1988), La Trobe University (July, 1988), and Princeton University (October, 1988). The paper has benefited from the discussions it received on those occasions. I am very grateful to the following for helpful comments on a late draft at very short notice: Fiona Cowie, Norbert Hornstein, Bill Lycan, and Georges Rey. Finally, I must thank Nathan Salmon and Scott Soames for comments that removed some misunderstandings and led to other improvements.

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On Sense and Direct Reference

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