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Reference and indexicality

Ostertag, Gary John, Ph.D.

City University of New York, 1994

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REFERENCE AND INDEXICALITY

by

GARY JOHN OSTERTAG

A dissertation submitted to the Graduate Faculty in Philosophy
in partial fulfillment of the requirements for the degree of
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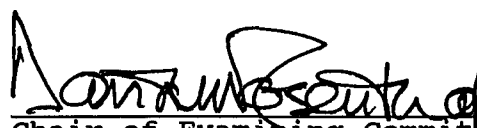
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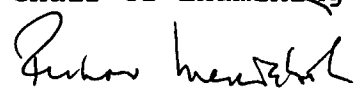
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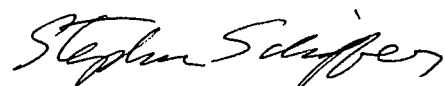
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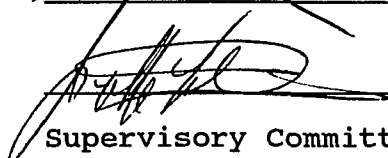
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Abstract

REFERENCE AND INDEXICALITY

by

Gary Ostertag

Adviser: Professor Stephen Schiffer

The dissertation attempts to provide a treatment of belief reports and definite descriptions consistent with a directly referential semantic theory. By the latter I mean (at very least) a theory according to which that-clauses are singular terms that have as their referents structured propositions. Part I defends the claim that belief reports, sentences of the form 'A believes that S', make explicit reference to a proposition (*that S*) and implicit, context-sensitive reference to the manner in which the subject represents the ascribed proposition. A problem is developed for this view, namely, that the context cannot plausibly be held to guarantee a unique (implicit) reference to the subject's internal representation. Four strategies of response are developed. Part II proposes, but ultimately rejects, a treatment of definite descriptions according to which incomplete definite descriptions are completed by contextually salient descriptions--that they make implicit,

context-sensitive reference to such completions. Instead, a directly referential interpretation of definition descriptions is defended. It is argued that the assumption of the referential theory is compatible with the Russellian use of descriptions, but that the alternative assumption of the Russellian view is not compatible with certain referential uses of definite descriptions.

ACKNOWLEDGEMENTS.

There are many people to thank for their interest, advice and time, but I hope it will be forgiven if I confine myself to those whose influence on this dissertation has been most immediate. First, I would like to thank my advisor, Stephen Schiffer, for his guidance and for providing an example of how to do philosophy. My indebtedness to his writings and lectures will be obvious to anyone who reads what follows, but my occasional citations do not begin to reflect the number of suggestions and comments that I have incorporated. Russell Dale's painstaking comments on various portions of the dissertation were invaluable to me, as were his enthusiasm and encouragement. I also want to thank Eileen O'Neill, for listening to what follows and for kindly taking the time to show me how most of the problems raised can be traced to my ignorance of traditional grammar. Finally, I want to dedicate this dissertation to my parents, Bernhard and Rosemarie Ostertag. Without their encouragement and generosity none of this would have been possible.

PREFACE.

1. The idea that sentences of natural languages have, in Bertrand Russell's words, the "humble property" of being interpretable, given knowledge solely of their constituent expressions and their syntax, is intuitively plausible. Yet the possibility of a meaning theory that explains the apparent compositionality of meaning is threatened by the fact that belief reports, sentences of the form 'A believes that S,' have so far proven recalcitrant to semantic analysis. There has been a good deal of progress in the semantical analysis of a large fragment of English (for example, the study of contexts containing anaphora, adverbs, mass terms, non-logical quantifiers, etc.). Yet a partial picture is philosophically unsatisfying; we must be able to explain this humble property wherever it is exemplified and not just in scattered contexts. Thus, developing a treatment of belief reports consistent with the assumption of compositionality is urgent. It explains something quite fascinating about language and about thought. In addition, without an acceptable treatment, semantical analyses of other fragments seem philosophically idle, since such

analyses will not be expected to fill in part of a complete (or reasonably complete) picture.

These considerations, then, motivate the following question, which I will address in Chapter One:

What is the proper treatment of belief reports relative to the assumption that natural languages possess a compositional meaning theory?

2. Definite descriptions are used in significantly different ways. In some cases, it seems natural to classify them as demonstrative pronouns--expressions that are commonly thought to be devices of direct reference. For example, an utterance of 'The woman was abrupt' seems to carry the same content as its demonstrative counterpart, 'She was abrupt' (at least when the intended referent is an adult). Alternatively, descriptions can be used to make general assertions, making no reference to particulars. For example, 'The being such that none greater can be conceived created the eternal truths' expresses a proposition even though the constituent description may fail of reference. This suggests that descriptions are ambiguous between referential and non-referential interpretations. But the immediate problem with such a suggestion is that it violates Gricean principles of semantical economy. For it seems clear that assuming that descriptions are univocal is not incompatible with facts about their usage. That is, assuming a Russellian interpretation of descriptions allows

for an explanation of the alternative pattern of usage. Thus, the ambiguity hypothesis is decidedly unappealing. Yet this makes the question of the correct account of descriptions all the more pressing. For definite descriptions are often used in a manner which a non-referential treatment cannot accommodate.

This motivates the following question, which I will address in Chapter Two:

What is the proper analysis of contexts containing definite descriptions? In particular, are definite descriptions quantifiers--as the Russellian account seems to suggest--or are they ambiguous between a quantificational and a referential interpretation?

3. Yet what reason is there to think that these questions are related, beyond their being questions as to how best to interpret certain lexical items--i.e., verbs of propositional attitude and certain uses of the definite article--relative to certain assumptions? The answer to this is straightforward. The particular theory of the semantics of belief reports that I defend in Chapter One faces a problem that is structurally similar, if not identical, to a problem faced by Russell's theory of descriptions. This is the problem of *incomplete* definite descriptions. Let me briefly indicate the problem(s) in question, before returning to the relationship between the two questions.

The theory of belief reports that I defend is the hidden-indexical theory. One version of this theory maintains that sentences of the form:

[1] Sam believes that Merlin is a tabby

make implicit, context-determinate reference to a way in which Sam represents the proposition *that Merlin is a tabby* to himself. This is the view, for example, of Crimmins and Perry (1989). The version I advocate (following Schiffer (1992)) is that [1] makes implicit reference to a *type* of psychological representation. Again, according to the former view, [1] makes reference to a particular representational state, a state which has as its content the ascribed proposition. On the latter view, [1] simply "constrains" the representational state. For example, it might indicate that Sam believes the proposition in a manner that represents Merlin as a Persian and not, say, as some other breed of cat. This implies that if Sam believes the proposition that Merlin is a tabby but that he fails to represent Merlin in the manner implied--he represents Merlin as a shorthair, or as a robot cleverly designed to pass for a feline--the proposition expressed will be false.

A problem for the latter view is that a context will typically generate indefinitely many candidate mode-of-presentation types. That is, an actual utterance of [1] typically will raise to salience an indefinite number of constraints on how Sam believes that Merlin is a tabby.

This precludes the possibility of the utterance's making definite reference to such a constraint.

Consider a Russellian theory of definite descriptions of the following sort. The theory claims that a sentence of the form 'the F is G' is to be analyzed as saying that exactly one Φ is G, and that all Φ 's are G's (where Φ is either the property expressed by 'F' or, in case the description fails to completely specify its denotation, a contextually definite completion of 'the F'). The theory is therefore not strictly Russellian, since it does not claim that an utterance of 'the F is G' must be used to assert the Russellian expansion of that particular sentence-form. Rather, the theory claims that the utterance makes reference to a contextually salient completing description. It is the expansion of the resulting "complete" sentence that determines the content of the utterance. The theory is thus similar to the hidden-indexical theory of belief reports. Yet an immediate objection to this version of Russell's theory is that such incomplete utterances typically will manifest an indefinite number of completing descriptions. For example, an utterance of:

[2] The book is on the table

will generate indefinitely many eligible completions--for example, *the book that came in this morning's mail*, *the book underneath your coffee mug*, etc. This precludes the

possibility of the speaker's intending a definite completion in uttering [2].

4. It has struck me that a parallel treatment of these two problems would be of some interest. This seems especially so given Stephen Neale's (1990) recent rehabilitation of Russell's theory of descriptions. Moreover, I have felt that extending the strategy I developed to defend the hidden-indexical theory to the theory of descriptions would provide the Russellian with a much-needed response to the problem of incompleteness. (Neale's treatment of this problem is rather weak.) Yet in developing my defense of Russell's theory, I have become increasingly skeptical about the Gricean arguments in favor of a Russellian account. Such arguments are to be found in Grice (1969), Kripke (1977) and Neale. So far as I have been able to see, the considerations raised in these discussions succeed only in motivating an account according to which descriptions are univocal; what they fail to do is point to any one of the competing accounts as uniquely correct. To be sure, a referential treatment of certain descriptive contexts appears unintuitive, but the same is true for the Russellian treatment of those contexts in which descriptions seem to function as demonstrative pronouns. And in each case, one has the option of explaining away the contravening intuitions by an appeal to speaker's meaning. In any event,

it has seemed to me that investigating a referential interpretation of definite descriptions would be important.

This suggests the following question, investigated in Chapter Three:

What is the proper analysis of contexts containing definite descriptions? In particular, are definite descriptions best analyzed as quantifiers, or as directly referential singular terms?

As it turns out, it seems fair to say that the referential thesis, while by no means secured as the correct treatment of definite descriptions, is at least as attractive as the Russellian alternative. This is not to say that the matter is settled, for utterances containing definite descriptions seem to be meaningful even in those cases where the description fails to refer. And, while a pragmatic explanation of this phenomenon is available, it may strike some as unsatisfying. Nonetheless, the theory fits neatly into a compositional semantics. Moreover, as I point out in Chapter Three, the behavior of descriptions as pronouns is entirely natural on the assumption of the referential hypothesis. Quite the opposite is the case if we make the alternative assumption that definite descriptions are quantifiers.

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In the above-mentioned work, on which I was then engaged, I made opportune and inopportune reference to the propositions of that book, not in order to refute them--what have I to do with refutations!--but ... to replace the improbable with the more probable, possibly one error with another.

--Nietzsche, *On the Genealogy of Morals*

CHAPTER ONE

Context and Constraint: Evaluating the Hidden-Indexical Theory

Introduction.

Recent proposals concerning the semantics of belief discourse have emphasized the context-sensitivity of belief reporting. But the purpose of the emphasis is not always the same. Direct reference theorists, notably Salmon (1986) and Soames (1987b), urge that, while our concerns in reporting beliefs are more complex than simply conveying that a relation between an individual and a proposition of a certain sort obtains, the truth of such a report hinges solely on whether or not the individual referred to bears the appropriate relation to the proposition referred to. This picture, to be sure, has unintuitive consequences, but these, it is said, can be explained away as having to do with pragmatic impropriety and not with truth or meaning.

Another theory, developed by Schiffer (1977), (1987a), (1992), Crimmins and Perry (1989), Fodor (1989), and Crimmins (1992), attempts to incorporate these discarded pragmatic elements into the actual propositional content of token belief reports. (I will call this view, following

Schiffer, the hidden-indexical theory, or 'HI'.) What both views have in common is their subscribing to the principle that propositions are to be analyzed as ordered pairs of objects, or object sequences, and attributes. But whereas the direct reference theorist maintains that *believes* expresses a binary relation between a thinker and such a proposition, the HI theorist argues that belief is a three-place relation holding among a thinker, a proposition and a mode of presentation. The latter view captures speakers' intuitions about the truth or falsity of belief reports, rather than simply relegating these intuitions to pragmatics. This is accomplished by locating, following Crimmins and Perry, an *unarticulated* constituent in those propositions expressed by token belief reports. This constituent places conditions on how the subject of the belief report presents the ascribed proposition to herself. Thus when Jimmy Olsen asserts:

[1a] Lois believes that Clark Kent writes for the Daily Planet,

we take him to be putting a condition on how Lois presents *that Clark Kent writes for the Daily Planet* to herself that would not have been present were he to have assertively uttered:

[1b] Lois believes that Superman writes for the Daily Planet.

This is the case even though the respective that-clauses refer to one and the same proposition.

The direct reference theory ('DR' hereafter) avoids such subtleties. According to DR, [1a] and [1b] express the same proposition. Thus [2] could never be truly asserted, as it expresses a contradiction:

[2] Lois believes that Clark Kent writes for the Daily Planet, yet she doesn't believe that Superman writes for the Daily Planet.

Of course, *prima facie*, [2] states a 'fact' about the propositional attitudes of a fictional character, one which, among other things, proves useful for explaining her behavior within the fictional context. But the direct reference theorist has a response. He acknowledges that facts about how a thinker represents a proposition to herself are often central to the concerns of the belief reporter and his audience; that people often produce belief reports to convey information not simply concerning which proposition a given individual believes, but also about how that proposition is represented to that person. Yet the direct reference theorist denies that this latter information is part of the information content of the utterance in question. I will, in the following section, consider this response in some detail, but only after first evaluating a neo-Fregean response, according to which [2] is true (assuming for the moment the literal truth of the fictional example).¹ After evaluating DR I will sketch a deflationary proposal, but ultimately reject it for reasons similar to my rejection of DR. Finally I will consider HI,

arguing that it is superior to the other theories considered. Part 2 develops the theory in full.

The remainder of the paper is given over to a defense of HI against various criticisms. I will consider three responses to these criticisms, one of which ends up defending a theory distinct from HI, but similar in that it acknowledges the contribution that context makes to the propositional content of belief report tokens.

1. Four Theories of Belief Ascription.

1.1 Neo-Fregeanism.

The neo-Fregean view denies that [1a] and [1b] express the same proposition.² Consequently, it denies that [2] states a contradiction, thus delivering the truth conditions ordinary speakers would expect. Let us see, for a moment, how the theory accomplishes this. It claims that for each simple singular term token *t* there exists a corresponding sense, or mode of presentation (*mp*); moreover, this *mp* is the propositional contribution of *t*. Given the assumed background information concerning Lois, and assuming the ignorance of the belief reporter to the identity of Kent/Superman, the occurrences of 'Kent' and 'Superman' would express distinct senses. This difference would allow the respective that-clauses in [1] to refer to distinct propositions and thereby for [1a] and [1b] to express distinct propositions.

The above story provides for mp-variation across speakers, as it makes a claim about simple singular term-tokens, not types. Framed in terms of types, the account would not allow for the evident possibility that different speakers associate different modes of presentation with the same simple singular term. Yet someone informed about Superman will most likely attach a sense to 'Kent' different from the one Lois attaches to that name. Such a person might even mean the same thing by [1a] and [1b], given an equally well-informed audience, and thus, were he to utter it, would express a contradiction by [2]. The Fregean may explain this variability by suggesting that a token belief report makes salient certain features among speaker and audience that help to determine the reference of the that-clause. Thus, for example, an utterance of [1a] provides information indicating to the audience which mode-of-presentation-containing proposition is being referred to. One constraint on the reference of the that-clause is that it must be the very proposition that the *subject* of the belief report (in this case, Lois) associates (or would have associated) with the content sentence. The problem with this condition is that it is unlikely that the context can be relied on to provide information sufficiently rich to determine reference to such propositions. For example, if this suggestion is true, no contemporary speaker would be in a position to assertively utter the sentence 'Wagner

believed that Ludwig was mad.' For no such speaker could know just which mode-of-presentation-containing proposition (expressed by 'Ludwig ist verrückt') Wagner believed. And yet we can unproblematically assert the proposition that Wagner believed that Ludwig was mad (whatever that is) with the aforementioned sentence.

The obvious response for the neo-Fregean is to simply lift the above constraint: in a belief-report-token, the reference of the that-clause is to a mode-of-presentation-containing proposition that *the speaker* associates with the content sentence, and not necessarily one that the *subject* associates with the content sentence (or a translation thereof).³ Yet this reduces the implausibility only slightly. For it is not at all obvious that the information that the tokening makes salient suffices to determine reference to such a proposition either. The prior move suggested that, for an utterance 'A believes that S', the that-clause refers to a contextually salient mode-of-presentation-containing proposition *that A associates with 'S'*. My suggestion is that the move becomes scarcely more plausible if we eliminate the italicized condition.⁴

A problem for this variation on Fregeanism formulated by Mark Richard (1990) is even more damaging. He argues that if we take the reference of 'that Hesperus = Phosphorus' in A's assertive utterance

[3] Everyone who believes that Hesperus = Hesperus believes that Hesperus = Phosphorus

to refer to a mode of presentation that A associates with that sentence, and if we assume that A associates the same sense with both 'Hesperus' and 'Phosphorus', then A's utterance of [3] is true. Any good Fregean will tell you why. A's utterance of [3] is true just in case:

[3'] Everyone who believes the proposition A associates with **Hesperus = Hesperus** believes the proposition A associates with **Hesperus = Phosphorus**.⁵

Given our assumption that (the proposition A associates with **Hesperus = Hesperus**) = (the proposition A associates with **Hesperus = Phosphorus**), [3'] is true; so [3] must be as well.⁶

Note that it is possible for the Fregean to respond as follows: A true utterance of [3] would raise to salience the mode of presentation that A associates with 'Hesperus is Phosphorus,' manifesting A's intention to refer to it. Thus, at a context at which A did intend to convey [3'] in uttering [3], A's intention would be recognized by his audience. I will address this response later (in section IV), when we have sufficient resources to evaluate it.

A related problem with Fregeanism, formulated by Stephen Schiffer (1992: 507-08), is that it generates intuitively incorrect truth conditions for sentences like the following:

[4] Everyone who has met him believes that Al Sharpton shampoos regularly.⁷

For, according to even the weakened account, this is true just in case there is a unique mode-of-presentation-

containing proposition *p* such that *p* presents Al Sharpton and the property of shampooing regularly, and everyone who has met Sharpton believes *p*. Yet it is plain that this sentence could be true even if (as is of course the case) not all those who have met Sharpton represent <Al Sharpton, the property of shampooing regularly> to themselves in precisely the same way.

1.2 Direct Reference.

Another strategy is to deny an allegedly platitudinous principle that allows [2] to be derived--this is the disquotation principle, which we can state as follows:

[D] A believes that *S* just in case A is disposed to reflective, sincere assent to '*S*'.⁸

This strategy acknowledges that [1a] is true given evidence that Lois assertively utters the sentence 'Kent writes for the Daily Planet,' but it denies that Lois's disavowal of a tokening of [1b] constitutes evidence for [1c]:

[1c] It is not the case that: Lois believes that Superman writes for the Daily Planet.⁹

Each belief report token, in addition to expressing (or semantically "encoding") a relation (*believes*) between a thinker and a proposition, *implicates* or imparts another proposition about how the thinker in question represents the ascribed proposition to herself. Often, this imparted information is as valuable, relative to the conversational interests of the participants, as the semantically encoded

information.¹⁰ Thus, the appearance of [1b]'s falsity is explained as follows. Jimmy Olsen's utterance of [1b] makes salient a proposition about how Lois represents *that* Superman writes for the *Daily Planet* to herself which is in fact false. Someone might demur when asked whether [1b] is true, as they might reasonably take the question to concern a proposition implicated by that sentence, and not its semantic content. This possibility would explain the appearance of [1b]'s falsity and by the same token, the impossibility of [2]'s being true.

There are two options one might pursue in developing this pragmatic strategy. The pragmatically imparted 'representational' proposition might concern a particular way in which the thinker presents the proposition to herself. That is, the proposition might make reference to a particular psychological state which is implicated in the tokening of the belief. Or, the imparted proposition might concern a more general aspect of the thinker's mental representation. It might, that is to say, place a condition on how the thinker is to be taken to be representing the proposition to himself. For example, in producing [1a], Jimmy Olsen makes salient a proposition to the effect that Lois believes *that* Kent is a writer for the *Daily Planet* under (e.g.) a newspaper-reporter-presenting mode of presentation, without taking a stand on which one it is; in producing [1b] another proposition is made salient, in this

case one to the effect that the proposition is believed under a superhero-presenting mode of presentation.

In each case, the pragmatically imparted propositions are about *modes of presentation*.¹¹ They are not modes of presentation as Frege understands this notion, since for Frege those entities functioning as modes of presentation also fix reference, in Kripke's phrase. No such dual function is ascribed to modes of presentation on this account. Another contrast with Frege is that, under the direct reference analysis, modes of presentation do not figure in the propositions directly expressed by belief report tokens. Nonetheless, the pragmatic strategy goes exactly as far in accommodating the explanatory role of belief ascription as the thesis that modes of presentation are the actual semantic content of belief reports.¹² For in each case, whether through semantic or pragmatic means, my utterance of a belief report produces a belief in my audience about the way in which the subject believes the proposition referred to. How a person believes something is typically thought to be relevant to her subsequent behavior. The pragmatic strategy provides a way of understanding the mechanism by which this mode is transmitted that is compatible with propositions' being course-grained entities. Insofar as it does this it cannot be faulted for disregarding ordinary conversational interests in ascribing beliefs--namely, in predicting and explaining behavior.¹³

Note that the strategy of denying [D] is available to any theory of belief reports, but it is most attractive to direct reference theorists--who, as we know, maintain that 'believes', as it occurs in 'A believes that S', is a binary relation, holding between thinkers and propositions. (It is, by the same token, unattractive to neo-Fregeans, as it leaves their account of belief ascription without motivation.) For the success of this strategy would remove the most overwhelming obstacle to acceptance of the theory. On the other hand, it should be noted that this response goes beyond what is implicit in our belief-ascribing practices. If Lois responds negatively when asked whether Superman works for the Daily Planet, it would seem rather arbitrary to discount this information and staunchly adhere to the claim that she believes it--the pragmatic proposal sketched above notwithstanding. I think there are good arguments for the strategy, however.¹⁴ But it is revisionist; and our concern is to describe our actual practices, not to recommend alterations of this practice. It may, of course, be the case that the practice admits of no consistent description. Since Tarski, it has been commonly accepted that our use of the truth predicate cannot be consistently described. (Although there are some who think that the inherent inconsistency of concepts such as 'true' provides no real cause for concern.¹⁵) Consequently, any acceptable theory of truth must provide a partial

revision of this practice. If we come to similar conclusions about belief reports--that they too admit of no consistent description--then we will be forced to revise (or possibly abandon) our practices. Yet it is not yet clear that our practices are in fact inconsistent. Thus it would be premature to adopt this strategy.

1.3 Minimalism.

Another strategy, endorsed by Mark Johnston (1988), denies that any substantive theory is required to accommodate belief reports. As Johnston writes:

Meaning has no hidden and substantial nature for a theory to uncover. All we know and all we need to know about meaning in general is given by a family of platitudes ... (38)

He labels this doctrine "minimalism". It claims that the logical behavior of 'believes' is exhausted by certain platitudes. Among these are the following (we abstract from context-sensitive expressions such as indexicals and demonstratives):

[B] A believes that S iff: Bel(A, the meaning of 'S')

[M] S means p (in L) iff: L-speakers, in uttering S assertorically, assert p.

Presumably we can add principles such as [D], and [T] below, which have self-evidence to recommend them.

[T] If S means p (for a community of speakers, relative to a context C), and S' is a translation of S, then S' means p (for a community of speakers, relative to C).

Johnston's positive doctrine is that we can derive meaning facts from facts about the potential of expressions to have meanings. That is, we analyze, following [M], the fact that *S* means *p* in *L* in terms of *S*'s potential to mean *p* when uttered assertorically by *L*-speakers. In this way facts about meaning supervene on facts about use. But the analysis provides no illumination:

So far from offering hope of a reduction of talk of sentence meaning, the platitudes simply show how we can get from '*S*' having the potential to be used to mean that *p* to '*S*' meaning that *p*. *It is just a simple paraphrase which gets us to reifying talk about the meaning of 'S' being p* (39, emphasis added).

The fact expressed in [B] is similarly insubstantial: it neither presupposes, nor implies, any substantive claim about reference or meaning. To deflect the obvious objection that according to [B] that-clauses are referential singular terms, and hence "ontologically committing", Johnston (following a recommendation by Schiffer, 1987) suggests that we construe quantification into that-clause positions as substitutional. (A suggestion not entirely unproblematic.) This would preserve the ontological neutrality of his minimal theory of meaning. Alternatively, he mentions the idea that the reference of a that-clause is not compositionally determined and is itself, in fact, unstructured (37). (In contrast, other unstructured meaning-candidates--possible worlds, the 'situations' of Barwise and Perry--are referred to by expressions possessing

compositional structure.) On this view meanings or propositions are insubstantial entities, mere "shadows of that clauses". This strategy avoids the need for an explanation of how the that-clauses in [1] make different contributions to the respective propositions expressed: they make different contributions simply by *being* different that-clauses. In each case, the strategy is to deflate the semantic-significance of expressions such as 'the meaning of *S*,' 'the proposition that *S*,' etc. Such expressions either require no referential analysis, or they are given a referential analysis, but with that relation denatured and insubstantial.

Unfortunately, these seemingly minimal facts are sufficient to derive our paradoxical results. [B] seems to generate the same paradoxes as its more substantive counterpart--the relational theory of belief. There are various arguments designed to show the incompatibility of the relational theory with certain views about the reference of that-clauses. The Lois Lane example attempts to show the incompatibility of the relational theory with the view that that-clauses refer to structured propositions. The direct reference theorist, under the grip of a certain picture of semantical reality, denies that Jimmy Olsen's dissent from [1b] should be taken at face value. If *x* assents to *S* (in the example [1a]), then *x* believes that *S*; but Olsen's disavowal of a sentence *content-identical* with *S* (i.e.,

[1b]) should not be taken as counter-evidence to the claim that he believes that *S* unless we can suppose that Pierre recognizes that *S* and *S'* are synonymous (in which case, of course, the assumption of his rationality or his seriousness would have to be withdrawn). It is hard to see how someone could explain away the appearance of a contradiction here without going beyond semantic appearances. For the minimalist to maintain that [B] does not imply a contradiction in this case could only be accomplished by restricting the platitudes in a way not justified by usage (say, by denying [D]¹⁶). At this point minimalism begins to lose its charm. For any alteration of the platitudes would be motivated by a picture of a semantic reality lurking behind our practices that cannot simply be read off of those practices. And this is at odds with Minimalism. As Johnston writes in a recent paper on personal identity: "Practices that spread and endure are typically justifiable in nonmetaphysical terms" (1992: 590). On the other hand, if we leave the offending principles unaltered we must countenance the same paradox that Minimalism's more robust counterparts are forced to countenance.

1.4 The Hidden-Indexical Theory.

Finally, there is the view that maintains that, in at least certain tokenings, [2] does not constitute a contradiction, even as it acknowledges that the respective that-clauses co-

refer. This view locates a propositional component in belief reports that is not explicitly referred to, but implicitly specified in their tokening. This component provides a restriction on the mode under which the subject of the report is acquainted with the proposition referred to. The strategy in question is the hidden-indexical theory. The theory preserves Fregean substitutivity intuitions, but is immune to the objections leveled against Fregean theories in 1.1. What is needed is a theory that acknowledges the context-invariance and speaker-invariance of that-clause reference (at least those that-clauses not containing explicit context-sensitive expressions). Otherwise the ability of ordinary speakers, upon hearing a belief report *u*, to determine the reference of its that-clause, and thus to determine the proposition *u* expresses (or, more neutrally, the conditions under which *u* is true), is left unexplained. It is this relativity of that-clause reference to belief reporter, or to belief-subject, that is responsible for the Fregean's unacceptable analyses ([3] and [4] above). What HI provides is an account which denies this relativity, but which also denies that a simple binary analysis of belief suffices. Relativity to speaker or subject yields bizarre consequences, as does the insistence that nothing matters semantically to the evaluation of a token belief report beyond the person and proposition

referred to. The resolution is to introduce context-relativity, but not at the level of that-clause reference.

In spite of its attractiveness, Stephen Schiffer has advanced a number of arguments against the hidden-indexical theory. In the next section I will state the theory. I will then state Schiffer's criticisms. Finally, I will consider in detail four separate responses that a hidden-indexical theorist can make to these criticisms. (At least one response departs somewhat from the letter of HI, although it does retain HI's incorporation of context-provided constituents into the propositions expressed by belief report-tokens.)

2. The Hidden-Indexical Theory Stated

According to the hidden-indexical theory, *believes* expresses a three-place relation, $B(x, p, y)$, where x is a speaker, p a proposition, and y a mode of presentation. But the truth definition schema for a belief report is not derivable from this information alone. First, truth is defined for utterances of belief reports, even where such reports do not explicitly contain context-sensitive elements.¹⁷ For HI locates unarticulated indexical elements in all such reports. Rather than making reference to a particular mode of presentation, the indexical makes reference to a property of modes of presentation (what I will call a 'style'), functioning as a constraint on modes of presentation. This

indexically-provided information must be represented in the full analysis of a belief report.¹⁸ (The Fregean view discussed in 1.1 maintains that in each tokening of a belief report reference is made to a particular mode of presentation. But, as we have seen, whether or not this reference is made purely on the basis of the meanings of the expressions explicitly contained in the utterance, or partly on the basis of information the utterance itself makes salient, the Fregean account still delivers the intuitively incorrect truth conditions for sentences such as [4].¹⁹)

The logical form HI ascribes to any particular utterance of the following type:

A believes that S

is:

[HI] $\exists x(\phi x \ \& \ B(A, [S], x))$.

For example, to an utterance of:

Jones believes that George is a woman

it assigns:

[*] $\exists x(\phi^*x \ \& \ B(A, [George \ is \ a \ woman], x))$.²⁰

[*] should be read as maintaining a proposition to the effect that, for some contextually definite property ϕ^* (to which the belief report makes tacit reference), something both is ϕ^* and presents the proposition that George is a woman to Jones. The relevant property will be a conversationally salient condition on modes of presentation, a condition which varies with the interests and expectations

of the conversational participants. This feature of belief reports--their context-sensitivity--indicates that they contain an indexical component. Yet, the condition stated in the truth definition does not appear in the actual report itself; it is "hidden".

In a sense, HI posits a "hidden variable" that accounts for the variation in truth conditions of token belief reports (such as [1a/b]) whose articulated constituents are identically semantically-valued. This allows HI to maintain, with direct reference theories, that that-clauses are semantical singular terms, referring to structured propositions. It departs from such theories in that it maintains that *believes* expresses a three-place relation, $B(A, p, x)$. In addition, it holds that a belief report '*A believes that S*' expresses an existential quantification of a formula of the form ' $\phi x \ \& \ B(A, p, x)$ ', where ϕ is a contextually determined, yet unarticulated, property, and *A* and *p* are supplied by the report's articulated constituents.

The theory of direct reference poses no obstacle to compositionality; it claims that a belief report asserts a relation between a thinker and a proposition and that these respective elements and their sequential ordering are finitely determinable on the basis of the surface syntax of the report and the semantical interpretation of its semantic primitives.²¹ Yet, if the theory of direct reference is compatible with compositionality, HI will be as well. HI

goes beyond direct reference only in that it specifies an additional argument position in the *believes* relation and maintains that a belief report token places implicit constraints on what can occupy that position. (This "constrained" position, as we have seen, is quantified into.) None of these features of HI conflict with the assumption of compositionality.

HI's resistance to typical counterexamples can be seen even from the relatively skeletal remarks already made. It can be shown that while the *that*-clauses in [1a] and [1b] co-refer, tokenings of the respective sentences do not necessarily express the same propositions. For the individual tokenings may raise to salience distinct mode-of-presentation properties.²² As these properties are constituents of the propositions expressed, it follows that the propositions will, in such cases, be distinct. This will allow for the possibility of [1a] being true and [1b] being false and, hence, of [2] being true.

Thus, the theory nicely captures our truth-conditional intuitions regarding belief reports. However, certain features of the theory are troublesome--namely, the reliance on modes of presentation and their properties. Of course this is a deficiency that the hidden-indexical theory shares with Fregeanism and with direct reference theorists as well. But Fregeanism is not a genuine alternative at this point, and the direct reference theorist has the option of

characterizing the concerns as 'pragmatic' and outside the purview of semantics proper. (Not that this latter maneuver is a proper response to a genuinely philosophical problem.)

In the following section I will state what I take to be the central problem facing HI--what Schiffer has called the "meaning-intention problem". Before proceeding to my three lines of defense of HI against the meaning-intention problem (section 5), I will first describe the overall structure of the responses to be put forward (section 4).

3. The Meaning-Intention Problem

3.1 The Inaccessibility of Styles.

Now to Schiffer's objections. Harold reports:

[5] Jane believes that Julia Kristeva is a genius.

According to [HI], Harold has said that

[6] There is an mp that is Φ and is such that Jane believes [Julia Kristeva is a genius] under it.

Schiffer objects that neither Harold nor his audience "is aware of [his] meaning any such thing" (513). This is not simply to say that there is no unique style--no property of modes of presentation--that we can plausibly hold to be conversationally salient. We will investigate this criticism presently. But the more pressing difficulty is that what Harold says in producing [5] seems to have nothing whatsoever to do with modes of presentation and their properties. This contrasts with paradigmatic cases of

unarticulated reference. For example, Harold's inscription of 'It's three o'clock' in a diary makes unarticulated reference to both the time and place of the writing. But in such a case the reference, although implicit, would obviously be transparent to the writer and (if contextual parameters are evident) the audience. In contrast, the claim that Harold, in producing [5], was similarly aware of making reference to modes of presentation and their properties strikes us as deeply unintuitive, if not downright false.

It thus seems unlikely that Harold was aware of making reference to a style in producing [5]. But, if he was unaware of this, it becomes unclear how he could have meant what [6] suggests he meant. Moreover, Schiffer argues, Harold seems to be in no position to specify the style referred to by ' Φ ' in [6]. This property would be a function of the style-relevant properties of the individual Julia Kristeva and the attribute of being a genius, and it seems implausible to suppose that Harold is capable of referring to this property. This latter problem I will address at length below, so let us set it aside for now. In any case, we seem faced with the conclusion that if HI is true, then speakers in general, when ascribing beliefs, are unaware of what they are saying *and* of the propositions they are expressing.

The quick and easy solution to this problem is to invoke the notion of tacit belief (and tacit intention)--belief-like (and intention-like) states which are related to perception and behavior in manners similar to explicit belief, but which are inaccessible to conscious inspection and which are inferentially inert with respect to explicit belief (e.g., the tacit belief that S doesn't imply the explicit belief that S). The HI theorist can acknowledge that Harold does not explicitly believe that he is referring to a style ϕ in producing [6], and that he does not explicitly intend to get his audience to believe that, for some mode of presentation x, x is ϕ and Jane believes that Julia Kristeva is a genius under x. For the position is available that Harold *tacitly* believes that he is referring to ϕ and *tacitly intends* to convey the above proposition to his audience in producing [5].

This is not the happiest of outcomes, as Schiffer points out. For speakers in these cases have no conscious awareness of the propositions they are expressing and, additionally, *fail to have the conscious thoughts they think they have*. Surely Harold thinks that what he is saying is completely captured by [5] and thus that he is consciously aware of all that he means in uttering this sentence. But according to HI, [5] provides an incomplete specification of the proposition it is used by Harold to express. Consequently what he thinks he is consciously aware of--the

complete content of his assertion that [5]--he is in fact not consciously aware of. And this is at odds with certain intuitions we have about privileged access to our own mental states. (This is not ameliorated by the fact that he knows--tacitly--more than he thinks he does.)

On the other hand, we should not be overzealous in dismissing theories--especially in relatively speculative areas--because they conflict with what seem inviolable assumptions. Perhaps certain facts about her intentions are not transparent to the speaker; perhaps, in fact, what she takes to be her intention is not her intention. This possibility will call for a re-evaluation of our accounts of speakers' meaning, but that might be a reasonable price to pay if our account is in other respects satisfactory.

In short, that Harold is not aware of having referred to Φ in producing [5] is a *prima facie* worry. But it would be hasty to dismiss a theory simply because it violated the assumptions in question--that facts about one's meaning or intention must always be transparent to consciousness.

3.2 The Multiplicity of Styles.

Another aspect of the meaning-intention problem has to do specifically with styles. In many cases of belief attribution it will be evident that there are indefinitely many modes of presentation under which the ascribed proposition is believed. In the case of [5], for example, a

variety of styles may be available, no one of which is especially salient. How then could Harold mean something of the form of [6] in uttering [5]? To mean something of that form, he and his audience would have to be mutually aware of a style. Only then could the utterance of [5] convey information about *this* style, in particular. Yet, given the plurality of available styles, there should be no expectation on Harold's part that his attempt at implicit reference would succeed; for nothing provided by contextual features could isolate a unique style. Hence, reference to a style could not be made.

I will call this problem the *multiplicity problem*. It contains a powerful objection to HI. In most cases of belief reporting, the conversational context does not raise to salience a unique property of modes of presentation that both speaker and audience know to hold of a relevant mode of presentation. While the cognitive environment contains many such properties, it fails to isolate one particular property that the speaker can reasonably assume she can be taken to have referred to (and thus that the hearer can justifiably assume that the speaker intended him to identify).²³ This is especially likely in Schiffer's example, where Flora says:

Harold believes that TWA is offering a New York-Paris return fare for \$318.

According to [HI], Flora has said that there is some mp, x, that has the mode of presentation property, or style, ϕ , and

that Harold believes [TWA is offering a New York-Paris return fare for \$318] under x. Yet, no one style can be guaranteed to be especially salient. (I say "especially salient" to convey the idea that a certain range of styles are mutually available--they each would suffice for the purposes of the exchange--yet none of them is, so to speak, most salient.)

This problem is made vivid by a thought-experiment Schiffer devises. Schiffer coins a verb, 'shmbelieves', that by definition means what the hidden-indexical theorist claims that 'believes' means. That is, in uttering

A shmbelieves that S

a speaker has asserted a proposition of the form:

There is an x that is Φ and is such that A believes [S] under it,

where ' Φ ' refers to a style that the utterance makes evident. Now take our sentence [6], substituting our new expression for 'believes'. The result is:

[5'] Jane shmbelieves that Julia Kristeva is a genius.

But, the objection proceeds, while Harold's utterance of [6] is clearly meaningful, it isn't clear that Harold's utterance of [5'] has any determinate content. For it is implausible that Harold can specify Φ and thus it is implausible that he can intend to refer to Φ , since (among other things) he cannot make manifest to his audience which among the available styles he is referring to. Consequently nothing has been asserted. In contrast, no such obstacle

appears to be present in his asserting [5]: Harold obviously said something, whatever that might be, in producing [5]. Schiffer concludes: "Since 'shmelieves' is by stipulation what 'believes' would be if the hidden-indexical theory were true, this appears to show that the theory is not true" (517).

4. Structure of the Responses.

I have no one solution to the problems Schiffer's objections raise. Two general lines of response suggest themselves. In what follows I will develop these types of response individually. According to the first, determinate reference is made, in the tokening of a belief report, to a style. According to the second, no such determinate reference is made: it is denied that there is a fact of the matter as to what style is referred to, and thus what is meant, in the tokening of a belief report. I will use the apparatus of supervvaluations to provide an account of the truth conditions of belief report tokens consistent with this idea. Supervvaluations supply us with (*so to speak*) *determinate* indeterminate references. That is to say, we do not assume that a speaker makes determinate reference to a particular style (the reason being that the multiplicity problem renders the assumption dubious), but we assign truth conditions in such a manner that it is as if a particular determinate reference was made. It should be noted that the

latter line of defence is a defence of something not quite the hidden-indexical theory. For, according to HI, belief reports make determinate (although implicit) reference to mode-of-presentation-properties. According to the theory defended on the second strategy, objective reference is not made to such a property. Thus the theories are distinct. Yet, the difference is slight when compared with the similarities--for example, according to each theory, styles made salient by the tokening of a belief report place conditions on the mode of presentation under which the ascribed proposition is 'grasped'.

The first strategy actually has three variations. The first variation is the simplest. It attempts to argue that the cases in which there are a multiplicity of styles are precisely the cases in which the style requirement should be dropped. That is to say, in some cases it is mutually manifest that no one style is especially salient. In such cases belief is ascribed directly, without an intervening mode of presentation. What the opposition must argue is that styles are in fact needed in such cases. But, the rejoinder goes, to be made good, such a claim will ultimately have to describe the situation in a way that in fact makes it plausible that a style is mutually manifest.

The second variation suggests sacrificing the standard Gricean accounts of speaker's meaning: for A to refer to ϕ in uttering u is not necessarily for his hearer, H, to

recognize his intention to do so. This circumvents the multiplicity problem by eliminating the requirement that there be a fact of the matter concerning conversational salience. A might refer to style ϕ in uttering u; H might take her as making reference to ϕ' in uttering u. If ϕ and ϕ' are sufficiently similar (they allow more or less the same substitutions, say), then communication has occurred--even though no single proposition has been transmitted.

The third variation retains the Gricean notions of speaker's meaning and communication, but maintains that conversational interests do select the relevant styles in all cases of belief reporting. This variation is a straightforward defense of HI. It claims that reference to mode-of-presentation-properties can proceed, for the most part, unproblematically. The distinctive feature of this defense--something that seems implicit in Crimmins (1992)--is that reference to mode-of-presentation-properties does not presuppose individuating knowledge of such properties. I argue that if such a requirement (of individuating knowledge) is not placed on reference, then there is no obstacle to holding that reference to modes of presentation can be made even absent individuating knowledge. It seems, furthermore, that well-known arguments due to Kripke show that individuating knowledge is not a requirement on successful reference.

Finally, the second strategy abandons determinacy altogether. The obvious suggestion here is that this view takes belief reports to be vague. Following Kit Fine (1975), I take vagueness to be deficiency of meaning. Put in a manner relevant to the current discussion: the linguistic meaning derivable from the utterance of a belief report is often insufficient to provide a propositional constituent. (Indexicals, like 'I', are examples of expressions whose *linguistic meaning* underdetermines their propositional contribution.²⁴ We can take vague expressions to have linguistic meanings or rules which are similarly 'incomplete', but with the difference that, in the case of vague expressions, supplying a context does not necessarily provide a determinate propositional constituent.)

Harold's verbal report (of [5]) is compatible with a variety of precisifications of its meaning. Since the completed meaning, so to speak, is what is valuable in the transaction, Harold's report is insufficiently detailed. In contrast, 'Fire engines are red' is compatible with a variety of precisifications, but, in some contexts (e.g., in response to the question 'Are fire engines red or blue in Manhattan?') the vagueness is compatible with conversational interests. In the current case, the correct analogy would be, in the context of the question 'Do you want your walls egg-shell white or China white?' a response such as: 'I just want my walls white.' The information is simply not

precise enough for the purposes of the exchange, which is to determine exactly the shade of white the customer wants.

(We assume that the response is not the exasperated response of: any precisification will do. For such a response would be precise.) Thus, unlike other vague sentence-types, belief reports are informationally useless unless precisified.

This latter account I will call the *super-truth* account, as the semantics it provides for belief sentences defines their truth conditions in terms of their "super-truth" conditions. This parallels a similar treatment of vague sentences by Fine.

The following sections will pursue, respectively, the assumption that determinate reference is made to a style in the tokening of a belief report, and the assumption that no determinate reference is made. Section 5 treats three defenses of HI compatible with the determinacy assumption, and section 6 treats one defense of HI compatible with the contrary assumption.

5. Determinate Implicit Reference.

5.1 Shifting the Burden of Proof.

Let us, then, return to the multiplicity problem. As has been suggested, the response is available that not all utterances of belief reports make implicit reference to styles. In such cases we can imagine that this information

is itself part of the cognitive environment--that no condition is being placed on the way the believer referred to is acquainted with the ascribed content. Formally, this can be expressed by putting a vacuously satisfied condition on modes of presentation--self-identity, for example. It could be the case that *only* when it is mutually evident that a way of thinking is relevant for the communicative purposes of the speaker does a mutually evident style get referred to.²⁵ (Or, more plausibly, only when it is *not* mutually evident that a way of thinking is thus relevant.)

According to HI, a mutually evident style is ascribed to a way of thinking of a proposition whenever a belief report is tokened. But in some cases the style-provision requirement might be canceled. In such a case the proposition expressed by the report would simply be that the subject believed the ascribed proposition under some mode of presentation or other.²⁶

The argument for the claim that reference to a style is always made in the tokening of a belief report would have to be based on substitution failures. The claim would presumably be something like the following: each true belief report token has at least one substitution instance which is false. I am suggesting that, in some contexts, any substitution-preserving inference is sound. For example, cases where it is evident that speaker and hearer have no knowledge of how the subject of the report would have

grasped the proposition. Now it is open for someone to argue that even in a sentence such as 'Napoleon believed that Constantinople was once the capitol of the Roman Empire' certain substitutions for 'Constantinople' (e.g., 'Istanbul') would not be sound. (Against this, see Soames, 'Substitutivity' (1987b), which provides, to my mind, a convincing case for the arbitrariness of substitutivity intuitions.) But it seems more likely that it would be common knowledge among speaker and hearer that no attempt could be made to refer to a style, since any claim (implicit or explicit) about how Napoleon believed the proposition would be pure conjecture. That is to say, it is part of the cognitive environment that no style could be specified.

A genuine counterexample would provide a context in which a style is necessary for communicative purposes and yet it remains implausible that one is mutually evident to speaker and hearer. If it is not demonstrated that postulation of a style is necessary, the hidden-indexical theorist could demand evidence that, in the case in question, a style was referred to. And yet, it seems that the stronger our intuitions that substitutivity fails (and that a style is necessary), the stronger our intuitions that there is a mutually evident style. This seems to be the case because any description of a context in which substitution within the that-clause fails would have to advert to features of the cognitive environment in which

either the conversational participants were both aware that a particular mode of presentation was being referred to, or that a mode of presentation type was salient.

Take the case of A's uttering the following in the above context, to communicate to H simply that Lois believes that the particular, Kent, flies:

[7] Lois believes that Kent flies.

Since a tokening of [7] among A and H raises to salience a style that applies only to mild-mannered-reporter-involving styles, the utterance is false. Why? Lois doesn't believe the proposition [Kent flies] under that kind of mode of presentation and this tokening of [7] asserts (among other things) that she does.

There is the following response to this defence. 'But there are lots of mild-mannered-reporter modes of presentation, none of which are rendered particularly salient by A's tokening of [7]. So here is a case where styles are necessary according to your criterion and yet no one style is made conspicuous by the context.' But the rejoinder is available that a single style is made salient, one that possesses the common features of all (or most) of the candidate styles. Actually, the style doesn't have to encapsulate the common features of even a majority of the candidate styles for it to be salient. It is enough that it has more in common with the other candidate styles (or, alternatively, has common features with more of the other

candidate styles) than any other single style. This style is an abstraction from various ways of presenting Kent mild-manneredly. It is this that is salient in the context I have described.

This allows us to see how Schiffer's analogy between the multiplicity problem and the problem of incomplete descriptions breaks down (Schiffer, 516). The latter problem concerns a certain theory of definite descriptions. This theory maintains that for any sentence-token *u* containing an incomplete description *D* there exists a sentence type containing a complete description *D'* whose tokening *u'* would be equivalent in meaning to *u*.²⁷ Thus, for example, what A meant in uttering 'The man sneezed' is, according to this view, equivalent in meaning to some completion of 'The man who is ... sneezed.' But that is to say that A intended, in uttering the former, to produce the belief in his audience that the man who is allergic to long-haired cats sneezed, or the man who just bought pepper at Zabar's sneezed, or the man who said he was going to swear off Indian food sneezed. And it seems that any such completion is arbitrarily selected, for, while many completions will typically be salient and thus be considered eligible for what is meant, none is especially so.

But, as suggested, the analogy seems inappropriate. We can concede that many styles are salient in a typical tokening of a belief report. But, in contrast with the case

of multiple descriptions, it is possible to argue here that the class of conspicuous styles is unified by a property common to a majority of its members and it is *this* that the utterance makes especially salient.²⁸ On the other hand, in the case of incomplete definite descriptions, this possibility is unavailable: we cannot derive a completion of 'The man' by abstracting from all its potential completions or even from a privileged subset of the completions, for in most cases these completions have nothing in common.

The objection continues. 'What you say may often be true. But in many cases the salient mode-of-presentation properties will possess no common features. For example, some people think of Kristeva as a French semiotician, some as the author of a book on Chinese women, some simply as a famous French feminist. None of these ways of thinking of Kristeva need have anything in common. Thus, it is not open to you to suggest that the style made salient by a tokening of [5] is an abstraction from all the available styles.'²⁹ For, as in the case of the incomplete description discussed above, the set of styles will have nothing in common.' This is a compelling objection. If it is true that I am intending to refer to a given style, ϕ , then it simply cannot be the case that various candidate styles are all equivalently salient. For my intention couldn't then be realized. Yet, nothing I have said so far rules out the

possibility of multiple styles being equivalently salient. What the objection states is that, at least in the hypothetical case, HI has it that I intend reference to a particular style, but that the requirements for reference--namely, mutual awareness among speaker and hearer of a contextually definite style--are absent. And this is exactly parallel to the case of incomplete definite descriptions.

But it is not obvious in the above case that both the following are true: (i) I intend to refer to a style and (ii) no one style is particularly salient. I suggested above that styles are motivated only insofar as they support our substitutivity intuitions. In the suggested counterexample, no one style is salient. But then it seems clear (from the point of view of HI, at least) that we have no reason to suppose that substitution should fail here. In other words, the objection can't have it both ways: it can't be assumed that no one style is salient and that substitution fails without an explanation of the failure. And it seems that any explanation of the failure would have to suppose a situation in which a style was conspicuous to speaker and hearer.

If this sounds somewhat circular, consider that the explanation a Fregean would give for the failure of substitutivity would be no better off than the hidden-indexical theorist's. For she would assume something far

less plausible--that a particular mode of presentation was being referred to. Yet, if one argues that it is implausible that an utterance of a belief report fails to provide information rich enough to enable reference to a mode of presentation type, one must concede that it is much less plausible to suppose that it provides information sufficiently rich to allow reference to a mode of presentation token. Moreover, if a direct reference theorist were to explain the (perceived) failure of substitutivity, he would claim that the respective belief reports each implicated propositions concerning either mode-of-presentation-types or tokens and that these implicatures were incompatible. Thus, even though the direct reference theorist countenances an unrestricted principle of substitutivity, in his explanation of the pragmatic facts he must make use of the same resources as the Fregean or the hidden-indexical theorist (mode-of-presentation types, and tokens). Consequently, he must assume as well that the context of utterance provides information sufficient to secure reference to either mode-of-presentation-types or tokens--even if these do not figure in the truth conditions of the belief reports themselves.

There are obviously other treatments of belief reports. My point here is merely to show that one cannot simply imagine a situation that by definition falsifies HI: one must explain how that situation is possible. And I cannot

see how the explanation would go unless it made assumptions either similar to, or less plausible than, those made by the hidden-indexical theorist. This is not a knock-down argument against the criticism; it simply shifts the burden of proof.

5.2 Meaning without Intention-recognition.

The second strategy is more direct. It argues that HI provides the correct account of the logical form of belief reports; but, unlike the first strategy, it meets the multiplicity problem head-on. The suggestion is made that our misgivings about HI can be traced to Gricean conditions on what counts as successful communication, and argues that these conditions are not inviolable. The operative condition amounts to the requirement that in order for a speaker A to mean *p* in uttering *u*, A must make his intention to get the audience, H, to think *p* fully manifest. This requirement gives rise to a stronger claim: that in order for A to mean *p* in uttering *u* this fact itself must be evident to H. Let us call this the *transparency requirement*: If A means *p* in uttering *u*, then it must be evident (in particular, to H) that A means *p* in uttering *u*. If this requirement fails to hold, then, according to the Gricean, *u* cannot be said to possess determinate meaning; nor can A be said to have communicated a determinate proposition to H. Schiffer makes this requirement explicit:

Surely, it ought to be a consequence of any acceptable theory of meaning that, if a speaker, speaking literally, meant that *p* in uttering *u*, then one who understood his utterance perfectly well took him to have meant that *p* in uttering *u* (Schiffer, 1981: 77-78).

Yet, given that the possibility of a meaning-theory seems effectively blocked by the problem posed by belief ascriptions, it might be reasonable to reconsider this requirement. This seems especially so in light of the fact that in relaxing the requirement the multiplicity of styles ceases to pose a problem.

The proposal is quite straightforward. In producing a belief report, *u*, *A* makes reference to a style ϕ . But making this reference does not presuppose that the hearer, *H*, recognize *A*'s intention to refer to *H*.³⁰ A fortiori, it does not presuppose that *A* expects that *H* recognize *A*'s explicit intention. What *H* does is *interpret A's utterance*. The conditions on correct interpretation are not as stringent as those traditionally placed on communication--where communication is understood in terms of the hearer's either grasping the same proposition the speaker intends to be grasped, or the hearer's simply identifying the intended proposition. In particular, *H* does not have to associate the same proposition with *u* that *A* does. The respective propositions associated with *u* should be similar to one another, but the similarity is itself likely to be context-sensitive and interest-relative. In general, however, for belief-ascription a test for similarity might be in terms of

substitutions both speaker and hearer accept. To the extent both accept the same substitutions in *u*, we can maintain that the respective propositions associated with *u* are similar enough for communication to have taken place.

This idea is reminiscent of Frege's suggestion that if reference is preserved in conversational exchanges, shifts in sense do not matter. Strictly speaking, of course, if both A and H associate distinct propositions with A's utterance *u*, then Frege would maintain that *u* has communicated no proposition. Thus Frege is making a pragmatic suggestion. However, so understood, Frege's suggestion is false: deviations of sense can result in fluctuations in reference even where the senses in question present the same object. To see this, consider the following. Suppose that H believes that the persons referred to, respectively, as 'Benedictus Spinoza' and 'Baruch Spinoza' are distinct philosophers. He believes that the person referred to as the former wrote the *Ethics* and that the person referred to as the latter wrote the *Tractatus*. (For simplicity, we assume that this exhausts his beliefs about the two.) If A utters 'I've just read Baruch Spinoza's most famous work' he will produce a belief in H about the *Tractatus*, even though he intends to be taken to be referring to the *Ethics*. Thus a deviation in sense (i.e., the sense associated with 'Baruch Spinoza') results

in a deviation in reference (i.e., of the complex expression 'Baruch Spinoza's most famous work').

We must not, then, take Frege at his word: deviations in sense cannot always be tolerated. The question we must answer is when they can be tolerated. My suggestion is that if the mode of presentation, or style, that A intends to refer to is similar to the mode of presentation, or style, that H takes him to be referring to, then A has 'communicated' a belief report to H. One account of similarity of style that suggests itself construes this similarity in terms of acceptance and related notions. For example, we may say that two styles Φ and Φ' are similar to the extent that they both permit acceptance of the same sentences. What does it mean for a style to *permit acceptance of a sentence S*? Let us recall that styles are properties of modes of presentation; modes of presentation are ways of thinking of propositional constituents and thus, determine ways of thinking of complete propositions. For convenience, we use the same expression to designate *modes of presentation* of complete propositions. These modes of presentation consist in the manner in which we grasp information contents, or propositions. This manner of grasping determines what inferences one is likely to make when one takes the grasped proposition to be true--that is, it determines the other beliefs one has *and* how one presents these beliefs to oneself. It also determines what sentences

one is likely to accept when one takes the grasped proposition to be true (assuming that there is some relation between such ways of grasping and linguistic expressions).

Thus we can imagine that A's utterance of:

[8] Hans believes that Kafka wrote *The Trial* would provide information to the effect that Hans grasps the ascribed proposition in a manner that would make it likely that she accepted, among other things, the content sentence, whereas A's utterance of:

[9] Hans believes that Kafka wrote *Der Prozess* while relating Hans to the same content, would provide information that Hans would accept *its* content sentence, but not necessarily the previous one.

The moral is, how one grasps information can be determined (at least in part) by which sentences one accepts--in particular, by those sentences expressing the information in question. (Of course our grasp of the propositions we believe is determined by other beliefs we have. For example, how I grasp the proposition that Kafka is a novelist is surely conditioned by other beliefs I have [that Kafka wrote *The Trial*, that *the Trial* is a novel, etc.]. But the fact that it is not simply the other propositions I believe that determine my manner of grasping that Kafka is a novelist but *how I grasp those propositions* makes it impossible to provide even a simple example showing how this grasping is determined. In a word, the grasping

relation is holistic: it is held in place by one's entire network of beliefs and graspings.)

Let us then apply the account that takes what mode of presentation one grasps a proposition under to be a function of what sentences (expressing that proposition) one accepts to a simple case. Assume that S = 'Benedictus Spinoza wrote the *Ethics*,' and R = 'Baruch Spinoza wrote the *Ethics*.' Suppose now that A has learned of H's mistaken belief, and that he begins to doubt his own belief that there was only one major philosopher named 'Spinoza'. In time, he convinces himself that he was mistaken and that there were two such Spinozas, but, unlike H, A accepts R and not S . Suppose that A convinces some graduate students of his beliefs. To make conversation, he goes on to suggest that there is controversy even among those who hold a dual-Spinoza theory. He utters assertively to the group:

[10] For example, H believes that *Baruch* Spinoza wrote the *Ethics*.

(We recall that H does *not* accept this sentence, but rather accepts S .) This utterance, we may suppose, raises to salience a style, Φ , which is a property of a mode of presentation m only if the actually prevailing cognitive environment contains the information that the sentence 'Baruch Spinoza = Benedictus Spinoza' is true. And part of the cognitive environment is that this sentence is false. A has asserted that H believes that R under a style Φ , a style which applies to ways of thinking of $[R]$ only when they are

disposed to issue in tokenings of sentences such as:
'Baruch Spinoza is Benedictus Spinoza,' 'Only one great philosopher of the early modern period was named *Spinoza*,' 'One person wrote both the *Ethics* and the *Tractatus*,' etc. And this assertion is of course false: H doesn't believe the ascribed proposition under such a mode of presentation.

One point should be made in anticipation of certain criticisms. The response might be made that my rather tolerant stance concerning communication might serve Frege's account as well as my own. That is, one might simply agree with me that Frege was confused about communication, making the standards too rigorous in the ideal case (speaker and hearer grasping the same proposition) and too lax in the case of actual linguistic interaction (speaker and hearer assigning the same senses to predicates, but only the same reference to singular terms). But, the response suggests, given a looser notion of what it is to communicate-- assigning *similar* senses to identical singular terms-- Fregeanism becomes an attractive option. After all if, as I am suggesting, it is enough for communication to have occurred that my audience associates a style similar to the one I associate with my belief-report utterance, why can't the Fregean go on to say it is enough if the audience associates a sense to (embedded singular term) *t* similar to the one I associate with *t*? If similarity of style is

sufficient for communication, why not opt for similarity of sense?

The response questions the motivation for the hidden-indexical theory once it helps itself to the definition of (belief report) communication in terms of similarity of style. The rejoinder to this response is straightforward. First, the final objection to Fregeanism, stated in 1.1, still applies: [4] can be true even in cases where those who have met Al Sharpton do not possess similar ways of thinking of him.³¹ Second, the Richard case (mentioned in 1.1) is not dealt with adequately by the Fregean. Note that while [3] is true at some context, according to HI, the context would be rather special; it would be a context in which (following Boër and Lycan) semantical role rather than conceptual role is salient.³²

[3] Everyone who believes that Hesperus = Hesperus believes that Hesperus = Phosphorus.

That is, the speaker would be intending to say something with [3] that was determined simply by her intentions and by the available resources for realizing these intentions. In contrast, our neo-Fregean, who maintains that that-clause reference is to mode-of-presentation-containing propositions relative to the ascriber's idiolect and that such reference is determined (at least in part) by contextual factors, is forced to assign the same semantic value to [3], no matter what the context, as long as the speaker is held constant.³³

Put another way, according to HI, the proposition expressed by a belief report u is a function of the speaker, A , the time, t , and the utterance u -- $\mu(A, t, u)$. If we vary, for example, t , we may get a different proposition. But notice that, for the Fregean, the time parameter is irrelevant: the proposition the speaker can intend to mean by [3] is fixed (given specification of the speaker and [3]) once for all. The context enters in to determine reference, but either reference succeeds, or it does not (that is, when the context is insufficient to secure reference); the context cannot make the that-clauses in [3] refer to anything but what they refer to for the speaker.³⁴ Thus we see that the features distinguishing our neo-Fregean from his dialectical predecessors (context-sensitivity of that-clause reference and speaker-relativity of the same) do not sit comfortably together; for the speaker-relativity renders the context-sensitivity all but otiose.³⁵

5.3 The Pure Hidden-Indexical Theory.

The final defense does not engage in the burden-of-proof shifting of 5.1. Nor does it claim, following 5.2, that the hearer can conceivably fail to identify precisely which style the speaker referred to without the exchange failing to be a successful communicative encounter. Rather, it maintains that in every utterance of a belief-report, where the utterance is assumed to possess determinate content, the

cognitive environment is in fact sufficiently rich to select a unique style, constraining the mode of presentation quantified over.

The defense acknowledges that the multiplicity problem is a real problem and that it must be met without altering any central semantic assumptions. What is required is an account of how a context can select--make mutually manifest--a style. It attempts to meet this requirement by observing that reference to particulars or properties does not presuppose, on the part of the speaker, individuating knowledge of the particular or property in question.

The previous section provides us with some clues as to how the selection might take place. It was there argued that mode-of-presentation properties could be derived from facts about acceptance. But it was also allowed that if the properties separately fixed on by speaker and hearer failed to match, communication could still occur. The current strategy is not similarly tolerant about communication: it attempts to explain how it is possible that in all cases of belief-reporting the speaker can make manifest to her audience her intention to refer to Φ .³⁶

Consider the account provided above. The mode of presentation under which x grasps p partly determines (and is partly determined by) (a) which other propositions x believes; (b) which modes x believes them under; (c) which sentences x accepts. (Of course, evidentially, the

determination goes the other way around. That is, we select modes of presentations, or styles, by consulting (a) - (c).) For example, given that Lois doesn't believe that Superman writes for the Daily Planet under a relevant superhero-involving mode, not only would she fail to accept the content sentence, she would also fail to accept: 'Superman is on the Daily Planet payroll,' 'Someone on the Daily Planet can bend steel with his bare hands,' etc. Contrapositively, if she accepted any of the latter sentences, we would possess evidence that she believed the proposition in question in a superhero-involving manner. Perhaps, as I have suggested, our best access to the modes of presentation involved in a given individual's belief are the sentences she accepts. But access is available through other forms of evidence. Crimmins (1992: 79), for example, holds that our access to modes of presentation is diverse (what he says about notions applies to modes of presentation of particulars):

A notion can be described by enumerating a group of beliefs it is involved in, or by specifying when it was formed, or by noting any of the following: the individual it is of, the actions it helped to motivate, the natural language expressions and modes of recognition tied to it, and so on.

He defends the view that notions are concrete, non-shareable representational particulars. In addition, he wants to deny that they have structural complexity. Thus, for instance, notions cannot be captured by definite descriptions: that

would violate both the shareability requirement (both I and my audience can be in representational states whose content is the same definite description) and the simplicity requirement (such representations can be analyzed in terms of a complex structural description and are thus not structurally simple). We derive the relevant notions from information about the agent's history, her behavior, and her speech.

Crimmins does not say very much about how we can have access to such notions, although he does say that "on a liberal view of content, all we need in order to be able to think about another agent's representation is solid evidence that it exists" (92). It is undeniable that such solid evidence is provided by an individual's behavior, speech, etc. For example: Jones asks Smith if he wants a beverage; Smith responds: "yes"; Jones leaves the room, returning with a glass of cola; Jones hands the glass to Smith. In such a case, we have evidence that Jones has a notion of Smith, a notion that motivated his leaving the room to get a drink and which enabled him to re-identify Smith upon re-entering the room.

Another example: I open Montaigne's *Essays* at random and I find an essay entitled: "On Cannibals". Therein I find a reference to a King Pyrrhus. I promptly form the opinion that Montaigne had a notion of this individual, and

in addition, that this notion is attached, for him, to the name just employed.

Given that these cases provide me with evidence of notion-existence, and evidence for notion-existence is sufficient for notion-reference, in each case I have sufficient information to make reference to a notion. Yet, I argued above against the neo-Fregean that contexts were typically too informationally impoverished to allow reference to particular modes of presentation. What Crimmins wants to argue, it seems to me, is that whereas these contexts are too impoverished to provide us with information enabling us to specify precisely what the notion or mode of presentation is under which the ascribed proposition is believed, they are not so impoverished as to prohibit us from *referring* to that notion.³⁷

Let us see how plausible this is. Recall our example 'Wagner believed that Ludwig was mad.' I argued against the Fregean that no context could be sufficiently rich in information to provide Wagner's mode of presentation (or notion) of King Ludwig. (This holds at least for those of us who are not students of nineteenth-century German history.) Nonetheless, such assertions are made frequently. I know nothing of the particular notion employed in Wagner's 'cognitive economy' representing Ludwig: that is to say, I have no individuating knowledge of such a notion. But Crimmins responds that I do have enough knowledge to make

reference to such a notion. I know that Wagner had such a notion, after all, and that should be sufficient. (I will consider the objection from the theory of reference below that perhaps such knowledge is insufficient. Also, I will consider the objection that even if we allow non-conceptual [i.e., non-descriptive] access to modes of presentation we must at least have causal access to such modes of presentation. That is to say, I must be in a given causal relation to Wagner to enable me to refer to his way of thinking of Ludwig. But it is not at all obvious that I must be in such a relation to the relevant way of thinking in order to make reports like the above. All this will be considered below.)

Clearly in such a context the attribution of modes of presentation does absolutely no work: it can only do work if (1) there are multiple notions of Ludwig in Wagner's system of representation, and (2) my means of referring to Ludwig (in the context of my ascriptions of belief to Wagner) can make evident which notion I intend to pick out. If Wagner has only one such notion, or if he is in possession of distinct notions but I am not in a position to distinguish among them, then nothing is gained by referring to them. Nonetheless, Crimmins' point, if of limited usefulness, is valid: it doesn't take much to refer to notions, just as it doesn't take much to refer to Wagner's mother, or his heart.³⁸ In both cases, knowledge of their

existence is sufficient. However, I don't think these observations can motivate an account (which Crimmins favors) according to which reference is made to a particular mode of presentation. Since persons often have multiple notions of a given object, reference cannot be secured simply by thinking of the relevant notion as 'so-and-so's notion of x', as this description is ambiguous. The more we know about an individual the more likely it is that we can make reference to her notions, but in cases where we have evidence only that the individual in question has at least one notion of an object, reference to a particular one is not possible. Put another way: evidence that there exists for Wagner a notion of Ludwig is insufficient to allow reference to it, since there may be more than one such notion. Consider the analogy just given. I can make reference to Wagner's mother. I can wonder, for example, if she lived to see any of his operas performed. Yet I cannot make reference to Wagner's brother simply given evidence that a brother exists, since 'brother of x' doesn't always refer to or denote a single person. Similarly with modes of presentation. The description 'Wagner's mode of presentation of Ludwig' is not necessarily uniquely instantiated; thus I am in no position to make reference to it in this manner if all I know is that at least one such mode of presentation exists.

Yet, HI is unaffected by this outcome, for, in reporting this belief the possibility remains that I constrain the mode of presentation without making reference to it. In the case of 'Wagner believed that Ludwig was mad,' my report implicitly specifies a style, to the effect that the mode of presentation is one that presents Ludwig to Wagner. So far Crimmins' suggestion has not failed us: reference to modes (or, at least, styles) is easy, even if individuating them is not. Communication succeeds if we simply *identify* the semantic content of the utterances of our fellow speakers; there seems to be no additional requirement to be able to provide (even in principle) a characterization of that content that would enable one to distinguish it from other contents.

Yet, Crimmins' suggestion seems to be imperiled by the multiplicity problem. Consider again the objection: in many cases there are simply too many styles available for my utterance to single one out. The response that in uttering [5] Harold simply refers unproblematically to a style constraining Jane's notion of Julia Kristeva seems a bit sanguine.

[5] Jane believes that Julia Kristeva is a genius.
But let us make clear that such reference is conversation-relative. In such a context the level of specificity will have to be determined by the conversational interests of the participants. Consider the following conversational

context. A utters assertively, 'Clinton believes that Maya Angelou is a poet.' His audience knows nothing more about A's knowledge of Clinton's knowledge of Maya Angelou than is made manifest by the utterance, which assumes that the audience knows, at least, who Maya Angelou is. The notion referred to in A's utterance might be specified by the expression 'Clinton's notion of Maya Angelou' or 'the notion Clinton associates with the name *Maya Angelou*'. If asked to decide between the two, the response could even be made that the condition that the style places on the notion is disjunctive. But let us not lose sight of the main point: conversational interests determine which style is relevant. The speaker sets up certain expectations (in the above context he assumes that the audience knows who Maya Angelou is); these expectations are mutually manifest. In a more complex case, the utterance might carry a greater number of expectations. For example: Jones makes the same assertion to an acquaintance who knows a great deal about Clinton and his reasons for selecting Angelou to speak at his inauguration. Yet only to the extent that it is mutually manifest that Jones and his acquaintance share this knowledge about Clinton can Jones's knowledge allow him to refer to a more complex style--one that applies to notions just in case they are associated with certain kinds of evidence provided by Clinton's behavior or speech.

The moral is the following: *only what is part of the cognitive environment is permissible as a basis for securing reference.*³⁹ I can only expect to make my reference manifest--and thus truly refer, according to the Gricean--if my audience has the same resources as I do for securing reference. Thus, I cannot refer to Maya Angelou's worst book (at least not with that expression) to an audience not acquainted with Maya Angelou, or to 'that inferior remake of *Breathless*' to an audience not acquainted with it or with the original. (Again, I am assuming a notion of *refers* relevant to communication; if one insists that reference, to succeed, does not have to be manifest, I can only respond that that is not the notion of reference currently relevant.) If this is the case then conversational expectations severely constrain the styles that are available.

I write "severely constrain", but what we need is for styles to be uniquely constrained. In cases where we are faced with multiple styles--where conversational interests fail to select a unique style--it would seem that the style is in fact disjunctive. This disjunction, moreover, would be itself part of the cognitive environment. For example, I and my audience, H, know of a variety of ways under which A thinks about B. Of course, there are ways of thinking about B that I possess that I am not sure H possesses, and vice versa. Thus, these ways are not part of the cognitive

environment. What remains may well be quite varied. No gesture of mine could single out which style I had in mind. Yet, since each style is mutually accessible, it seems there is no harm in supposing that I am making implicit reference to a disjunction of styles. Indeed, this seems to be the only plausible account of style reference in a context in which the participants have (and are mutually known to have) considerable knowledge concerning the referent in question.

But a suggestion of Brian Loar shows this tack to be incorrect. Assume that it is part of the cognitive environment, between H and myself, that a specification of who I intended to refer to in a particular dated utterance of 'He missed the bus' would either be 'the man who addressed you in French' or 'the man who needed change for the bus'. Now assume that it emerges that no one *both* addressed H in French and needed change for the bus. It would be implausible to suppose that, upon being informed of this circumstance, I would take myself to have referred to one or the other of the denoted individuals. But this is just what the disjunctive analysis entails. The correct response seems to be that, on reflection, the pronoun in the sentence referred to nothing.

Thus Loar:

If ... someone addresses us, and I then say 'He looked familiar', I may not be prepared to reject either 'the person who just appeared to speak to us', or 'the person we just saw in such and such place', as essential to what I meant. That in such a case it is their *conjunction* which is the

essential individual concept is made plausible by the fact that, if through some bizarre contingency those individual concepts ... are discovered not to be conjointly instanced, then one will be inclined to think that there is nothing which clearly was one's referent (Loar, 1976: 361).

Consider another example. When I assert the following:

[11] Quine believes that Carnap was a great philosopher.
in the context of an audience, H, who is a professional philosopher and who knows that I am a student of philosophy, but who doesn't know the extent of my knowledge concerning Quine's relation to Carnap (beyond the minimal knowledge that the utterance implies that I have, that I expect H to assume that I have, and that I expect H to have), a certain range of mode-of-presentation properties suggest themselves. They suggest themselves because they place conditions on ways Quine thought about Carnap that I could conceivably have attempted to convey to my audience. Clearly, the more we have in common (and know each other to have in common) with respect to this subject matter, the less manageable the styles will be: I will be unable, simply in uttering what I did, to get H to fix on a particular style. But this is hardly disastrous: given the array of possibly relevant mode-of-presentation properties, I can hardly be expected to refer to any particular style. Rather, I will be taken as referring to a conjunction of styles--the minimal conjunction, made salient by my utterance, of appropriate constraints on how Quine believes that Carnap was a great philosopher.

But this invites the following sort of criticism. 'How can it be claimed that I referred to a conjunction of styles when I had no conscious knowledge of doing so? Presumably, what a speaker, A, means in uttering 'S' is transparent to A. Accordingly, if A intends to convey p in uttering 'S', this knowledge will be accessible to her. But, in general, a speaker will not recognize herself to have referred to a conjunction of styles when she utters, for example, [5]. Thus, your conjecture doesn't really harmonize with the linguistic data.' Yet, in the passage just quoted, Loar provides the basis of a response to this question. He suggests that, while meaning facts are accessible to the speaker, they are not *immediately accessible, or transparent, to the speaker*. That is, to be essential to what A meant in uttering u is not necessarily to be (a component of) A's explicit intention in uttering u. As Loar suggests, determining what is essential to what one meant might require entertaining "bizarre contingencies"--various changes in background knowledge. This will enable the speaker to determine whether or not what she intended to convey is immune to such shifts. There is intuitive support for Loar's claim. Consider again the example of an utterance of 'He missed the bus.' In this case, a semantically relevant contingency might consist in the discovery that an assumed identity claim turns out to be false. 'the man who addressed you in French' turns out not

to be 'the man who needed change for the bus', with the result that the utterance is meaningless. This seems to show that a belief can be essential to what is meant and it fail to be transparent to the speaker that the belief is thus essential.

Similarly, in the example involving the belief report, [5], the speaker might reflect on whether her speech act would have been successful if it emerged that her background belief about the *(mutual belief)* that Carnap was a member of the Vienna Circle turned out to be false. Perhaps a style that the actual utterance made salient would not have been made salient in the counterfactual context, with the result that the counterfactual utterance would have been either meaningless or false. If so, then it seems that the belief was essential to what she meant.

In sum: I have argued that if we take seriously the idea that communication proceeds by making one's intentions manifest, then the multiplicity problem seems to be less fatal than expected. For if reference must proceed in accordance with conversational expectations, then the class of styles I can refer to in any given conversational transaction is limited. It is limited by what is mutually manifest between myself and my interlocutor. Thus I agree with Crimmins that how we make reference to such styles is not deeply problematic. Although I do emphasize that such reference has to be understood in the context of explicit

conversational interests. In addition, I suggest a conjunctive account in certain contexts: Harold, in uttering [5], might have in mind that H think of (say) Julia Kristeva according to the set of mode-of-presentation-types $\{\Phi, \Phi'\}$. The style thus made salient is a conjunction of Φ and Φ' . For, intuitively, if no mode of presentation was both Φ and Φ' then Harold would not have expressed anything by his utterance.

Whether or not the case of incomplete descriptions is analogous on this score is unclear. I am inclined to think that similar considerations might resolve *that* problem. It might be argued that, unlike the current case, conversational expectations cannot typically be relied on to select completions of incomplete descriptions. But I am not so sure that this is true. Moreover, the conjunctive hypothesis can surely be put to work here. As Loar's discussion suggests, the completions of incomplete descriptions are typically implicit and complex. The test for their being conjunctive--containing the range of mutually manifest completions--is, as we have seen, straightforward: assume a counterfactual in which at least two candidate (mutually manifest) completions of the same definite description D fail to be satisfied by the same object. The intuition is strong that the speaker could not simply select one candidate completion and maintain that *that* is what, in fact, he meant.

In any event, I do not feel that it is a requirement, in resolving the multiplicity problem, to show how it is not analogous to the problem of incomplete definite descriptions. I will, however, pursue a solution along the lines just suggested in Chapter Two.

I cited two objections above which I would like to address now. The first concerns reference in the absence of individuating knowledge. The claim that one can refer despite being unable to distinguish the actual referent from, say, a duplicate, is based on solely on intuition. Nonetheless, the intuition is prevalent and the view has few objectors. So I think it is safe to assume it. I bring it up to make it explicit, but also to introduce the next objection, which worries whether reference can be made in the absence of a causal chain of some sort. I will label this the "objection from causal isolation."

If I am acquainted not only with Jones but, say, with how he refers to Clinton, his behavior in seeing Clinton on television or in a newspaper photograph, and his history of exposure to Clinton, then I know of the mode of presentation of Clinton that Jones possesses. To individuate it would involve specifying its origin, its role in various beliefs and desires that Jones has (and thus in the implementation of Jones' behavior), and its relation to certain verbal signs produced by Jones (to, for instance, 'Hilary's

Husband', 'Mr. Clinton', 'Bill Clinton', 'The President', etc.).⁴⁰ My ability to refer to the notion is not ruled out by my inability to come up with a specification either of the particular notion in question, or style partially characterizing this notion.

But now let us consider the ascription of a belief to an individual who I am causally related to in the appropriate manner, but who I have never observed and about whom I have little factual information. How can I refer to his way of thinking of a given particular if I have no direct or indirect contact of the appropriate kind to it?

What, we might ask in response, is contact of the appropriate kind? To give a positive account is impossible, but there are clear cases of contact and absence of contact. And it seems however lax we are about which conditions have to be met for *x* to be appropriately related to *y*, we can imagine someone successfully ascribing a belief where it is clear that he could in no way satisfy the conditions enabling him to make reference to any of the agent's relevant modes. That is, the objection has it that in many cases in which I can ascribe belief it is apparent that I do not bear the relevant relation to the subject's way of thinking of the ascribed proposition.

I do admit (at least for the sake of argument) that there is a causal gap between my tokening of 'Wagner believed that Ludwig was mad' and Wagner's notion of Ludwig,

that does not hold between that same utterance and the individuals Wagner and King Ludwig, respectively. In the latter cases a causal/historical chain is unmistakably present. But it strikes me that reference doesn't require contact of this sort. For example, I can refer to Ockham's birthday, even though no one knows when it was (or for that matter, the year that Ockham was born). If temporal points are not happy examples of references, take Ockham's birthplace or Ockham's mother. I can refer to all of these. If one quibbles that I am not really referring in such cases then, rather than choose a new term for the semantic relation I have in mind, allow me to extend it. The point is, in such cases the semantic value of the expression 'Ockham's birthplace' is simply the object satisfying the description.

There might be resistance to this point. One might argue that there are possible worlds at which Ockham was born (e.g.) in Venice. Thus the description 'Ockham's birthplace' picks out, in those worlds, a city distinct from its actual-world denotation. Yet, referring expressions have constant denotation--they are rigid. Thus, descriptions cannot provide the correct analysis of reference. And consequently, the capacity to fully understand a description--linguistic competence, causal relations of the appropriate kind to its constituents--does

not in itself entail the capacity to refer to any object, including the description's actual world denotation.

But the argument in this paragraph involves a proprietary use of 'refers', a use which it might be best to abandon. Rorty (1979) suggests that we reformulate the Kripke-Donnellan counterexamples to the description theory in terms of the theory-neutral 'talking about'. This takes the sting out of Kripke's well-known Gödel-Schmidt example and others like it. The idea was that if we propose that to refer to Gödel is to think of him under the uniquely satisfied condition 'the F' then we must really be referring to the F--who, as it happens, is Schmidt and not Gödel. Kripke concludes that, nonetheless, we are really referring to Gödel in our 'Gödel'-involving discourse. Well, perhaps. But this may simply be stipulative. Were we, on the other hand, *talking about* Schmidt, the F, or Gödel in our 'Gödel'-involving discourse? No answer, it seems, can be given that applies to all contexts. Consider: A Mathematician first hears of Gödel's theorem and says to a friend, 'This Gödel must be a remarkably bright fellow.' Was the mathematician talking about *whoever it was that derived the theorem*, or Schmidt or Gödel? Although the example may suggest even the former, it seems clear that what someone meant in such a context must be a matter of interpretation. Intuition alone cannot tell us that the mathematician's communicative intention was to produce a belief in the mind of his hearer

that the proposition <Gödel, the property of being remarkably intelligent> is true, as opposed to the relevant descriptive proposition: <The F, the property of being remarkably intelligent>.

The upshot is that the claim that one cannot refer to a mode of presentation if that reference is secured by a definite description and not by causal contact of the appropriate sort is unjustifiably restrictive. Intuitively, one can refer to (that is, 'talk about') such causally isolated objects. Again: reference is a matter of interpretation, and it is not settled in advance that the interpretation will conform to the causal/historical account's predications.

In sum, we use relative clauses unproblematically to 'refer' to things that we couldn't refer to independently.⁴¹ In particular, we can refer to an individual's way of thinking about a particular even in the absence of the relevant causal relation between our act of referring and the way of thinking in question. The proviso is that we must know that the relatum is unique. Otherwise, we can still refer, not to a notion, but to a style. In the above example, the style might simply be 'x is one of Wagner's ways of thinking about Ludwig'.⁴²

Thus, the objection from causal isolation has been answered. It seems then, that reference to styles--and, in some cases, particular notions--is not significantly

problematic, at least when freed from the requirement of individuating knowledge. But even if causal isolation is a barrier to referring to notions, it provides no barrier to talking about (i.e., denoting or describing) notions and no barrier whatsoever to referring to styles. Styles are, after all, properties; thus causal contact as a prerequisite for referring to, or talking about, them is inapplicable.

The next section develops an alternative account: that reference to styles is indeterminate, or vague.

6. The Indeterminacy of Belief-Reports

Vagueness is often taken to be a defect of natural language. Ideal languages are languages that assign determinate extensions to each predicate, resulting in the assignment of definite truth values (*true* or *false*) to the sentences of the language. Yet the requirement of definiteness seems out of place for most communicative purposes: temporal, spatial and phenomenological information transmission is normally imprecise, and yet the imprecision is not typically taken to be a defect of language or language-use, but, if anything, an inevitable concomitant. The current strategy takes belief reports to possess the characteristic feature of vague expressions--meaning deficiency. Yet paradigm cases of vagueness are size or color concepts: concepts which admit of precisification and whose use does not in itself

specify the appropriate level of precisification. But I think there is a clear sense in which vagueness can be said to characterize our belief discourse. (I am indebted to Stephen Schiffer for this suggestion.) Let me first, however, describe the paradigm cases before arguing for a parallel account of belief reports.

My assertion that I wish to purchase white paint does not indicate to the hearer just which shade of white I have in mind: perhaps my intention is to refer to anything within a given range; on the other hand perhaps my intention is to refer to a particular shade. Kit Fine's account of vagueness begins with the observation that the meaning-extension involved in precisification preserves truth-value.⁴³ As Fine writes:

A sentence does not become indefinite [neither true nor false] upon being made more precise. This is, perhaps, definitional of 'making more precise'. For what distinguishes this operation from a mere change in meaning is that it preserves truth-value (Fine, 1975: 275).

The claim that vague sentences are sentences whose meanings have the potential for being extended is intuitively correct, as is the requirement that the extension preserve truth-value. From here it is a short step to the following claim: a vague sentence is true iff all its precisifications are true; false iff all its precisifications are false; otherwise it is indefinite. As an example take the sentence:

His walls are blue.

An utterance of this is indefinite if the context is a wall whose color borders on green and blue. Certain precisifications of the sentence may preserve truth-value ('His walls are turquoise'), but certain precisifications will fail to ('His walls are navy blue'). Thus the original sentence is indefinite. Yet, the following comes out true no matter how the precisification is made:

His walls are either blue or they are not blue.

Fine's account is a "super-truth" account. A sentence is super-true just in case it is true for all valuations (or all extensions of its meaning), super-false just in case it is false for all valuations (or extensions); it is indefinite otherwise. Fine thus equates the truth of a vague statement with its super-truth--its truth at all admissible precisifications. Consequently vague statements are often indefinite, or "gappy".

Let us now relate this idea to our discussion of belief, where what is at issue is the choice between referents (that is, styles) each of which would make the sentence as a whole come out true. Belief reports are vague, we might say, since their meanings (taken at a context) do not determine which propositions they express. But the notion of precisification seems at first blush inapplicable. What we have in the case of belief reports is a linguistic meaning that can be interpreted in a variety of ways (as specifying, at a context C, one of a number of

salient styles). Perhaps we should say that taking the utterance of a belief report to make reference to style Φ as opposed to Φ' is a precisification. In fact, there seems no good reason for not saying exactly this.⁴⁴

Let's assume with HI that for every context C at which a belief report of the form 'A believes that S' is produced there exists a non-empty set of styles which are mutually manifest between speaker and audience. The speaker is aware that his utterance would raise to salience styles $\Phi_1, \dots, \Phi_n$. He gives no indication to his audience that any one among these styles is to be dropped from consideration. Yet he selects no one style.

Obviously the choice of any one style, say Φ_j , as capturing what the speaker meant would be arbitrary. His actions (given the context) allow us to consider the style Φ_j a potential choice, but he does not indicate that this, rather than another, is to be the final choice. Yet, the fact that any choice would constitute a successful communicative transaction suggests that our semantics for belief reports--in particular, our assignment of truth conditions to token reports--does not require us to make such (admittedly arbitrary) choices. The super-truth account [ST] provides an alternative.

[ST] An utterance of the form 'A believes that S'
is true just in case the schema:

$\exists x(\Phi x \ \& \ B(A, [S], x))$ is true for any selection of an admissible candidate style Φ_i ; false just in case it is false for any selection of a candidate style Φ_i ; indefinite otherwise.⁴⁵

The difference between this view and HI should be obvious: there is no requirement of determinate reference.

One might think that little is gained by incorporating arbitrariness into the semantics of belief reports, maintaining that questions of objective reference and meaning should be settled before attempting a formal reconstruction. Yet considering David Lewis's (1973) analysis of counterfactuals may help to dispel this idea. Lewis provides a semantics for counterfactuals, one which assigns truth conditions to sentences of the form 'If A were the case then B would have been the case' (schematically ' $A > B$ '). The truth conditions are as follows.

' $A > B$ ' is true at w iff:

' B ' is true at the closest A-world accessible from w .

(I assume that the notion of accessibility is sufficiently familiar.) Lewis rejects this account (in essentials due to Stalnaker), as he deems it implausible that there is a unique closest A-world. Take the sentences:

- [12a] If Verdi and Bizet were compatriots, Verdi would be French.
- [12b] If Verdi and Bizet were compatriots, Bizet would be Italian.

For Stalnaker, there is a closest world w' at which Verdi and Bizet are compatriots; either 'Verdi is Italian' or 'Bizet is French' would be true at this world. Now it is desirable to have [12c] come out true, given that we know that one of [12a/b] is true.

[12c] If Verdi and Bizet were compatriots, either Verdi would be French or Bizet would be Italian.

But there seems no principled choice between [12a] and [12b]. Moreover, it seems fantastic to think there is a fact of the matter concerning the closest antecedent-world.

All of this is hard to believe for Lewis as well. What he suggests is something like the super-truth account. A sentence ' $A > B$ ' is (super) true iff ' B ' is true at any arbitrarily selected accessible A-world. It is (super) false iff ' B ' is false at any arbitrarily selected A-world. It is indefinite otherwise. This allows Lewis a semantics that handles sentences that intuitively should possess a definite truth-value while refraining from making arbitrary decisions (or, worse, assuming that there are facts about comparative similarity of possible worlds which will make the decision). For example [12a] and [12b] are indefinite, but [12c] is super-true--true for any choice of closest accessible antecedent-world. Whether this is how things should be is not relevant here: my only purpose in discussing this example is to show that recognizing arbitrariness in the evaluation of certain sentences does not rule out the possibility of assigning the intuitively

correct truth-values to compounds of such sentences. Again, we may be cautious about assigning definite values to certain counterfactuals, believing there to be no modal fact of the matter deciding their truth. Yet this need not prohibit us from assigning definite truth-values to those sentences unaffected by how the decision is made.

As we have seen, my proposal concerning belief-report tokens adopts a similar strategy: An utterance of a sentence:

[13] A believes that S

is (super) true iff; for any admissible style ϕ_i raised to salience by the production of [13], the sentence

[14] $\exists x(\phi_i x \ \& \ B(A, [S], x)$

is true. Just as it would be arbitrary to maintain that 'A > (B v C)' is true because there is a closest accessible A-world w' and exactly one of 'B' and 'C' is true at w', so it is arbitrary to maintain that all utterances of the form 'A believes that S' are true because the utterance renders manifest precisely one style that provides an appropriate constraint on how A presents [S] to himself. (I do not deny, of course, that at least some utterances of the form 'A believes that S' do succeed in making determinate style-reference.)

The basic idea is that it is not the business of semantics to worry how the reference to a style is accomplished. It is enough to know that, even in the

absence of a semantic fact of the matter, a definite (super) truth-value can be assigned to the relevant report. As for the particular precisifications--the various propositions of the form [14]--these are all indefinite. What we may take to be definite is the claim that one or another of the styles in question (but no *particular* style) is in fact the one that provides the appropriate constraint on how A entertains the proposition that S.

A problem for the super-truth view arises with multiple embeddings of propositional attitude verbs. Let us take the simplest case, involving the 'says that' relation. Assume that 'says that' is binary, holding between speakers and structured propositions. Now take the following attribution:

[15] Jimmy Olsen said that Lois believes that Kent flies. The super-truth account fails here, for, according to it, there is no proposition: *that Lois believes that Kent flies*. We can assign truth conditions to a tokening of the sentential clause 'Lois believes that Kent flies', but we cannot (according to [ST]) assign it a determinate proposition. Thus, we cannot assign [15] truth conditions nor, apparently, super-truth conditions. If this is so, then an important component of HI--that that-clauses are semantical singular terms--is contradicted. For the that-

clause with widest scope in [15] fails of determinate reference.⁴⁶

The situation admits of an easy remedy, although it is up to the reader to judge how satisfying it is. The remedy consists in showing that we can indeed provide sentences of the form exemplified by [15] with super-truth conditions. (Truth conditions are of course out of the question, since an ostensibly referential singular term in [15] fails of reference.) First, we assume that precisifications of belief-reports are propositions. Second, we adopt the following belief-report clause:

[C] If 'S' is a belief-report, then 'A says that S' is super-true iff, for any arbitrarily-selected precisification of S, 'A says that S' is true.

For example, [15] is super-true just in case any precisification of 'Lois believes that Kent flies' is such that it makes [15] true. A precisification of this latter sentence is a proposition of the form:

[16] $\langle \langle \langle \phi, \lambda yB(\text{Lois}, [\text{Kent flies}], y) \rangle, \text{CONJ} \rangle, \text{SOME} \rangle$

(Where ' λxFx ' is a singular term referring to the property of being F.) Now take the set of precisifications of [16]--the set of propositions determined by replacing ϕ in [16] with a candidate style. If for each arbitrarily selected precisification:

[16'] $\langle \langle \langle \phi_i, \lambda yB(\text{Lois}, [\text{Kent flies}], y) \rangle, \text{CONJ} \rangle, \text{SOME} \rangle$

the following is true:

[17] SAYS(Jimmy Olsen, <<< Φ , $\lambda yB(\text{Lois}, [\text{Kent flies}],$
 $y)>$, CONJ>, SOME>

then [15] is super-true.

In sum:

A said that C believes that S
is assigned truth conditions as follows: it is super-true
just in case it is true for all precisifications of C
believes that S; super-false just in case it is false for
all precisifications of C believes that S; indefinite
otherwise.⁴⁷

Of course this doesn't resolve all our worries: it
apparently follows from [15] that Jimmy Olsen said
something. That is:

[18] $\exists x$ Jimmy Olsen said x

Yet, since the that-clause in [15] fails to refer, the
position it occupies cannot be bound by an external
objectual quantifier. Thus, it seems, [18] is not a logical
consequence of [15] on super-truth account.

But this conclusion is misleading. Our concern should
not be with the consequence relation as such, but the super-
consequence relation:

Q is a *super consequence* of P iff Q is a consequence of
each precisification of P .

Clearly, if [15] is super-true, then [18] will be a super-
consequence of [15].⁴⁸

Conclusion.

I have provided four separate defenses of the hidden-indexical theory. The first three assume the semantic determinacy of belief-reports, the last employs a framework that tolerates indeterminacy--the super-truth account. The response I favor is philosophically the most interesting: that reference can be made to styles, and that this reference can be manifest to both speaker and hearer. The strongest response is, of course, the super-truth account. It defuses the multiplicity problem directly, arguing that the conclusion that no style at a given context can be non-arbitrarily selected does not provide an obstacle to a general semantical treatment. The objector who urges the multiplicity problem is logically committed to the super-truth account. The question is whether she wishes to identify the super-truth of belief reports with their actual truth. If other accounts are available, then super-truth is obviously unattractive. But it is nice to know that if the prospects for a semantical account of belief reports look bleak, the super-truth account is available--if, that is, we don't mind substituting super-truth conditions for truth conditions.

Notes to Chapter One.

1. I proceed in this order because I think DR is best understood as a response to a Fregean account. It strikes me as disingenuous to maintain that DR is a 'naive' approach to semantics, and that more 'loaded' approaches are motivated only if DR fails. (Although to argue that Fregeanism is the intuitively plausible view strikes me as equally wrong.) My reason is that I do not think anyone could have seriously proposed a theory which admits of the substitutivity of co-referential terms without having provided an account of informative identity and opacity. (Thus I do not regard Mill's suggestion that names lack meaning as a serious semantic proposal; at best it is an observation as to how proper nouns differ from common nouns.) My point is that the dialectic could have begun anywhere, with either the Fregean defending himself against an imaginary direct reference theorist (as we find in 'On Sense and Reference') or with a direct reference theorist defending himself against a (real) Fregean (as we find in, say, Soames (1987a)): but in each case the theory being put forward must defend itself against an intuitively plausible competitor.

2. The neo-Fregeans I have in mind are the Kaplan of (1968), Forbes (1987), (1990), Evans (1982), Taschek (1988).

3. This view is entertained (and rejected) in Richard (1990: 66-67, 78-84), and Crimmins (1992: 202). The most popular version is the similarity view: i.e., my utterance of 'A believes that S' is true just in case 'that S' refers to a proposition (or, more naturally, sentence or sentence-analogue) that is *similar* to one that A believes. See also Forbes (1987: 5-6, note 5) and Lycan (1986: 11-15).

4. As Stephen Schiffer has suggested, this view also has the unwelcome consequence that 'that S' is not in the range of the belief relation. Consequently, inferences exemplifying the following form come out invalid:

A believes that S.

Therefore, what A believes is that S.

5. In this context I mean by 'proposition' *mode-of-presentation-containing proposition*.

6. Richard is not oblivious to the irony of this result. After all, it is precisely sentences such as [3] that the Fregean typically claims the Russellians cannot accommodate. Now it turns out that the Fregean cannot quite account for it either. (In this respect Richard's example is much like Kripke's Pierre case.) As Richard writes, "[the anti-

substitutivity intuitions] do not suffice to motivate a theory like Frege's. Indeed, they actually seem to *undermine* such a theory" (83, emphasis in text).

7. There are other unattractive features of neo-Fregeanism which, while not relevant to my line of criticism, are nonetheless significant. For example, Segal (1988) observes that sentences of the following form are not given their intuitively correct reading on the neo-Fregean analysis:

Jones believes that Smith is tall, but, if anything, he's short.

Intuition suggests that the pronoun here has the same semantic value as its antecedent, 'Smith'. But given that the antecedent is embedded and the pronoun is unembedded, the neo-Fregean is committed to their possessing distinct semantic values, contradicting the intuition.

8. This is in fact the strengthened disquotation principle. Direct reference theorists are not forced to deny the weakened principle [D'] (see Salmon (1986, Chapter 9, and Appendix A); the original source is of course Kripke, (1979, 112-13), where one can also find the strengthened version):

[D'] A is disposed to reflective, sincere assent to 'S' only if A believes that S .

The only rationale for rejecting [D'] is that it allows for the ascription of contradictory beliefs to logically competent speakers (whereas [D] can force belief reporters to assert literal contradictions in ascribing beliefs; consider Jimmy Olsen's asserting the contradictory sentence [2], a situation induced, not by logical incompetence, but by a desire to report the facts as they appeared). For example, if [1a] is true, but [1b'] is also true (as the Superman fiction suggests), then [2'] results:

[1b'] Lois believes that Superman does not write for the Daily Planet.

[2'] Lois believes that Clark Kent writes for the Daily Planet but believes that Superman does not.

Given the resources of the pragmatic strategy (to be stated below), this is not especially troublesome. Moreover, Kripke's Puzzle shows that Fregean theories are not immune to similar results.

9. Nor, for that matter, does Lois' disavowal of [1b] provide evidence for [1b'], according to the strategy.

11. Furthermore, speakers are not necessarily competent to distinguish the pragmatic features of their utterance from the semantic. Thus, one might in fact be convinced that what Jimmy Olsen asserted in producing [1b] was strictly speaking false, since one might have taken the information about how Lois represents the ascribed proposition to herself as part of the propositional content of the utterance, and not as a proposition simply made salient by the utterance.

Of course this has the consequence that speakers do not know exactly what they are intending to communicate in certain contexts, and this conflicts with Gricean accounts of speakers' meaning.

11. In the former case, where it is claimed a token belief report implicates a proposition *p* about a particular mode of presentation, we have a theory that differs from neo-Fregeanism only in its assignments of truth conditions to token belief reports. For, according to both accounts, the context of utterance will always be sufficiently rich to allow specification of a particular mode of presentation. This is implausible for reasons already mentioned: contexts are not typically sufficiently rich to secure reference to modes of presentation.

12. Of course, this raises a problem for psychological laws but not (at least not immediately) for folk-psychological explanation. For the attribution of belief can specify belief states finely enough for the purposes of such explanation. Since this specification operates pragmatically, and not semantically, it is unclear that any covering laws could be formulated consistently with this view.

13. Devitt (1989) argues against the conclusion I have just asserted. See p. 223 and *passim*.

14. Briefly, the strategy requires disquotation, and this principle presupposes the following belief transparency principle:

[BT] A competent speaker *x* of *L* believes that *S*, only if *x* assents to any sentence *S'* of *L* that means in *L* what *S* means in *L*.

Clearly this is false, even if *x* is a fully competent speaker of *L*--that is, even if *x* understands *S*, and every *S'* synonymous with *S* in *L*. Yet if [BT] is false then we have little reason to assume disquotation, for the authority of *x* to specify what he does not believe is undermined. There is no reason to take his word for what he believes.

15. See Charles Chihara, 'The Semantic Paradoxes: A Diagnostic Investigation', *The Philosophical Review* 88 (1979), 590-618.

16. And even this is not a solution for Paderewski/Bugsy-style cases. Johnston considers the response that "Talk of meaning in the sense of the platitudes is talk of reified use" (1988: 37). This meta-platitude might distinguish uses of 'London' and 'Londres', and even distinct uses of 'Paderewski' (i.e., as intending to refer to the statesman, and as intending to refer to the pianist, respectively). But, as Johnston himself acknowledges, this has the non-platitudinous consequence that no two expressions can have the same meaning (where the distinct patterns of application of 'Paderewski' may count as applications of different expressions).

Note that a similarly unfortunate conclusion could be derived even if we restricted ourselves to the weaker principle [D']. Note 8 provides the derivation, which results in Lois's having contradictory beliefs. In both cases platitudes (either [D] or [D']) result in either forcing the (otherwise rational) belief reporter to assert contradictions, or the (otherwise rational) subject to believe a contradiction.

17. This is true as well for some versions of the neo-Fregean view we have been considering.

18. It should be noted that the theory specifies no function from utterances of belief reports to such mode-of-presentation properties, or constraints. Kaplan's Logic of Demonstrative evaluates sentences with respect to parameters other than simply models or possible worlds, for example, relative to spatio-temporal location, agent, audience, etc. One could conceivably extend this treatment to parameters not explicitly specified in the target sentences, for example, the styles implicitly referred to in belief-report utterances. It is not clear, however, that it would be worth the trouble.

19. Mark Crimmins (1992) maintains a version of the hidden-indexical theory, but one on which the hidden indexical refers to a *particular* mode of presentation, and not a mode-of-presentation type; see for example pp. 203-04. (He has since retracted this claim: see Crimmins (1993, 279n).) Instead of adopting the traditional Fregean terminology, Crimmins speaks of *notions*--modes of presentation--and *constraints*--mode-of-presentation properties. Moreover, he claims that in a report of the form 'A believes that S', the mode of presentation implicitly referred to is the one that the subject associates with 'S', not the speaker. I will discuss his proposal below (5.3), to see if it can overcome the obstacles facing the neo-Fregean account.

20. Square brackets designate functions from sentences to the propositions they express.

21. I allude here to the fact that the content of a belief report is a sequentially ordered, or "structured", proposition itself.

22. They may not, however. Depending on contextual features, utterances of these two sentences may in fact express the same propositions.

23. I borrow this expression from Sperber and Wilson (1986: 39), who define a "cognitive environment" as a set of facts manifest to a given thinker at a time. I will sometimes use this expression (as now) as indicating the *mutual* cognitive environment--the set of facts manifest to a group of thinkers at a time.

24. As Kaplan would put it, there *character* underdetermines their *content*.

25. Schiffer himself (1992) makes this point at 503-04. Boër and Lycan (1986, 52-55) provide another way of looking at the matter. They claim that sometimes a belief report ascribes a relation to the content sentence's conceptual role, at other times, to its semantical role (which, for our purposes, amounts to a Russellian proposition). What kind of object the belief report reports a relation to is determined by the conversational interests of the speakers (see especially, p. 55). One way of pursuing this sort of strategy is to view 'believes' as an indexical; for details see Richard (1990), Chapter 3, section 2, esp. 139-40.

26. It might be objected that this theory is not HI, since what HI states is that speakers put substantive conditions on ways of thinking in uttering belief reports. Fair enough: take me to be defending a close relative of HI, HI* say. In any event, HI* still conforms to [HI].

27. I'm assuming for simplicity that neither *u* nor *u'* contain other context-sensitive elements.

28. Again, I am assuming that a variety of situations can raise a single style ϕ to salience. (a) Most candidate styles--styles that are part of the cognitive environment and are potentially referred to by the utterance in question--are compatible with ϕ ; (b) the subset of candidate styles compatible with ϕ is larger than any other subset compatible with an alternative style ϕ' ; (c) the members of the subset X of candidate styles compatible with ϕ have more in common with ϕ than the members of any other subset Y have with an alternative style ϕ' . (Although (c) will probably not raise

a style to salience unless a certain threshold number has been met--i.e., X must comprise a certain portion of the set of candidate styles before the similarities among its members enable this feature to raise a given style to salience).

29. Where by *available style* I mean *candidate style*--a style that is included in the cognitive environment but not necessarily selected as the referent. (Compare the notion of a *candidate completion* of an incomplete definite description--a contextually possible completion that the context does not necessarily select as correct.)

30. Some might part company with me here, arguing that if one's intention to refer to *o* is not recognized, being mistaken (say) for an intention to refer to something else, then one has not, in fact, succeeded in referring to anything. I am assuming it is coherent to say that A referred to *o* in uttering *u* but that H took A to be referring to something else.

31. However, the Fregean can account for inferences such as:

A believes that S

H believes that S.

So, there's something that both A and H believe.

The conclusion is true just in case the respective occurrences of 'that S' are co-referential. But given that the Fregean position developed in 1.1 claims that 'that'-clause reference is context-sensitive, and given that it is only fair to evaluate the two premises relative to the same context, the argument is valid on the (preferred) Fregean analysis: there is no context C under which the premises are true and the conclusion false. This point is made in Richard (1990, 79).

32. Perhaps this is more obvious in a sentence such as: Anyone who believes that Superman flies believes that Kent flies.

33. This responds to a question I raised in 1.1.

34. NB The following position is unavailable: 'Say nothing about whether the mode of presentation associated with a sentence *S* is relative to the speaker or the subject. Reference simply gets made, given a belief report token *u*, to some appropriate mode of presentation.' This leaves us with no way of assigning truth conditions to *u*. Simply succeeding in getting your intention to refer to mode of presentation *m* recognized is not yet to say something true.

35. The first remarks apply, *mutatis mutandis*, to the version of HI endorsed by Crimmins (1992), where it is argued that each belief report provides a particular mode of presentation under which the subject grasps the specific proposition. As Crimmins does not maintain the thesis that that-clause reference is idiolectic-relative, the second set of remarks do not apply to his position.

36. As distinct from the purely psychological claim that all belief is belief-under-a-mode-of-presentation. This claim may well be true without having any immediate semantic implications.

37. Also, Crimmins indicates that there is no grasping requirement on his view--no requirement that in asserting that $B(A, p, m)$ the speaker herself 'grasps' m . But, on close inspection, Frege didn't really hold such a view. Recall that Frege maintains that that-clauses refer to mode-of-presentation-containing propositions. To understand a belief report--in particular the contained that-clause--is to (a) know what the clause refers to and (b) grasp the sense (mode of presentation) associated with the that-clause. There is absolutely no requirement that the speaker 'grasp' in any way the proposition ascribed. So there really is no difference between this view and Fregeanism on the matter of grasping.

On the other hand, [4]--the ultimate counterexample to neo-Fregeanism--doesn't provide any serious difficulty for Crimmins' view. For it is presumably mutual knowledge among any group of informed speakers that the manners in which everyone who has met Al Sharpton thinks of him can have nothing in common. So no style is implicitly referred to in the tokening of [4].

38. Crimmins' example of referring unproblematically to (say) Clinton's kidney is not apposite, as Clinton presumably has two kidneys. It seems to be a pragmatic principle that if x doesn't know that F is uniquely instantiated (either no F 's exist or multiple F 's exist) then x cannot refer to the F . This principle survives even if we allow for reference in cases of causal isolation. But more on this below.

39. I don't wish to quibble about definitions. If someone wants to say that I can refer without making manifest to my audience what I am referring to, that's fine (in fact, strategy 5.2 recommends this course). But if we are sticking to Gricean constraints on speakers' meaning, then it seems that speaker's reference, as speaker's meaning, must be mutually manifest.

40. Jones might have an entirely separate--and unrelated--notion of Clinton (for example, 'the brother of that lousy Country and Western singer'). Crimmins characterizes the former notion as Jones' *normal notion* of Clinton. However, it is not clear what "normal" could mean here if we are to take any hint from the word itself (what is Lois's normal notion of Superman/Kent?). One notion, it seems to me, cannot be said to be more normal than another. Rather, one is simply more conversationally salient than another.

41. Which is not to say that all such relative clauses can be intelligibly used simply because the constituent individual can be referred to. For example, I cannot refer to Wagner's teacher since (as I explained above) 'x's teacher' is not uniquely denoting. Moreover, I cannot use that expression referentially, given my ignorance about Wagner's musical training. Thus, unproblematic reference to *o* doesn't suffice for unproblematic reference to *R(o)*--where '*R*' is a relative clause. One has to know that *R(o)* exists and that it is unique.

42. Crimmins (1992) handles the objection from causal isolation rather differently. (He actually develops what could be a response in a different connection, concerning whether or not a notion, to be of an individual *o*, must be causally related to *o*.) His view is that causal connection is not essential to the aboutness of notions, but rather to their utility. Notions causally isolated from their objects are relatively useless (e.g., about the shortest spy, or Kaplan's Newman I). But it is not clear that they can be disqualified as notions simply because of uselessness. As he suggests, such a notion would be like an FBI file that contained general information (e.g., 'the first person in 1994 suspected of selling defense secrets to Iraq'). It would take me a bit to far afield to evaluate Crimmins' (rather persuasive) case here.

Related to this is a discussion in Adams (1986), where he distinguishes the *metaphysical* thesis that

there are not yet any thisnesses of individuals that will exist but do not yet, nor any singular propositions about future individuals--and, hence, that all possibilities are purely qualitative except insofar as they involve individuals that already do exist or have existed (Adams, 1986: 23-24).

from the *semantical* thesis that we cannot have beliefs, or make assertions, involving individuals that do not yet exist. While he leans toward the latter thesis as well, he leaves the question of its truth open. As he suggests, the current

assertion that Newman I will be the first child born in the twenty-first century can be seen as "standing in some transtemporal relation" to the relevant singular proposition by Newman's contemporaries. The open question is whether these relations will be the familiar relations of propositional attitude.

This attempts to show, again, that whatever our semantic intuitions, they cannot be made law simply because they reflect a metaphysical picture, however plausible the latter might be.

43. There are further constraints. One requirement on a precisification (making it 'admissible') is that an extension of meaning that reverses the value true cannot be further extended even if it yields the original value true. The latter extension is by definition a precisification--it extends the meaning, and retains the truth-value, of the original--yet it is surely unacceptable. An example of Sorensen's (1988: 237) is apt here: 12:16 PM cannot be a precisification of 'noon-ish' if 12:15 PM fails of noonishness.

44. Not all precisifications are precisifications of vague sentences. Consider the following exchange :

A: Merlin is a cat.

B: Be more precise: What kind of cat is he?

The precisification requested here is a one-step precisification, requesting a particular type of cat. But notice that if A's assertion is true it cannot true for all admissible precisifications. Hence it is not vague.

45. The styles must be restricted further than mere candidacy, which I take to consist in belonging to the cognitive environment. This restriction is indicated by requiring that the styles be admissible candidates. My reasons are as follows. Some mutually known styles are simply irrelevant to the communicative transaction. For example, I utter 'Jones believes that Mick Jagger is a poet' in a context at which I and my audience are mutually aware of the fact that Jones, a Rolling Stones aficionado, knows that Jagger was born in 1943. The style suggested (determined by the property of being born in 1943 and being a poet) is by definition a *candidate* style, but it is irrelevant to our communicative interests. Thus it is inadmissible. (I am indebted to Schiffer for making me see this point.) Following Fine, I take the notion of relevance (or admissibility) as a primitive. Alternatively, it can itself be taken as vague. See further, note 43.

46. It is interesting to note that attributions of the form 'A believes that S,' where 'S' is vague, will all be susceptible to similar considerations. That is,

Jimmy Olsen said that the wall is blue,

uttered in a context C of a blue-green region, will also fail to make reference to a determinate proposition on the super-truth account. The reason is clear: 'the wall is blue' fails (at C) to express a determinate proposition; thus, 'that the wall is blue', fails (at C) to refer to a determinate proposition.

47. Stephen Schiffer has suggested another, related problem to me. [ST] should assign "super true" to [15], to reflect its intuitive truth value. Yet, according to [ST], [15] is super true iff it is true for all precisifications. And [15] is, in fact, false for all precisifications. (The reason [15] is super false is that the that-clause with widest scope fails of determinate reference, making each precisification of [15] false.) Thus it is super false, contradicting the intuitively correct truth-value assignment.

My solution is to recommend a separate clause for embedded belief reports--[C] above. We assign truth conditions to 'A said that S' (where 'S' is a belief report) relative to a *precisification* of 'S'. If 'A said that S' is true relative to each precisification of 'S', it is super true.

48. We might even go so far as to provide a notion of *super reference*. *t* super refers in ...*t*... iff any admissible assignment of reference to *t* makes ...*t*... true. Thus, 'that Lois believes that Kent flies' would super-refer in [15] iff [15] was true according to each admissible assignment of reference to the that-clause.

CHAPTER TWO

Descriptions and Incompleteness

Introduction.

I would like, at this point, to join some of the themes of the previous chapter with issues concerning the semantics of definite descriptions. The transition is a natural one, as the central concern of the prior discussion was answering the multiplicity problem, and this is, in effect, the problem of incomplete definite descriptions.¹ What I propose to do in the current chapter is to pursue two of the strategies employed above in defense of the hidden-indexical theory, and apply these strategies to the problem of incompleteness.

The structure of this chapter is as follows. I will consider a single argument for the claim that some utterances of definite descriptions are referential and that, consequently, definite descriptions are semantically ambiguous. This is the argument from incompleteness. That is, I will not mention arguments for the *ambiguity thesis* having to do with misdescription. One justification for this omission is that the argument from misdescription does not, in itself, motivate a referential interpretation of

definite descriptions. It is uncontroversial that speakers can refer to objects with descriptions that fail to denote those very objects. For example, the definite description 'Smith's murderer' can be used to refer to someone accused but not actually guilty of Smith's murder. This is to say, following Kripke (1977), that in the case of definite descriptions, *speaker's reference* and *semantic reference* are not necessarily coincident. Adding a little bit of semantics, we might even go so far as to say that it is (relatively) uncontroversial that a speaker can, in an utterance *u* of 'The *F* is *G*', have as her primary intention to communicate the proposition that *a* is *G* (where *a* is a particular falsely believed to be uniquely *F*). But these observations hardly motivate an account according to which 'The *F*' can function, *semantically*, as a demonstrative. Yet this is what the argument from misdescription purports to show. As Stephen Neale writes:

The problem with this argument is that it relies on the existence of a clear intuition that the proposition expressed is still true despite the fact that neither Jones nor anyone else satisfies the description 'Smith's murderer'. But this is simply not so... After all, the description [the speaker] used *failed to fit* the person [the speaker] wanted to "talk about," and to that extent the speech act was defective (Neale, 1990: 91, emphasis in text; note omitted).

Section 1, accordingly, concerns itself exclusively with the argument from incompleteness. Section 2 develops two general strategies (the first due to Howard Wettstein, the second to William Blackburn and to Neale) for resolving

the problem of incompleteness. In part 3 I evaluate these strategies, arguing that the preferred strategy--essentially Neale's and Blackburn's--is deficient in certain respects. This motivates two strategies of response, to be considered in sections 4 and 5. The first response will pursue a defense of the claim that the context of an utterance of a definite description (where it is assumed that the utterance possesses determinate content) is typically rich enough to isolate a unique completion. It is argued, contrary to what is commonly assumed, that incomplete descriptions are elliptical for complete descriptions and that the completion is uniquely determined by features of the context of utterance. The second response abandons the assumption of semantic determinacy, and develops a treatment of definite descriptions according to which (i) the definite article is semantically unambiguous but (ii) token definite descriptions are semantically indeterminate. It is argued, once again, that utterances of incomplete descriptions are elliptical for complete descriptions, but it is maintained in addition that such utterances fail to determine unique completions. Consequently, such utterances possess no determinate content. My conclusions in each case are negative; neither development of the Russellian theory is compatible with certain antecedently plausible semantic theses.

1. The Argument from Incompleteness.

1.1 Formalities.

Following Grice (1969: 139), Davies (1981: 149-52) and Neale (1990), I will interpret descriptions as restricted quantifiers. This interpretation is not essential to my argument, but the analogy with other quantifiers will highlight certain weaknesses in Wettstein's argument, which we shall consider below. The other alternative is, of course, to regard descriptions as singular terms. In any case, we will assume here that the choice is not philosophically tendentious: one can consult Davies (*op cit*) for a proof of the equivalence of the two formulations (in the context of a language allowing restricted quantification).² (I will postpone a critical evaluation of the significance of the equivalence until Chapter Three.)

An English sentence of the form 'the F is G' thus gets represented as:

[1] [the x: x is F] x is G.

Quantification is uniformly interpreted as restricted. Restricted quantifiers are constructed from determiners (expressions such as 'some', 'all', 'no', 'most', 'at most forty-one') and open sentences. The determiner binds a position in an open sentence to form a restricted quantifier. For example, 'no F's are G's' becomes:

[no x: x is F] x is G.

The determiner 'no x' binds a position in the open sentence 'x is F' to form the restricted quantifier '[no x: x is F]'; this in turn binds the open sentence 'x is G' to form a closed sentence. Similarly for the remaining quantified sentence forms.

1.2 The Argument.

The argument from incompleteness, which I derive from Strawson (1950), Donnellan (1968: 204n) and Wettstein (1981), runs as follows. Most descriptions (e.g. 'the senator', 'the lead guitarist', 'the newspaper', 'the coke machine') are incomplete. That is, they do not uniquely specify any object; in particular, they fail to pick out their intended denotations. The first observation implies that the relevant descriptive sentences must be literally false on a Russellian analysis. For, on that analysis, a sentence of the form [1] is false if 'F' fails to be uniquely instantiated (if, in Russell's terminology, 'The F' is "improper"). And yet, the propositions conveyed by such sentences are not typically taken to be false by speakers who utter them. (Even though they well realize that the descriptions they produce are not in fact uniquely instantiated.) Thus, it seems, Russell's analysis fails to comport with facts about how the language is actually used.³

The natural way to circumvent this criticism is to maintain that an utterance of the form 'The F is G' (where

'F' is incomplete) is elliptical for some contextually definite completion of the (incomplete) sentence produced. Yet, it is maintained, there is no non-arbitrary way to complete sentences containing incomplete descriptions. This blocks the natural Russellian rejoinder to the first criticism. It is concluded that the only account consistent with the assumption that we can ascribe determinate content to utterances of such sentences must take them to be referential, that is, as expressing singular propositions.

Take, for example, the sentence [2]:

[2] The table is covered with books.

As Wettstein suggests, many pairwise non-synonymous completions, drawn from a set of candidate completions, may be made salient by such an utterance ('the table in room 209 of Camden Hall at t_1 ', 'the table at which the author of *The Persistence of Objects* is sitting at t_1 ', and so on). But each completion would result in a different descriptive proposition's being assigned to [2]. As Wettstein asks, "The question now arises, which of these more complete (Russellian) descriptions (or conjunction of descriptions) is the correct one, the one that actually captures what the speaker intended by his use of the definite description 'the table'?" (1981: 41)

Wettstein poses two difficulties for a Russellian response.⁴ The first is epistemological. Even if there were a correct completion of the description in question, he

suggests, the audience would typically not be in a position to determine what that completion is. The second response is deeper: the speaker may often have no determinate intention.⁵ Asked, the speaker might respond that he did indeed intend to speak about the table, but that he did not intend to have it thought of as (for example) the table in room 209 of Camden Hall at t_1 , or the table at which the author of *The Persistence of Objects* was sitting at t_1 . Yet, if the speaker himself cannot determine what he meant--cannot paraphrase what it is he attempted to convey in additional descriptive terms--then it seems unlikely that there could have been anything descriptive that he did mean. Thus Wettstein:

It now becomes difficult to attach sense to the idea that one of these Russellian descriptions could be correct. Surely it is implausible in the extreme to suppose that in fact one of these descriptions captures what the speaker intended but that we cannot, even with the help of the speaker himself, come to know what description that is (Wettstein, 1981: 42).

Schiffer (1981) expresses a similar worry about the context supplying a unique description or individual concept. His remarks are directed against a description theory of demonstratives, but they apply equally well to a Russellian theory of definite descriptions. He imagines a hypothetical speaker who utters the sentence: 'I'll be damned! He's drunk.' According to the description theory, the propositional content of the pronoun is a description.

That is, in uttering 'He's drunk' the speaker asserts a proposition of the form 'The F is drunk.' But it is likely in such a context that speaker and audience share, and know themselves to share, a number of descriptions uniquely specifying the relevant individual. He writes:

Imagining myself as your audience, I do not see how I could have identified any one individual concept, however complex, as the one which figured into the [Russellian] proposition that you asserted. And yet it would seem that I understood your utterance perfectly well ... (Schiffer, 1981: 77).

How then is it possible to assign determinate content to utterances of sentences such as [2]? Wettstein responds that, while the context is insufficient to determine a completion of the description, yet "the context does reveal ... which item is in question" (43). The conclusion is drawn that the only way to account for the *prima facie* semantic determinacy of the utterance of [2] is to interpret its description referentially, indeed, as Wettstein suggests, as a demonstrative. "The proposition is determinate since the speaker makes a *determinate reference* and goes on to predicate a property of the thing to which he has referred" (44). Wettstein can be construed as maintaining that the relevant descriptive utterance expresses an object-dependent proposition. In general, to say that α occurs referentially in an utterance u is to say that the proposition u is used to assert is object-

dependent. For example, the proposition asserted by an utterance of 'She is bold' is dependent for its existence upon the existence of the referent of 'She'. If there exists no relevant referent, then the utterance is contentless.

Michael Devitt offers a diagnosis similar to Wettstein's:

In my view it is more plausible to see [incomplete definite descriptions], in their normal use, as like demonstratives and personal pronouns. In that use, not only do they not denote, *they do not even purport to denote*. They are not uttered with the intention of applying uniquely, nor would their audience normally take them to have been uttered with that intention (Devitt, 1981: 517; emphasis in text).

We seem to have the following argument:

- [a] There exists an utterance *u* containing an incomplete definite description. (Assumption)
- [b] *u* possesses determinate content. (Assumption)
- [c] The content of *u* is not simply the proposition determined by its linguistic meaning, interpreted relative to the context of utterance. (I.e. strict Russellianism provides the incorrect analysis of *u*.) (Assumption)
- [d] Either a description is strictly Russellian, or it is completed by contextual information. (Modified Russellianism)
- [e] Since *u* is not strictly Russellian, it must be completable by contextual information.
- [f] But while *u* is completable by various contextually-provided descriptions, there exists no *unique* completion. (Assumption)
- [g] Since there is no fact of the matter as to whether or not a given meaning-assignment to *u* holds, *u* is semantically indeterminate.

- [h] But we assumed in [b] that *u* possesses determinate content.
- C1. Thus, Russellianism is incompatible with the assumption of *u*'s semantic determinacy.
- [i] The only remaining interpretation compatible with the assumption of *u*'s semantic determinacy is that *u* is referential.
- [j] An utterance *u* is referential just in case *u* expresses an object-dependent proposition.
- C2. Thus, *u* must express an object-dependent singular proposition.

Note that Wettstein's (and Devitt's) claim--that is, C2--is of genuine philosophical interest only because it is about the proposition determined by the literal meaning of the sentence uttered (evaluated relative to the context of utterance). To say that some *uses* of descriptions are referential is not yet to say anything about the propositions *expressed* or asserted by such uses. For it is still possible to maintain, following Kripke, that speaker's reference and semantic reference diverge in such a context. What Wettstein has done is to provide the basis for the following diagnosis: the definite article 'the' is semantically ambiguous. Some uses of 'the' are attributive, others are referential.

In Wettstein (1983) there is a clear endorsement of the ambiguity thesis:

A referentially used description functions semantically as a demonstrative; an attributively used one functions as a property-attributer. Accordingly, referential and attributive utterances of "The murderer of Smith is insane" will express different propositions (Wettstein, 1983: 51).

His claim is that there is in fact a semantical ambiguity associated with the definite article, which definite descriptions inherit. This is "the semantic significance of the referential-attributive distinction." And this claim is a direct consequence of C2.

It should be noted that there are good methodological reasons for avoiding the postulation of such ambiguities. Linguistic forms often have a diversity of uses--consider the connectives 'if' and 'and', for example. It is surely unattractive to specify different senses for each use of 'if' if we can explain the various uses by reference to Gricean conversational maxims. For example, it has been held that while the linguistic meaning of 'if' is given by the definition of the material conditional, the proposition that one asserts in uttering a conditional 'If A, B' is often not simply *not-(both A and not-B)*. The claim that the meaning of 'if' is exhausted by the standard truth definition for '⊃' is unsatisfying, and has moved some philosophers to distinguish between the truth-functional sense (i.e. '⊃') and the indicative sense (to give it a name) of the conditional. (Most, though not all, philosophers assume that the sense of the subjunctive

conditional is distinct from the truth-functional or indicative sense.) For example, in uttering, 'If Jones lands a movie role, he'll abandon philosophy' (' $A \supset B$ ') I implicate the proposition that the probabilities of ($A \supset B$) and ($A \supset B$)/ A are both high. More simply, I implicate a proposition to the effect that ($A \supset B$) is a non-vacuously true conditional: that it is not true simply because Jones will very likely never become a movie actor.⁶

Conversation, according to Grice, is guided by a Cooperative Principle, which is expressed in certain maxims. In particular, in uttering the above conditional, I am guided by the Maxim of Quantity:

Make your contribution as informative as is required (for the current purposes of the exchange) (Grice, 1989: 26).

And it seems clear that one is hardly being appropriately informative in reporting that ($A \supset B$) is true if it is the case that A is very likely false. For, in that case, one should simply say not- A . Now obviously the maxim is vague: but just as obviously, if one asserts something that means ' $A \supset B$ ', when one believes that the probability of A is low, then one is not maintaining the required level of informativeness. Thus, since the speaker is not in fact saying not- A , but ($A \supset B$), and since he has given no indication of abandoning the conversation, he has presented himself to the audience as wanting them to believe that ($A \supset B$) is non-vacuously true. Which is to say that, although he

said that $(A \supset B)$, he meant, or implicated, the stronger proposition that the truth of the conditional is non-vacuous.

The methodology here conforms to a principle Grice called Modified Ockham's Razor (MOR): Senses are not to be multiplied beyond necessity (Grice, 1989: 47). Given that our uses of 'if' are equally accommodated by interpreting it as meaning ' $\supset$ ' or by distinguishing senses of 'if' for each distinct pattern of uses, the reasonable strategy is to assign it one sense. As Neale writes:

Given the viability of a broadly Gricean distinction between the proposition expressed and the propositions(s) meant, if a pragmatic explanation is available of why a particular expression appears to diverge in meaning in different linguistic environments (or in different conversational settings) then *ceteris paribus* the pragmatic explanation is preferable to the postulation of a semantical ambiguity (Neale, 1990: 80).

The principle is relevant to Wettstein's discussion of definite descriptions in that it is open to the semantic theorist to *decide* to classify the distinct uses of definite descriptions as corresponding to distinct senses of 'the'. What constrains the decision is something like Grice's principle. Doubtless, we can posit an ambiguity between referential and attributive uses. Differences in use are easy to discover and classify. The question is, whether we are *forced* to regard the classification as semantic. This is the philosophical significance of the referential-attributive distinction.

2. Responding to the Argument.

The argument from incompleteness is the strongest of the ambiguity theorist's challenges. Yet one important point to note is that not all cases of incompleteness can be analyzed as referential. Consider the following example. I walk past a concert hall on a Friday evening. Hundreds of adolescents are waiting on line. I remark to my companion: 'Well, the band must be good.' Clearly, the ambiguity theorist wants to say, there is no unique completion for the description 'the band'. Yet, just as clearly, we can assume that I don't have any belief about the band's identity. Thus a referential solution is out of the question here.

Wettstein does not ignore this problem (1981: 44-48). He acknowledges that there are attributive occurrences of definite descriptions which are nonetheless incomplete. Yet,

We can account for such determinacy by resorting to the notion of demonstrative reference For in uttering "The murderer is insane" in the presence of the mutilated body, the speaker relies on the context to reveal *whose murder is in question*. The speaker, that is, makes an *implicit* reference to the victim (47; emphasis in text).

One would like to know what effect this implicit reference has on the proposition expressed--does it contribute a propositional constituent? Does it select a completion? Wettstein offers no clue.

Yet Wettstein's remarks suggest that the entity implicitly referred to does become a propositional

constituent in that it is a determinant of the denotation of the description. For example, in the sentence, 'The murderer is insane,' uttered in the presence of the foully murdered Smith, the implicitly referred-to Smith will determine a completion: 'His murderer' (said pointing to Smith), or 'Smith's murderer'. As Blackburn notes: "Wettstein argues that on a Russellian use of an incomplete description the speaker makes an *implicit* reference to some object, rather than relying on an implicit *definite description*" (1988: 275, emphasis in text). Neale also claims that descriptions are available in such cases and for the same reason--that the context allows implicit reference to completing elements. I will return to this response to the problem of incomplete *attributive descriptions* shortly. Suffice it to say that, if the response is indeed adequate, then one is given to wonder why the defender of this view doesn't maintain that in all cases of incomplete specification the intended description is fixed by features of the context, and not just in clearly non-referential cases. (In fact, Neale suggests a general response to the problem that is quite similar to Wettstein's response to the specific problem of incomplete attributive cases. But, as we shall see, the suggestion is itself not wholly unproblematic.)

Whatever the merits of Wettstein's response to the problems arising from the purely attributive case, there are

decisive considerations against the referential treatment of the standard cases--cases where there is an intended denotation but where the intended denotation does not uniquely satisfy the description produced.

Consider the sentence:

[3] Everyone was sick.

This sentence is incomplete. Surely what the speaker asserted in uttering [3] was not the falsehood that every person was sick, but that every person meeting a conversationally salient condition was sick. Now one might argue that in this case as well as in [2] any completion would be arbitrarily selected. (We might select among: 'everyone who ate the humus', 'everyone who sat next to the fat kid with a cold', 'everyone who drank the water'. As in the case of incomplete descriptions, there would be no basis for a given selection.) But one would certainly not take this arbitrariness to motivate a view according to which 'everyone' occurred referentially in [3]. If this is the case, one wonders why identical considerations (i.e. absence of a unique completion) force upon us the view that incomplete descriptions are referential.⁷ Something more needs to be said if the argument from descriptive incompleteness is to establish its conclusion.

3. A Solution to the Problem?

I want in this section to consider Neale's "Russellian" solution to the problem of incompleteness, but there is a solution compatible with Russellianism that I wish to eliminate before considering Neale. The solution, while on the face of it quite attractive, seems decisively refutable.

3.1 Implicit Restrictions on Quantifiers.

Nathan Salmon (1982) challenges the referential reading of incomplete descriptions. He observes that "Everyone is accustomed to using determiners like 'any', 'every', 'some', 'no', 'few' etc., with a restricted domain ..." (89) His suggestion is to incorporate this observation into our semantical treatment of quantifiers. For example, he suggests that we evaluate sentences such as [3] relative to a restricted class K of persons. And this would, of course, suggest a similar treatment of definite descriptions: interpret 'The F is G' relative to an implicitly specified domain of F's. Let us call this strategy, following Neale, the "implicit approach". (Davies (1981: 161) also advocates a view according to which the use of quantifiers involves a "contextual limitation of the domain of quantification.")

The suggestion fails, however. As Lewis (1979) has shown, in sentences such as 'The senator met some other senators,' the implicit approach yields vacuous falsity.⁸ One might attempt to accommodate such an example by

responding that the implicit domain can switch at any point in a discourse--even in mid-sentence. This is implausible as it is. But what makes it incoherent is the fact that 'some other senators' refers back to 'the senator'. This being the case, we are not permitted to switch the domain. This is explicit when we look at the sentence's logical form:

[4] [the x : x is a senator] ([some y : y is a senator & $\neg(y = x)$] x met y)

The sentence [4] is true because:

[5] [some y : y is a senator & $\neg(y = x)$] x met y

is true under an assignment of some particular a to ' x '. To evaluate [5] in turn we must keep this assignment constant. Shifting domains is thus not possible.

Salmon's suggestion, while perhaps pre-theoretically intuitive, simply fails for contexts containing multiple occurrences of quantifiers binding the same predicate. It cannot provide a general solution.⁹

Barwise and Perry (1983: 146-151) develop an account of definite descriptions which is similar to Salmon's account. Unlike Salmon, however, Barwise and Perry espouse the ambiguity thesis. They maintain that a definite description can function as a device of direct reference in addition to its denotational role. Curiously, they do not opt for a uniformly referential treatment of incompleteness. (In fact it is unclear whether it is considerations stemming from

incompleteness which motivates their postulation of ambiguity.) It is their non-referential treatment that I will investigate here. Indeed, if Barwise and Perry's account is correct--if they have provided an adequate non-referential treatment of incomplete descriptions--then the principle motivation for an additional referential interpretation of definite descriptions will have disappeared.

Their suggestion is that incomplete descriptions can be accommodated by appealing to what they call "resource situations", where these are understood to be states of affairs that a speaker can make implicit reference to when producing an utterance. For example:

[6] The dog that ate french toast
while incomplete, is typically uttered with the intention of being interpreted relative to a particular situation. This situation can be taken as either completing the description ('the dog that ate french toast at s') or as indexically "anchoring" it. (The latter alternative requires that definite descriptions contain suppressed argument places for situations and that speaker's intentions determine the appropriate situation-assignment.) Either way we have a non-referential treatment of [6].

Instead of citing a domain of objects, relative to which we are to interpret the description, as Salmon suggests, Barwise and Perry make recourse to situations.

Implicit reference to situations enables one to recover the uttered description's semantic value. But the problems arising from the domain-relative account can now be seen to be inherited by the resource-situation-relative account.

For example,

The dog that ate french toast chased another dog that ate french toast,

requires a situation with a unique french-toast-eating dog to save its description from being improper and, hence, vacuous falsity; but such a situation will obviously not accommodate the quantifier 'another dog that ate french toast', which demands a second such dog. The possibility of mid-sentence situation-switching, entertained above, again fails to provide a solution, since interpreting 'another dog that ate French toast' requires that our interpretation of 'the dog that ate French toast' remains fixed. And mid-sentence situation-switching is incompatible with keeping the interpretation of 'the dog...' fixed.

3.2 Contextual Completions.

We will accept, for the moment, Neale's argument against the referential view. However, some problems remain. First, no positive account of the content-determinacy of utterances containing incomplete descriptions has been put forward. Perhaps Neale has in mind that his account of completions of attributive sentences will apply generally. This account suggests that incomplete descriptions are completed by

providing contextual coordinates--objects, times, locations.

I submit, then, that if descriptions may contain overtly referential components (including indexical and demonstrative elements), then there is nothing to prevent the ellipsed elements of incomplete descriptions from being referential (100).

(Which, as he points out, is not to say that the description occurs referentially.) This is, in essence, the position Blackburn ascribes to Wettstein, with the difference that Neale's account is intended to cover all uses of incomplete descriptions, not simply those resisting a referential interpretation. An immediate problem for such a suggestion, which Neale acknowledges, are descriptions having no implicit argument places for the context to fill in. The proposal at least gets off the ground with descriptions such as 'The murderer', produced in the presence of the victim. But with descriptions such as 'the table' in [2], no argument place seems available. Yet Neale takes 'The table' to be elliptical for 'The table over there' or 'The table over here'. These both contribute non-descriptive information to the proposition expressed--namely, a spatial location. But in doing so they serve to provide a complete description.¹⁰

Note that this proposal accounts for the intuition that incomplete descriptions have a demonstrative character, while avoiding Wettstein's suggestion that we assimilate such descriptions to demonstratives.

But this will not work. For example, take the sentence, 'The waiter is ignoring us.' Just as there will be indefinitely many completions that do not make immediate reference to particulars ('the waiter wearing black', 'the waiter who speaks with a unique accent' etc.), there are also indefinitely many completions that do make such reference ('the waiter who just served us', 'the waiter standing next to Dolores', 'the waiter serving *him*' etc.). It is obvious, I take it, that spatial coordinates do not have priority--that the correct expansion is not necessarily 'The waiter over here'. In some cases, spatial coordinates are simply inapplicable ('The movie was disappointing', 'The novel was poorly translated', 'The thought was premature'). Thus, the suggestion for completing 'The table'--even on the assumption that in the case in question 'The table at 1' is the contextually definite completion--doesn't generalize. Moreover, there seems to be no general procedure for uniquely determining a completion given simply an incomplete description and pragmatic elements.¹¹ The upshot is that we are faced with the same problem we started with: Utterances of incomplete descriptions typically have indefinitely many completions. Any choice among completions seems arbitrary. The problem, for the anti-referentialist, is how to explain away this apparent arbitrariness, and show that a unique completion is possible. But this Neale has not done. Certainly nothing in his discussion (100-01) leaves one

confident that this problem has been resolved.¹² But if this is the case we are without an explanation as to how an utterance of an incomplete description can express a determinate proposition.

Thus, the problem Neale's solution faces is simply a recapitulation of the problem Wettstein developed for the Russellian. As suggested above, Neale's solution is identical to Wettstein's solution to the problem posed by *attributive* cases of incomplete descriptions. Blackburn's criticisms of this solution correspond to my criticisms of Neale:

But this argument faces an objection exactly parallel to Wettstein's objection to Donnellan: the context does not always supply a unique implicit reference for incomplete descriptions (Blackburn, 1988: 276).

In some cases the implicit reference might be obvious (e.g. to Smith in the context of the utterance 'His murderer is insane'); but Blackburn has us consider a case of Donnellan's:

Donnellan imagines a speaker at a Temperance Union meeting saying: "The man drinking the martini, whoever he is, is breaking the rules of our club". This is a Russellian [i.e., attributive] use of an incomplete description. If Wettstein's account is correct, the speaker makes an implicit reference to some object. But which? The speaker may be unable to say whether he means "the man at *this meeting* who is drinking a martini" or "the man *in our club* who is drinking a martini", or "the man *in this house* who is drinking a martini". Thus there is no more reason to say that there must be a unique implicit reference than that the context always determines a unique complete description (Ibid.)

What Blackburn says about Wettstein's solution to the problem posed by attributive occurrences of incomplete descriptions is true of Neale's general strategy to solve the problem of incompleteness: implicit references are as numerous as implicit completions, and choosing among them is just as arbitrary.¹³

In section 4, I will examine the claim that utterances of incomplete definite descriptions do have unique completions. And in 5, I will examine the implications of denying the assumption that incomplete descriptions possess determinate content.

4. Defending Russell's Theory.

4.1 Definite Descriptions and Indexicality.

The account I wish to consider in this section maintains that there is a unique completion of 'the murderer' in a content-determinate utterance of 'the murderer is insane.' I have no preference for one or another account of how the description is completed: whether it is by adding additional properties ('the murderer *being addressed by the judge* is insane'), or by providing referents for elided argument places ('the murderer of *that man* is insane'). I do think, however, that philosophers who subscribe to the former account would subscribe to the latter as well. To maintain that unique completions are available in such cases carries no implication that the completions be wholly

descriptive--that is, that they be devoid of direct reference to any particulars. (As Neale points out, Russell himself uses 'my only son'--i.e. 'the only son of mine'--as an example of a definite description.) In any case, what I want to argue is that an utterance of an incomplete description has a unique completion which is the *conjunction* (in a sense to be made precise) of eligible completions that speaker and audience mutually believe the intended denotation to uniquely exemplify. More precisely, an utterance *u* of 'the *F* is *G*' has a unique completion δ such that (i) δ is the conjunction of eligible completions of 'the *F*', and (ii) the content of *u* is *the proposition that δ is G*. My argument will support a more general claim, which will surface from time to time: any descriptive utterance *u* makes salient a number of alternative descriptions which combine to determine the propositional content of *u*.

The theory is the hidden-indexical theory of definite descriptions (HI-D). It maintains that an utterance *u* of the form:

[7] The book is overdue.

is completed by a description or individual concept, where the resulting completing proposition is what *u* asserts. The completion is not articulated in [7]; thus it is hidden. Moreover, the completion varies across contexts; thus it is indexically specified.

As with HI, HI-D faces a problem: a descriptive utterance typically generates a multiplicity of completions, none of which seems favored by contextual elements, including, crucially, the speaker's explicit intentions. Defending this theory requires, then, making plausible the suggestion that part of what is meant or intended in uttering [7] is not explicitly represented to the speaker. The suggestion is held by some to be problematic. It seems to violate the assumption that if a speaker means *p* in uttering [7], then she must be able to specify, in terms other than [7], precisely what it was that she meant.

Wettstein has carefully defended the claim that it is, in general, impossible to isolate, even with the speaker's help, the unique completion that she meant in uttering an incomplete description. He claims, with respect to the intended completion of a descriptive utterance, that "In many cases ... the speaker will have no ... determinate intention" (42). But Wettstein restricts himself to what the speaker, at the moment of utterance, explicitly had in mind. And he is correct in maintaining that, typically, what the speaker (explicitly) had in mind (in addition to contextual information) will not yield a completion. But this does not rule out the possibility of arriving at a completion given what the speaker implicitly intended (together with contextual information). In addition, Brian Loar has suggested a method for arriving at the speaker's

implicit intentions. If effective, this will show that implicit content is not an arbitrary posit, motivated solely by concerns internal to the Russellian strategy.

Loar maintains that implicit content can be determined by the speaker herself reflecting on what was essential to what she meant in a given utterance. Let us consider his argument that some completing proposition is involved in a descriptive utterance:

If ... someone addresses us, and I then say '[The man] looked familiar', I may not be prepared to reject either 'the person who just appeared to speak to us', or 'the person we just saw in such and such place', as essential to what I meant. That in such a case it is their *conjunction* which is the essential individual concept is made plausible by the fact that, if through some bizarre contingency those individual concepts ...
are discovered not to be conjointly instanced, then one will be inclined to think that there is nothing which clearly was one's referent (Loar, 1976: 361).¹⁴

This suggests that there are beliefs about completing propositions that my utterance manifests. Indeed, Loar claims that these beliefs determine what I meant in uttering 'the man looked familiar'. The question arises, however, how we can identify those beliefs, and, perhaps more importantly, of the bearing of those beliefs on the meaning of what I uttered. Loar writes

Consider first the implicitness of what a speaker believes about his and his hearer's *mutual beliefs* about some familiar object of conversation. That such implicit beliefs play a central role in the psychology of interpersonal contact I take to be an uncontroversial assumption of any realistic psychology. But the content of our intentions is shot through with the content of our implicit

expectations. This is especially operative in just those cases in which a speaker reflectively tries to isolate what was essential to what he intended to communicate; in effect he consults his background beliefs about mutual beliefs, and decides which individual concept or concepts implicit in those mutual beliefs were such that their recognition was essential to the success of his communication (Ibid).

We can see a potential response to Wettstein in the final lines, one relying on counterfactual properties of what the speaker intended to communicate. But even if Loar has shown that there are determinate implicit beliefs involving descriptions or individual concepts that are, in retrospect, essential to what was communicated, he has not shown that these beliefs somehow *contribute* to what was communicated. In other words, a description δ may be essential to what I meant in producing 'the F is G' and yet not be a component of what I meant. The suggestion I am making is that Loar has not distinguished between propositions that an utterance presupposes, and the proposition(s) that the utterance means. Consider, in this connection, my utterance of [8]:

[8] None of John's friends find him especially amusing.

It is essential to what I meant, we may suppose, that John has friends. Yet it is quite implausible to maintain that I meant, in uttering [8], that John has friends. (Which is not to deny that one can concoct an unusual scenario in which that is precisely what I meant.) This then, shows that being essential to an utterance's meaning is not necessarily to be a component of that utterance's meaning.

Nonetheless, if it can be shown that a completing proposition *p* is essential to what I meant in uttering [7], then the argument Wettstein puts forward will have been met. In other words, there will be no reason to deny the Russellian the claim that the implicit proposition essential to my intention in uttering [7] is in fact what I meant in uttering [7]. After all, Wettstein mounts an impossibility argument: an incomplete descriptive utterance cannot make salient a unique completion. If, *contra* Wettstein, such a unique completion is available, and if, as Loar attempts to show, it is essential to what is meant, then surely it is what is (literally) meant.¹⁵

How, then, can we show that a completing proposition *p* is indeed essential to what I meant in uttering [7]? Loar suggests that we "consult [our] background beliefs about mutual beliefs" and determine which of those beliefs are essential to what we meant in uttering *u*. That is, we decide whether there are circumstances *w* at which certain of those beliefs are discovered to be false ("bizarre contingencies") and yet *u* means what it actually means. If there are no such circumstances, recognition of the beliefs in question was "essential to the success of [our] communication."

Part of what gives Wettstein's argument its plausibility is that there are typically multiple completions of incomplete definite descriptions. And, he

would respond, *each* of these completions has a claim to being essential to what was meant. But, if this is so, then the Russellian has the option of taking the conjunction of "essential" completions as the one completion essential to what was meant. Far from being arbitrary, this seems correct: if each completion made salient by an utterance *u* is such that, upon discovering that it is, in fact, not co-instantiated with the others, we retract *u*, then the conjunction of those completions is essential to what we meant.

Loar has established that an utterance containing a definite description has certain distinctive presuppositions. For example, a tokening of [7] may presuppose beliefs linking the referent of 'the book' with other, complete, descriptions. Elucidating the notion of a presupposition, Stalnaker writes:

Presuppositions ... are something like the background beliefs of the speaker--propositions whose truth he takes for granted, or seems to take for granted, in making his statement" (Stalnaker, 1974: 472).

For our purposes, presupposition can be understood as the following relation between utterances, propositions and persons:

u presupposes *q* iff *u*'s expressing a true proposition implies that the speaker believes *q* and *u*'s expressing a false proposition implies that the speaker believes *q*

According to this definition, a presupposition is (or reflects) a propositional attitude. This allows us to treat presuppositions as features of a context of utterance, and, consequently, as one of the determinants of the content of an utterance. (In particular, these presuppositions can be understood as determining the value of the hidden indexical.)

This account of the intuitive notion of presupposition is somewhat non-standard. For example, Lewis (1984) holds the following principle:

[L] If p is not presupposed by each conversational participant at t , and an utterance at t' requires p , then p will automatically be presupposed at t' .

If, for example, Jones utters [8] in the course of a conversation, then, even if the proposition that Jones has friends is not presupposed before the utterance is made--is not mutually believed or assumed--the utterance itself will generate the presupposition.

[8] None of John's friends find him especially amusing.

Lewis's account assumes that background beliefs (or assumptions) relevant to interpreting utterances will always be made manifest by those very utterances. But this is not true. Consider the following situation. H asks A whether Jones survived the operation. A responds: 'Jones's wife wore black today.' Clearly A has implicated the proposition that Jones did not, in fact, survive the operation. The

implicature, moreover, could not have been generated unless a presupposition was in place--namely, that Jones's wife's wearing black the day after his operation would be an unequivocal sign that Jones did not survive the operation. Without the presupposition, the implicature could not have been justified, and, as Grice makes clear, justification is essential for the determination of implicature. Yet, it is obvious that the presupposition is not made manifest by the utterance; someone who did not know the presupposed proposition would not learn it simply by hearing A's utterance.

One might argue that this example only establishes that background beliefs relevant to determining u's *implicatures* are not always made evident by u and that it fails to refute the claim that background beliefs relevant to determining what u asserts are always made evident by the utterance. But, presumably, Loar is claiming that the background beliefs that determine what our descriptive utterances mean determine what they *literally* mean, or assert. Yet, the following example, derived from Wilson (1991), shows Lewis's claim to be in error. Determining what [q] asserts presupposes [p], yet a tokening of [q] will in itself fail to make [p] manifest:

[q] If the U.S. president makes a surprise appearance tonight, then the former Arkansas governor will be swarmed by admirers.

[p] the U.S. president is the/a former Arkansas governor.

Descriptions, as is well-known, have a pronominal function. This is evidenced by the fact that [q] is content-equivalent to [q']:

[q'] If the U.S. president makes a surprise appearance tonight, then he will be swarmed by admirers.

Yet, to assign the proper content to [q], one must know [p]. Otherwise, the description 'the former Arkansas governor' will be interpreted attributively.

Thus, Lewis seems quite clearly to be wrong: not all presuppositions of an utterance are manifested by that utterance.

We now have the resources to respond to the following question:

Consider the following completion of a tokening of [7]:
[7'] the x such that x is a book and x is in Mary's bag
and Bill and Hillary are married.

Clearly, this should be eliminated as inessential to what I meant in a normal utterance of [7]. And yet, it is a completion that is mutually believed to be co-denoting with the uttered description, and with other obviously essential descriptions--for example,

[7''] the x such that x is a book and x is in Mary's bag and
x is a novel by Yourcenar.

(It is, of course, not an explicit mutual belief, but, rather, an implicit mutual belief.) How can we make a principled distinction between admissible completions, such as [7''], and inadmissible ones, such as [7']?

The problem here is that any mutual belief can be incorporated into a completing description. If 'the F' is complete and if p is mutually known to be true, then 'the F such that p is G' is essential to the meaning of my utterance of 'the F is G'. Yet the response to this is straightforward. Consider the following principle:

[P] A's utterance u presupposes p only if there is no possible situation w in which A would maintain the meaningfulness of u in light of information that p is false.¹⁶

(This restates, more perspicuously, the initial condition on presupposition.) Let us test this. Clearly I would maintain that my utterance of 'the book in Mary's bag is overdue' is meaningful in a situation in which it became clear to me that the Clintons are not legally married.

(That is, in which it became clear that [*] *the x such that x is a book and x is in Mary's bag and Bill and Hillary are married* is not identical with the *x such that x is a book and x is in Mary's bag and x is by Yourcenar*.) Thus, [P] shows that [*] is not a presupposition of the utterance of [7]. On the other hand, it classifies certain implicitly-held mutual beliefs as presuppositions which are intuitively presupposed by a given utterance. Consider an utterance of 'The author of *Dr. Faustus* died at twenty-nine.' Assume that the speaker, A, believes, at the time of the utterance, that the author of *Dr. Faustus* is identical to the author of

Richard II, but that A subsequently discovers that this belief is false. It seems right to say that, while A could explain what led him to utter what he did, he could ascribe to himself no determinate intention in making the utterance; for the specific background beliefs that would have held the intention in place are disrupted by the new information. The failure of the assumed identity renders the utterance meaningless. Intuitively, then, the utterance *presupposes* the identity. And [P] offers the same verdict, since there is a situation at which the speaker, learning of the falsity of the belief, would deny that what he uttered was meaningful (or, at least, true).

I think it is clear that Loar's argument shows that completing propositions are presupposed by descriptive utterances in the sense I have just sketched. I will now develop two problems for this view. The first problem is that there will be many completions that, by definition, are presupposed according to [P] but which are, intuitively, quite inessential. I will attempt to correct the definition to filter out these "inessential" concomitants. Finally, there is the problem of how to place this theory within the general context of a directly referential semantic theory. Espousing a Russellian theory of descriptions while maintaining a directly referential treatment of

demonstratives seems, on the face of it, arbitrary. I will address these problems in order.

4.2 Presuppositions and Enabling Conditions.

Recall that I am suggesting that various counterfactuals can be used as evidence to show that an utterance of [8] presupposes a set (or "dossier") of beliefs relating the speaker's referent to a number of complete descriptions. The justification for the latter claim is the counterfactual intuition that if an instance of a completing description δ had failed to be co-instantiated with the referent of 'the book' (or with a completion in the dossier), then the utterance would be either false or meaningless. This, following Loar, is an implicit background belief essential to what I meant. But, as we shall see, such a belief may bear no intimate relation to the content of my utterance. The problem is that the test fails to distinguish between content-relevant background beliefs, and mere enabling conditions for the speech act.

For example: it is presupposed, according to [P], by my utterance that we both speak English, that I am telling you the truth, that I wasn't created twenty minutes ago, that there are material objects, and so on. That is, according to Loar's test, these propositions are essential to what I meant. (If, on reflection, I didn't believe any of these propositions, I could not have meant what I did in

uttering [8].) Consequently, the following individual concepts is also essential to what I meant in uttering [8]: [8'] the x such that x is a book and x is in Mary's bag and

I speak English

(We assume for argument's sake that the book is in Mary's bag.) But the individual concept seems to have nothing to do with my particular utterance.

The problem is that the notion of presupposition recently defined doesn't correspond to any intuitive notion of presupposition or "required background assumption". But notice, also, that, Loar's directives for determining what is essential to meaning allows the same results.

It is clear what has gone wrong. The common feature of these meaning-irrelevant but nonetheless "essential" propositions is that they are enabling conditions for any utterance, and thus not utterance-specific. Let us call the set of counterfactuals that concern enabling conditions E-counterfactuals. If P is an enabling condition for u , then, for any utterance u' :

$\neg P > (u' \text{ is meaningless})$

(where ' $>$ ' is the counterfactual conditional). This definition is helpful. For in specifying the presuppositions of descriptive utterances, we need merely to restrict our counterfactuals to those that are not E-counterfactuals. This works as follows. For any

presupposition-candidate P , we ask whether P is a member of the following set E :

$$E = \{\sigma: \text{for all } u, (\neg\sigma > u \text{ is meaningless})\}$$

E is the set of generic enabling conditions: that set of conditions such that, for any utterance u , if any member of E fails to obtain, then u must be meaningless. We can make now our claim more precise: The content of an utterance u of 'the F is G ' is determined by the set of presuppositions of u not belonging to E . This, in effect, restricts the counterfactuals to those in which, among other things, we speak English. This is only fair: any use of the notion of presupposition must restrict the presupposition test to those worlds in which the assumption that we are speaking the language we are in fact speaking is not violated.

We can see now how to salvage the presupposition strategy. We redefine "presupposition" as follows:

[P] u presupposes* p iff u presupposes p & $\neg(\neg p > (u \text{ is meaningless}))$

That is, u presupposes* p just in case u presupposes p and p is not an enabling condition for u 's possessing content. In this revised sense, it cannot be claimed against any employment of the presupposition strategy that it is insufficiently specific.

4.3 Demonstrative Constructions and Schiffer's Dilemma.

Finally, there is the claim that if we allow for a Russellian theory of incomplete descriptions, there seems no reason not to allow a description theory of demonstratives. Loar, in fact, advocates a thoroughgoing descriptivist view. It is easy to see why: those beliefs essential to the meaning of an utterance of 'the F is G' at a context C must be assumed to be precisely those beliefs essential to the meaning of an utterance of 'that F is G' interpreted relative to C.

A description theory of demonstratives would take the following form:

An utterance *u* of 'that F is G' expresses a unique object-independent proposition *p* such that *p* is the conjunction of eligible completions of *u*.

(I assume that both descriptive and demonstrative utterances have completions.) And yet, the theory has little intuitive appeal and (perhaps more importantly) it is at odds with the direct reference semantics I have been advocating. Thus, as Schiffer (1994) suggests, we face a dilemma: to embrace a complete Russellian semantic theory--in particular, a description theory of demonstratives--or make an unmotivated distinction between the semantics of demonstratives and the semantics of descriptions.

I want to avoid a thoroughgoing Russellianism. But this is not for the reason that I deny that a sentence of

the form 'that F is G' can be used to mean a determinate object-independent proposition. If, as I have argued, 'the F is G' can be used to mean a unique completing proposition, so can 'that F is G'. It seems plausible that if one raises to salience a dossier of definite descriptions co-denoting with the intended referent, then both do. Rather, the reason for avoiding a hidden-indexical theory of demonstratives is that it does not comport with a relational theory of belief. In particular, it is incompatible with HI. For example,

John believes that he is giddy

will, according to any Russellian theory, have two readings: one in which the implicit definite description takes wide scope, and one in which it takes narrow scope. The narrow scope construal inherits all the difficulties associated with Fregeanism. For example, imagine the following, said of Al Sharpton:

Everyone who has ever met him believes that he* shampoos regularly.

If 'he*' abbreviates a definite description taking narrow scope, then this sentence expresses a false proposition. On the other hand, if 'he*' takes wide scope, then 'that he* shampoos regularly' and 'that he is giddy' fail to refer. This, again, contradicts HI.

Yet, Schiffer (1994) argues that one who subscribes to Russellianism must provide equivalent interpretations for 'the F' and its demonstrative counterpart, 'that F'. He

argues that two contexts differing solely in virtue of one's being a tokening of [9a] and the other's being a tokening of [9b] must, for the Russellian, have "exact psychological parity". Each determines a uniquely salient completing proposition.

[9a] The new italian restaurant has closed.

[9b] That new italian restaurant has closed.

And yet, if the utterances are identical in point of the speaker's attitudes, then they must be identical in point of what the speakers said. Yet, our Russellian maintains otherwise: he maintains that [9a] expresses a complex descriptive proposition, but that [9b] is object-dependent.

The claim that these sentences should be treated identically--assuming HI-D--is undeniable. A speaker's propositional attitudes--and a fortiori his *essential* propositional attitudes--in uttering one will be identical to what they are in uttering the other: in both cases his utterance will *mean* a proposition of the form 'the F that is H is G'. Thus a semantical distinction between demonstrative constructions such as 'that F' and descriptions such as 'the F' has no psychological motivation.

Can a semantical distinction be semantically motivated? Let us investigate this possibility. I will provide some examples designed to question Schiffer's claim that facts about meaning supervene on propositional attitude facts.

Consider a language like English--"English*"--except that the assertion rule for indicative conditionals in English is stipulated, in English*, to function as a truth definition:¹⁷

'If A, B' is true iff $P(A \supset B)$ and $P(A \supset B)/A$ are both high.

That is, the indicative conditional 'If A, B' is true (in English*) just in case the corresponding material conditional is (i) probable and (ii) not vacuously true (or not made vacuously probable due to the improbability of 'A').

Consider, then, the following indicative conditional, uttered by an English speaker, A, at a context C:

[10] If Shakespeare didn't write The Tempest, Jonson did.

Would we have any reason to suppose that a speaker of English*, who differed from A only in respect of the language he spoke, would have any intentions different from A in uttering [10] at the (counterfactual) context C'? Surely not. Presumably the propositions that A and his counterpart make salient in uttering [10] at C and C', respectively, are identical. But the supposition that they speak slightly different languages implies that what their respective utterances *said* is distinct.

This suggests that semantical content does not supervene on one's propositional attitudes. For two speakers could have identical propositional attitudes--and

consequently speaker-mean the same propositions--and yet not say the same things in their respective utterances. Of course, that this is the case does not show that two speakers could have identical attitudes and yet say different things in their respective utterances of 'the F is G' and 'that F is G.' But it shows at very least the possibility of a Russellian response to the charge of arbitrariness--namely, that identity of use does not necessitate identity of linguistic meaning.

The obvious question to put to this response is whether we have any right to assume that the English indicative conditional and its English* counterpart can be supposed to speaker-mean the same range of propositions relative to identical occasions of utterance. This is to ask whether we can fairly assume that the respective conditionals have the same use. No answer seems forthcoming.

Let us consider another example, one that concerns indefinites and the existential quantifier. It is often maintained that an utterance of the form 'Some dog escaped from the pound' is equivalent to one of the form 'A dog escaped from the pound.' Both receive the following analysis:

[an x: x a dog] x escaped from the pound.

Moreover, both utterances, evaluated with respect to contexts differing only in the choice of sentence uttered, seem to have psychological parity. The attitudes the

speaker has in uttering the one form are precisely the attitudes he would have had in uttering the other form. Let us examine the question of whether this implies the identity of the literal meanings of the respective sentences uttered.

It has been argued that indefinite descriptions have a semantically distinct referential use.¹⁸ According to such a view, a tokening of 'A dog escaped from the pound' could literally mean *both* an object-dependent and an object-independent proposition (say, in a context in which the speaker was in a position to use the description referentially). Yet, it is claimed, 'Some dog escaped from the pound' exhibits no such semantical ambiguity, even in a case where the speaker *is* in a position to use the description referentially. According to this hypothesis, there are contexts C such that 'an F is G' is semantically both referential and quantificational at C, whereas 'some F is G' is (or would be) simply quantificational at C.

In addition some have maintained that there are syntactic reasons for assuming a semantic difference. As Fodor and Sag (1992) suggest, 'an F' has properties that are rather exceptional for a quantifier. As they put it, 'an F' can move out of *scope islands* in a way that, for example, 'every F' cannot. A scope island is a syntactic position out of which quantifiers typically cannot be moved. For example, 'every senior reporter' cannot take wide scope in the following:

[11a] Few skaters knew that every senior reporter was present.

Nor can 'some senior reporters' in [11b]:

[11b] Few skaters knew that some senior reporters were present.

Yet 'a senior reporter' can (on one reading):

[11c] Few skaters knew that a senior reporter was present.

Fodor and Sag also claim that a referential treatment of indefinites captures the relevant data.

This claim is of interest, as it suggests that 'an F' and 'some F's' may have syntactic (and possibly semantic) differences that are independent of use. Yet, the following considerations dispel any idea that we have been given reason to doubt that a difference in meaning is compatible with identity of usage. First, as Ludlow and Neale (1991) point out, quantifiers exhibit varying degrees of freedom from such island constraints, so the data do not show that 'an F' is quite so exceptional as they claim. 'Several F's' and 'twenty-nine drunken F's', for example, seem to behave similarly. Secondly, much data for and against Fodor and Sag's hypothesis can be explained pragmatically. For example, the referential (or wide scope) reading of the indefinite in [11c] might be a reading that the sentence's tokening makes salient but which is not its literal meaning. Finally, and most importantly, the semantic properties of indefinites that Fodor and Sag have isolated do not carry over to quantifiers of the form 'some F's' *precisely because*

the two forms are used differently. If Fodor and Sag are correct, [11b] cannot be used at a context C to mean what [11c] would mean at C. Nor can [11c] be used at C to mean what [11b] would. But if that is so, then uses of the two forms in otherwise identical contexts will not always possess exact psychological parity.

One bit of data does seem to support Fodor and Sag's hypothesis. It seems that logical determiners--expressions that combine with an NP to form a quantified NP--cannot be attached to a quantified NP. That is, a Det + [Det' + NP] construction is not permissible if Det is a logical determiner.¹⁹ For example, one cannot say 'Each most women who enjoyed the show applauded.' On the other hand, such behavior is possible for pronouns: 'Those few women who enjoyed the show applauded.' Notice, however, that (1) 'A few women who enjoyed the show applauded' is acceptable, whereas 'Some few women ...' is not. (2) 'A few women who enjoyed the show' must be interpreted referentially. This shows that, although the two forms seem to be interchangeable, they are subtly different syntactically and, it seems, semantically.

This suggests that the best way to challenge Schiffer's claim is not to deny that semantic phenomena are wholly determined by the intentions of speakers. Rather, one should challenge the particular claim that replacing a descriptive utterance with its demonstrative counterpart in

any context would not require assuming a change in the speaker's attitudes. While this claim is plausible, it is an empirical hypothesis. Moreover, the existence of descriptive pronouns strains it somewhat. Descriptions, as we have seen, can occur as both bound pronouns and E-type pronouns. While 'that F' can also occur in such contexts, it does so distinctly less naturally than 'the F'. Of course, this does not suggest that substituting 'that F' for bound 'the F' would require an adjustment of the speaker's attitudes. But it does suggest that it would not really be that surprising were such an occasion to arise.

Yet, even though Schiffer's claim can be questioned, the Russellian is hardly in a comfortable position. For if the claim that descriptions and their demonstrative counterparts are used identically cannot be seriously challenged--and so far we have encountered no serious challenge--then the charge of arbitrariness will stick, and the hidden-indexical theory of descriptions will not be a serious option.

5. An Indeterminist Solution.

Finally, I wish to consider an account that denies the assumption that incomplete descriptions have determinate content. Wettstein maintains that an adequate treatment of incomplete descriptions must account for their semantic determinacy. Yet, this determinacy might be questioned.

After all, Wettstein argues that a speaker's attitudes, in uttering 'the F is G', do not determine a unique completing proposition. This psychological indeterminacy motivates the Russellian to investigate an account of utterances of incomplete descriptions according to which they are semantically indeterminate. The account maintains that in such a context there is no proposition that the speaker determinately means, but that there are an indefinite number of completing propositions that he *indeterminately meant*.²⁰ As previously, these completions are not articulated at the level of surface syntax. Rather, they are implicitly stated in our descriptive utterances. Furthermore, the completions are determined by contextual factors. Consequently, the theory is a hidden-indexical theory of descriptions. Unlike the previous theory, then, the current theory claims that incomplete descriptive utterances make implicit and context-sensitive *indeterminate* reference to completing descriptions.

The current account proceeds along the same lines as my account of the vagueness of belief-reports in section 5 of Chapter One. There I suggested that Kit Fine's (1975) application of *super-truth* semantics to vague sentences be extended to belief-reports.²¹ Fine suggests an account according to which a vague sentence is super-true just in case it is true for every admissible precisification; super-false just in case it is false for every precisification;

and that it is otherwise indefinite. The role of the super-truth account is to salvage compositionality: even if certain atomic sentences are indefinite, due to vagueness or indeterminacy, this does not entail that they cannot occur as constituents of complex sentences themselves possessing definite truth-values. For example, while 'a is red' may be indefinite on a given occasion of use, the sentence, Either a is red, or a is not red' (uttered in the same circumstances) ought not to possess this indefiniteness. Similarly, while a sentence containing an incomplete description may possess no definite truth value, it can occur in sentences that do possess definite truth values. For example:

The professor is either a philosopher, or she is not a philosopher.

The book is on the table only if *some book* is on the table.

As suggested above, the theory is also motivated by the fact that our characterizations of the linguistic intentions of speakers uttering incomplete descriptions seem indeterminate. For any utterance of [7]

[7] The book is on the table,

there exist an indefinite number of eligible completions. Any choice among these will go beyond what the speaker can be supposed to have intended. The obvious resolution is to assume that the speaker, in uttering [7], *indeterminately* meant each admissible completion.

Obviously, such psychological and semantic indeterminacy is a pervasive feature of the propositional attitudes of speakers in a wide variety of linguistic contexts. On the one hand, countenancing such indeterminacy is semantically unproblematic. I am suggesting that certain utterances typically fail to determine a unique literal content. That is, their linguistic meaning, or character, fails to yield, in all circumstances, a definite proposition. Yet, their character is perfectly well defined: 'the F is G' literally means the completing proposition its utterance renders most salient. If there happens to be no such completing proposition--as will typically be the case for incomplete descriptions--then the sentence will be semantically meaningless. (It will, of course, have an object-dependent meaning.) This is to be expected given the speaker's propositional attitudes; they typically will not raise to salience a unique completing proposition.

In sum: the indeterminacy introduced by incomplete descriptions does not eventuate in a non-factualism about meaning. Each sentence of English can be assumed to be assigned a rule, or character, which functions as a (partial) function from contexts to propositions. In the case of incomplete descriptions the function--whose value is defined as the unique contextually salient proposition--will typically have no value. A consequence of this view, then,

is that utterances of incomplete descriptions will be literally meaningless. Thus, it will not be the case that there is no fact of the matter as to what (incomplete) 'the F is G' means. If the utterance does not single out a completion, it is literally meaningless. Although, again, it can determinately convey an object-dependent proposition and it can indeterminately communicate an indefinite number of completions.

On the other hand, there seem to be sentences possessing definite truth values which contain semantically indefinite sentences as constituents. Yet, if we calculate the truth values of such sentences as truth-functions of their subformulae we will, contrary to the intuition, invariably assign the sentence "indefinite". Yet, as suggested, there is a way of accommodating both indefiniteness and the intuition that certain compounds indeterminate sentences possess definite truth values.

A preliminary suggestion for such a theory might be as follows. An utterance *u* of 'the F is G' is super-true just in case it is true for every admissible precisification of 'the F'--that is, every completion of 'the F'; super-false just in case it is false for every admissible precisification of 'the F'; indefinite otherwise.

Let us make this more rigorous:

[S] An utterance *u* of 'the F is G' is super-true just in case each eligible completing proposition is true;

super-false just in case each eligible completing proposition is false; indefinite otherwise.

This, I submit, provides a resolution to the problem of incompleteness. It assigns super-truth conditions to those utterances of incomplete descriptions possessing definite truth values. This shows how a minimal compositional semantics is compatible with the indefinite truth values of utterances containing incomplete descriptions.

This theory is, on the face of it, quite attractive. Yet, it faces exactly the same problem as HI-D. For the revised hidden-indexical theory of descriptions--call it HI-D₂--has a counterpart theory of demonstratives:

An utterance *u* of 'that *F* is *G*' indeterminately means each object-independent proposition *p* such that *p* is an admissible completion of *u*.

As before, it seems that HI-D₂ cannot be maintained, on pain of arbitrariness, without one's maintaining the revised hidden-indexical theory of indexicals. For, relative to any utterance situation, 'the *F* is *G*' and 'that *F* is *G*' seem to have "exact psychological parity". If, in uttering 'the piano has been tuned' I indeterminately mean each of a set Σ of propositions, then surely I would have meant each member of Σ had I uttered '*that* piano has been tuned.'

As I suggested above, it can perhaps be argued that the fact that the very same proposition(s) would have been

determinately or indeterminately meant had I uttered α at C instead of α' does not settle the question of the semantic identity of α and α' . But I also added that it seems unlikely that we can find a semantic distinction between expressions that is not in some manner reflected in their respective uses.

Conclusion.

I have considered two approaches to the problem of incompleteness. One approach argues that facts about the mutual beliefs of speakers determine the appropriate completions of utterances of incomplete descriptions. The other approach abandons the standard assumption that such utterances possess determinate content. The latter approach assimilates such utterances to utterances of vague sentences--and provides them with a comparable semantical treatment. Either option is somewhat problematic. The first strains what can be plausibly assumed to be mutually believed. The latter abandons something that should perhaps only be abandoned as final measure. But any attempt to meet head-on the problem posed by the providing of unique completions must either countenance arbitrariness and indeterminacy, or attempt to make sense of the psychological conditions that must be in place for unique completions to be selected. Neale's account has an inherent arbitrariness. Faced with this, I have suggested two possible strategies:

either to accept the arbitrariness and embrace an indeterminist semantics, or attempt to eliminate the arbitrariness by appeal to features of context. The latter is philosophically provocative in what it attempts to advance, the former in what it is willing to give up. Either way, we have provided a context for Neale's proposed solution to the problem posed by incomplete descriptions.

Yet, my conclusions have been negative. While each theory is attractive considered in itself, neither can be an option for a theorist working in the framework of direct reference semantics. For, such a theorist must maintain that demonstrative utterances are directly referential and thus express object-dependent propositions. Moreover, in adopting Russellianism, he must maintain that descriptive propositions are object-independent. Yet, intuition overwhelmingly suggests, in Schiffer's words, psychological parity between the two sorts of case.

In the next chapter, I will propose a different way of accommodating definite descriptions within a directly referential semantic theory. This theory abandons Russellianism and is, consequently, immune to the problem of incompleteness.

Notes to Chapter Two.

1. Schiffer cites James Higginbotham as first noting this connection.

2. The two formal representations of 'the F' do behave differently when embedded within the scope of a negation sign. But each can represent the different senses of a denial of 'the F is G' (i.e. corresponding to whether or not the negation is interpreted internally or externally).

3. Strawson (1950) was the first to fault the Russellian analysis for analyzing 'the F is G' as 'exactly one F is G.' Many speakers, as he observed, use the former, with full knowledge that there are multiple F's, to make an assertion about an individual F.

4. By "Russellian" I mean an account of definite descriptions according to which they function as restricted quantifiers and not as referential singular terms. I distinguish a *Russellian* account from a *strict Russellian* account in that the latter, but not the former, denies that the definite descriptions produced in actual speech situations are often elliptical for complete descriptions.

5. In fact, it becomes hard to see that there really are two objections here. If we are to suppose that in some contexts we have no way of knowing what the speaker's intended completion is, and we assume that the speaker is competent, then it must follow that he knows this fact as well. But in that case he couldn't really *intend* to have his favored completion recognized. Which is to say that there is no intended completion. Thus the 'epistemological' argument is itself a version of the indeterminacy argument.

6. This discussion is derived from Lewis (1986: 152-56), who cites Frank Jackson. I assume that it is obvious that even if the Lewis-Jackson account of the indicative conditional is ultimately inadequate, this is not due to their methodology.

7. Identical considerations apply to what Neale calls "the basic case"--cases where the intended denotation (**b**) and the object uniquely satisfying the description produced ('the F') are identical. It is argued that the basic case itself motivates the ambiguity diagnosis. Such cases meet the following conditions:

- (a) There is an object **b** such that S knows that **b** is uniquely F;
- (b) It is **b** that S wishes to communicate something about (65).

(I eliminate Neale's conditions (c) and (d) as they are irrelevant to our discussion.) There is an intuition that in such cases one's intention to produce the belief in one's audience that the F is G *just is* the intention to produce the belief in one's audience that *b* (the unique F) is G. For example, if I say 'The president will give a press conference tomorrow' I will be taken to have conveyed both the proposition that the unique president of the United States will give a press conference tomorrow *and* that Clinton will give a press conference tomorrow. But if, as some argue, these intentions are not separable, then it seems that one's intention to refer to Clinton in uttering the above sentence is part of what the utterance expresses.

This consequence would force the conclusion that some descriptive utterances have a referential meaning in addition to their attributive meaning. (It would not in itself show, however, that there were purely referential uses.)

The ambiguity theorist maintains that in this context I asserted both propositions: a singular proposition and a descriptive one. The response to this (due in all essentials to Neale [87]) is straightforward. I am about to visit a city in which I know no one. You have a friend, S, who lives there; you suggest I look her up and request that she show me the city. Later, I write you a post-card, saying: 'A friend of yours is taking me scuba-diving ...' Obviously my utterance expresses the quantified proposition, 'Someone is such that he is a friend of yours and he is taking me scuba-diving.' Yet, just as obviously, it conveys the information that S is taking me scuba-diving. Should we attribute an ambiguity to the *indefinite article* as well? This would surely be going too far: even though the indefinite description can be used, so to speak, referentially, there is absolutely no inclination to posit an ambiguity between a referential interpretation of 'a friend' and an attributive one. On this point see also Evans (1982: 321). I will consider this point again in Chapter Three.

The obvious moral is that the mere presence of referential usage does not in itself motivate the positing of an ambiguity.

8. See further, Blackburn (1988: 278, note 17) and Lewis (1983: 241). Lewis's example 'The pig is grunting, but the pig with floppy ears is not grunting' presents the same difficulty for this suggestion.

9. Reimer (1992) has additional arguments against the implicit approach.

10. Neale claims that this remedies a defect in Wettstein's account. "Wettstein's own list of complete descriptions for which 'the table' might be viewed as elliptical includes only sentences containing descriptions completed with additional descriptive material ('the table in room 209 of Camden Hall at t_1 ', 'the table at which the author of *The Persistence of Objects* is sitting at t_1 ', and so on)." This, he states, "is the weak point in his discussion" (101). Neale's remark is curious: the sample completions are all completions with an indexical component--namely, ' t_1 '. These are just the sorts of completions that Neale maintains are readily--and uniquely--derivable from the context. But as we see, contrary to the suggestion that a unique referential completion will be available, many distinct completions can be derived from the context, each of which makes reference to a contextual parameter. Perhaps these are not the right completions for other reasons. Yet, Neale doesn't say how context enters into the description to provide the appropriate completion, only that in many cases it does. So we don't have any guidelines as to how we may eliminate certain completions to arrive at the unique correct one. Without such guidelines, we seem to end up, as before, with a multiplicity of descriptions.

11. After writing the first draft of this chapter, I came across Reimer (1992), who suggests a similar difficulty for Neale's account.

12. However, I do think that something not too distant from Neale's suggestion may be correct. I will develop this idea below.

13. It is worth noting that Neale's theory of anaphora requires a solution to the problem of incompleteness. Neale goes on [in Chapters 5 and 6 of his (1990)] to claim that anaphoric pronouns that are not bound by their antecedents--so called E-type pronouns--stand proxy for definite descriptions. The descriptive content of such a pronoun is determined by a semantical rule, stating the manner in which the description is to be derived from the antecedent. Yet, often, the rule delivers an incomplete description. For example, the rule states that 'her' in the following is to be interpreted as 'the friend of John who writes':

John has a friend who writes. George is jealous of her.

But perhaps John has many women friends who are writers. If so, then Neale's analysis of the second sentence will be vacuously false. The obvious solution would be to take the description provided by his rule and add a completion. But

this requires an account of how context can supply a unique completing proposition.

14. Loar argues here for a Russellian treatment of all singular terms. But, as I will make clear, such an account is quite at odds not only with direct reference semantics, but with standard data regarding belief reports.

15. This is not to deny that a singular proposition can also be part of the utterance's (non-literal) meaning.

16. I assume that these situations are, from the speaker's point of view, exactly like the actual context of *u*.

17. This overall strategy for examining Schiffer's claim is due to Kripke (1977). I make use of it again in Chapter 3.

18. For example, Chastain (1975).

19. I am indebted to Ruth Reeves for this information.

20. The expression is Schiffer's; see his (1994).

21. The super-truth account derives from Bas van Fraassen's work on supervaluations. See van Fraassen (1966). Hartry Field (1974) develops an alternative approach. While van Fraassen and Fine generalize the truth predicate, Field suggests generalizing semantic notions such as reference (his surrogate is *partial reference*). This allows for a treatment of the truth predicate compatible with a correspondence theory of truth.

CHAPTER THREE

Definite Descriptions as Referential Singular Terms: Some Gricean Considerations

Introduction.

In this chapter I want to examine the thesis that definite descriptions are directly referential. By this I mean that utterances of the form 'the F is G' express object-dependent, or singular, propositions. This is an uncommon view; the standard position among those who hold that descriptions can be semantically referential is that descriptions are ambiguous between a referential and an attributive interpretation.¹ A distinct virtue of the referential thesis is that it dispenses with this alleged ambiguity. But what makes the thesis of immediate interest is two-fold: First, it is the most obvious--and, I will argue, the only--treatment of definite descriptions within the context of a directly referential semantics. Second, acceptance of the alternative Russellian account, as we have seen, seems to force acceptance of a description theory of demonstratives; and the latter is undesirable. In addition to these considerations, it will be shown that the theory explains analogies between descriptions and demonstratives, avoids the problem of incompleteness, and provides a more

satisfying account of descriptive pronouns than the Russellian view. Along the way I will criticize, in some detail, the major competing account, due to Stephen Neale (1990). Neale maintains that definite descriptions are Russellian, and that they are best understood as quantifiers. We have already seen (in Chapter Two) the problems facing Neale's Russellian solution to the problem of incompleteness. In this chapter, I will examine more fundamental arguments in favor of the Russellian account in general, and its quantificational interpretation in particular. I will show that there is nothing in them that favors a Russellian over a uniformly referential account. Gricean considerations do not tell against the referential thesis, contrary to what Kripke, Neale--and Grice himself--would lead one to suppose.² Indeed, I will suggest that Gricean considerations may actually lend support to the referential view. The case is far from decisive, however, and my more immediate aim is simply to do for the referential thesis what Kripke (1977) has done for the Russellian thesis--namely, show that it comports with the use of definite descriptions as well as do competing accounts.

The structure of the chapter is as follows. Section 1 provides some motivation for the referential thesis. In section 2 I show how the theory is compatible with the behavior of descriptions in that-clauses. In sections 3

through 5 I reconsider Neale's arguments in favor of the quantificational analysis of definite descriptions. This is important, as Neale presents the most elaborate statement and defense of a broadly Russellian treatment of definite descriptions in the literature. 3 and 4 critically discuss the formal considerations that Neale claims favor his system of restricted quantification and its handling of descriptions; 5 examines the role of the quantificational interpretation of descriptions in Neale's attempt to defend Russell against the argument from incompleteness. It is concluded that the Russellian has no defense against the referentialist if he is deprived of the assumption that definite descriptions are quantifiers. In section 6 I consider, and reject, Gricean arguments in favor of Russell's theory. Finally, I compare, in 7, Neale's theory and the referential theory on the subject of descriptive pronouns, arguing that the referential theory provides the best treatment of the data.

1. Speaker's Meaning and Semantic Theory.

1.1 Evaluating Semantic Hypotheses.

What, then, is the theory? The first point to be clear on is what the theory is not. I am not maintaining simply that 'the F' as it occurs in an utterance of 'the F is G' is semantically referential. To say this is to not yet to say that the proposition the utterance expresses is object-

dependent. After all, Frege and Searle (1979) take descriptions to be referential singular terms, but the propositions expressed by descriptive propositions are, on their view, object-independent.³ What I claim is that 'the F' is *directly* referential; it introduces its referent, and nothing else, into the proposition the sentence containing it expresses. The result is that the proposition expressed will be object-dependent.

The theory, then, can be encapsulated by the following claim:

[Th] An utterance of 'the F is G' expresses an object-dependent proposition.

Stating the claim is easy. Unfortunately, assessing its precise significance is less straightforward. The question to ask ourselves at this point is: What would make this thesis true? A reasonable first step towards answering the question is to arrive at an answer to a more general question, and to go on from there. That question being: What makes a semantical hypothesis true?

To answer this, let us consider the following picture of language, due to David Lewis (1983a). A language *L* is a function from sentence-types to sentence meanings. For each such type--which we can think of as a structural description of a sentence--*L* determines a meaning. What, then, is a sentence meaning? Following Strawson (1950) and Kaplan (1989), we can take them to be general directions for use (Strawson) or characters (Kaplan). These directions, or

rules, can be represented as functions--in particular, functions that assign propositions to sentences, relative to a context of utterance. In sum: a language *L* is a function from sentence-types to characters, themselves functions from contexts to propositions.

Now, what, it might be asked, is the relation between the function corresponding to *L* and the linguistic behavior of a given community? That is to say, how do we know when a given language is, in fact, *the language* of a particular community?

What one wants to say is that it is in virtue of how a certain expression is used by a given community that determines which semantical rule or character it expresses. Barring objections having to do with abnormal uses, this seems correct--even, as Lewis and Schiffer maintain, platitudinous. In more detail, what they maintain is that there is a relation *R*--following Lewis, the *actual-language relation*--between a community *C* and a language *L* such that *L* is the language of *C* just in case $R(L, C)$. As Schiffer remarks, determining what *R* is will enable us to understand "in what sense, and how, meaning supervenes on the practices and psychological states of language users" (Schiffer, 1993).

For my purposes, it is enough to assume that a true, non-trivial specification of *R* is possible; that is, in some sense, and in some way, meaning supervenes on use. If this

assumption holds, then an answer to the question *what makes a semantical hypothesis true* is that it conforms to the function associated with the language to which we (or the relevant community of speakers) bear the *R*-relation.

(Semantical hypotheses are, I am assuming, hypotheses about the rule associated with expressions people actually use. They are not hypotheses about languages considered in abstraction from language-users.)

The answer to the first question should by now be obvious. Our thesis [Th] is true--of, say, the language that members of the English-speaking community speak--just in case the function associated with that language assigns a directly referential meaning rule to sentences of the form 'the *F* is *G*.' With these (more or less stipulative) matters settled, let us clarify the question that must be addressed in order to evaluate [Th]--namely, what would make *it* true?.

Consider a language L_1 possessing the syntax and semantics of English, with the one possible difference that, according to the semantics of L_1 , R_1 is true by stipulation:

R_1 . utter 'the *F* is *G*' just in case you mean that some particular individual *a* is *G*, where *a* is the most salient candidate for the referent of 'the *F*'.⁴

L_2 , on the other hand, is identical to L_1 except that, in L_2 , it is R_2 and not R_1 that is true by stipulation:

R2. utter 'the F is G' just in case you intend your audience to believe that the ϕ is G (where 'the ϕ ' is a contextually definite completion of 'the F')
Our question is: Which of $\{L_1, L_2\}$ is in fact the language of the English-speaking community? I want to consider seriously the response that R1 is the rule that the use of 'the F is G' exemplifies--that we speak L_1 . But first, some disclaimers.

While in some sense obviously true, the suggestion that a meaning hypothesis is wholly determined by facts about the practices and propositional attitudes of speakers is misleading. The problem is that speakers typically use sentences of the form 'the F is G' to communicate both object-dependent and object-independent propositions. This suggests that a decisive case for one or another hypothesis as to the *linguistic meaning* of definite descriptions is not likely, for the uses of descriptive sentences do not exhibit compliance with a uniform constraint or rule. That is, the use of sentences of the form we are concerned with is compatible with a number of ways of construing the meaning function they encapsulate. The function might be one from contexts of use to object-dependent propositions (the referential thesis), or from contexts of use to object-independent propositions (the Russellian thesis). Finally, there might be two distinct meaning functions associated

with such sentences (the ambiguity thesis). That we reject the latter hypothesis for methodological reasons doesn't rule it out as a *logical* possibility.

That said, this diversity of use does not preclude the possibility of a uniformly referential interpretation. The above comments were simply to place this hypothesis in its proper light. Again, my primary concern is to establish that the referential thesis is a viable alternative to the competing accounts. This does not, however, prohibit me from making the strongest case I can for the referentialist. (But, again, the reader cannot expect any case for or against the thesis to be definitive.) Accordingly, the following sections will not be restricted simply to showing the compatibility of the referential view with linguistic usage or with how facts that seem to support a Russellian analysis are more equivocal than they might first appear. They will also indicate where the Russellian account is noticeably weak in comparison with the referential account.

1.2 How to Have a *De Re* Intention.

Consider again the claim that R1 states the correct meaning rule for definite descriptions. This claim seems vulnerable to the following criticism. 'The "meaning rule" that R1 assigns to descriptions fails to accord with the fact that speakers often utter sentences of the form 'the F is G' to mean object-independent propositions. A rule (such as R2)

that accords with this fact about usage seems preferable to one, like R1, that does not. This suggests, other things being equal, that R2 is the correct meaning rule to assign to definite descriptions.'

What this obvious criticism misses, however, is the not-so-obvious point that one way of using descriptive sentences in accordance with R1 is to use them to mean an object-independent proposition. That is, one way of having a *de re* linguistic intention is to have a *de dicto* intention. This is an important point. Our informal practices of specifying the contents of others' assertions and beliefs exhibit a considerable degree of independence from what, if any, *de dicto* attitudes they might have.⁵

How does this bear on the current topic? As Schiffer (1981) points out, that a speaker means such-and-such in producing an utterance is a propositional attitude property of that speaker and, as such, admits of *de re* and *de dicto* specifications. That is, meaning that such-and-such, just as believing, can be understood either as a relation to a complete proposition, or as a complex relation between a speaker and an individual. For example, I might utter 'The tallest man in the room is a poet' with a *de dicto* intention: to get you to believe, partly in virtue of your recognition of my intention, that [*] *exactly one man in the room is taller than all the others and that any men taller than all the other male room occupants is a poet*. Does that

mean that I have not conformed to R1? It hardly seems so. After all, it might have been my intention that you form the belief that a--the intended denotation of my description--is a poet by first forming the belief that [*]. This, then, is one way in which you can have a linguistic intention *de re* by having a linguistic intention *de dicto*.

Furthermore, it is quite possible that H can ascribe an object-dependent belief/assertion to A on the basis of A's purely object-independent assertion. Certainly, our practices of belief/assertion ascription support this claim. For example, imagine the following, asserted by someone (call him 'Smith') ignorant of the identity of the fastest runner alive, but certain that he must be African.

[1] The fastest runner alive is African.

This might be reported to Jones as follows:

[2] Smith said that you are African.

This shows that Smith's (*de dicto*) communicative intention admits of a *de re* specification. For, it seems that I can have spoken truthfully in saying what I did. (In 2.1 we will see more clearly what the truth of such reports turns on.) And what I said is true just in case Smith's intention admits of a *de re* specification. This is the case even though it does uncontroversially admit of a *de dicto* specification as well. In sum: one way of having a *de re* intention that such-and-such is to have a *de dicto* intention that such-and-such.

Now someone might argue that the converse is also the case--that one way of having a *de dicto* intention is to have a *de re* intention. And this could be used to show that no adequate defense of either view is possible. Yet, the following argument shows this suggestion to be incorrect.⁶ I may have the intention to mean that *a* is *G* in uttering 'the *F* is *G*.' But it cannot be assumed that I could mean the expansion of 'the Φ is *G*' (where 'the Φ ' completes 'the *F*') in virtue of meaning that *a* is *G*. At best, one might argue that there is a property Φ^* such that *a* is uniquely Φ^* and such that, in uttering 'the *F* is *G*,' I intended my audience to entertain the proposition expressed by the expansion of 'the Φ^* is *G*' and that I had this intention in virtue of my intention to mean that *a* is *G*. But this is far from the same thing. For, in the latter case, my act of meaning does not receive a *de dicto* specification; there is no unique Russellian completion I intended to mean. Rather, I gave my audience instructions (so to speak) for selecting the completion that suited her (namely, that it had to be *true of a*). In the former case, however, my act of meaning does receive a *de dicto* specification.

This argument makes use of a rather obvious point: An incomplete description plus an intended referent does not inevitably yield a unique completion of that description. (I ignore, for the moment, the possibility of a response along the lines of the Russellian in Chapter Two, 4.) This

shows that there cannot be a reduction of *de re* meaning acts to *de dicto* meaning acts--that, in a word, *de re* intentions are irreducible.⁷ Moreover, if there is no reduction, then there *is* an issue as to which proposal best accords with facts about speaker meaning.

With this in mind, notice that R2 requires that for any use of a description, the speaker's act of meaning must receive a *de dicto* specification. Yet, this fails to comport with how descriptions are used. I might intend that my audience associate *some* descriptive proposition with my utterance, without there being any descriptive proposition such that I intend my audience to associate *it* with my utterance.

Now the Russellian can respond, not altogether implausibly, that while such utterances do, strictly speaking, constitute a violation of R2, they are entirely explicable on the assumption that R2 gives their literal meaning. Consequently, he can maintain, the uses I am considering do not succeed in refuting R2. That is, the Russellian can hold that the audience can reason as follows (in the context of an utterance of (say) 'The waiter sneezed', where no unique completion is raised to salience):⁸ 'According to R2, A's utterance should be used, other things being equal, to mean a complete object-independent proposition. But A's utterance, together with features of the context, does not determine such a

proposition. Thus, A cannot have intended for me to recover a particular object-independent proposition from his utterance. But, given that she has used a rule according to which such a proposition should be recoverable, and given that it is manifest that none is recoverable, she must intend for me to entertain another proposition. The most salient candidate is the proposition constructed from an individual concept *that I associate--relative to this context--with the description produced* (here, 'the waiter') and the property of having just sneezed. *relative to the context:* the individual concept I associate with the description is selected by contextual information, including, crucially, the referent of the description.'

The problem is that this rejoinder, even if it is convincing, simply will not work for certain cases--namely, cases in which the description is incomplete but where the intended referent is not manifest to the audience. In such cases, I cannot expect the audience to associate an appropriate proposition with my utterance. The reason for this is that I am unable to constrain the completions I want my audience to entertain. For example, in the imagined utterance of 'The waiter sneezed,' the speaker makes reference to an individual in constraining the completion that the audience is to associate with the description. In what I will call (with some misgivings) purely attributive cases, no such object is present.⁹ In consequence, the

audience will have no guidelines as to how to select the completion.

This, it seems, provides the basis of a defense of the referential view over the Russellian view. Let me show this in more detail. Consider again Donnellan's example of the Temperance Union leader who utters [3], ignorant of the denotation of the description:

[3] The man drinking the martini is breaking the rules of this club.

While [3] is incomplete, the context provides no unique completion. Many completions will be made salient, yet none especially so. (Recall Blackburn's discussion of Wettstein, quoted in Chapter Two, 3.2.) This indicates that the hypothetical speaker violated R2--he had no completion in mind. Consequently (assuming R2) there is no determinate literal content to his utterance. Moreover, it is not at all clear that pragmatic facts help to explain the appearance of meaningfulness. In other words, the Gricean view that a Russellian theory is, at very least, descriptively adequate seems falsified by this datum. For, the assumption of R2 together with the available contextual information simply fails to determine a unique completion. In addition, it fails to provide a constraint on the proposition the speaker is to associate with the utterance. The last point is important; it shows that the Russellian's rejoinder of two paragraphs back is inapplicable here.

Of course, a Russellian of the sort characterized in Chapter Two, section 4, would maintain that it is the conjunction of mutually available completions that is intended as *the* unique completion of the description produced. But in contexts in which *what counts as a completion* is underspecified, this suggestion will not work. Let me explain. I might be unsure whether it is compatible with the speaker's intentions that he meant, in uttering [3], (i) *the only man to have drunk a martini at this meeting* is violating the rules of this club, or whether he meant (ii) *the man who is currently drinking a martini at this meeting* is violating the rules of this club. Nothing the speaker says rules out either. Yet, the sentences are inequivalent. If a man not currently drinking a martini drank one at some point earlier in the evening, and another man *is* currently drinking a martini, then (i) is false, whereas (ii) will be true. Thus, even if we assume that the speaker implicitly intended one of the completions, he cannot be assumed to have intended their conjunction. That is, unless we make another assumption that is not guaranteed by the context--that only one man drank a martini at the meeting.

Another example: It is Oscar night. The host discovers that the Oscar to be awarded to the next winner has been stolen by a previous recipient. He announces the theft, and adds:

[4] The person who stole the Oscar is in this room. Perhaps he intended, as a completion, (iii) *the person who just stole so-and-so's Oscar* is in this room, or perhaps he intended (iv) *the only person who stole an Oscar this evening* is in this room. Nothing that was said allows the audience to decide between the two. Can he have meant both? That is a third option: *the person who just stole so-and-so's Oscar and who is the only person who stole an Oscar this evening*. But notice that it is not a default option, as it says something that might be false even if a completion of [4] along the lines of (iv) is true (i.e., multiple Oscars were stolen, and so-and-so's was just stolen as well).

Note that if it was evident that the announcer believed that only one person stole an Oscar, then his intention could have been construed in any of the three ways, with, perhaps the default option being the most preferable. For, the three completing descriptions would on that assumption be co-denoting. But there is no requirement that belief in this proposition be mutually manifest for the speaker to say what he did. Indeed, upon being asked whether he believed that exactly one person stole an Oscar that evening, he might say that he neither believed nor disbelieved that proposition.

Thus, we can conclude that there really is no Russellian response to the problem of incomplete attributive descriptions.¹⁰

How does this bear on the problem concerning the proper semantic treatment of definite descriptions? Recall that I am assuming that questions as to the proper treatment of definite descriptions are questions about what language we speak--whether, in this case, we speak L_1 or L_2 . What decides this question is the use speakers make of descriptions. The problem the referentialist faces is that it looks as if this use conforms to R2--the Russellian model. The response I made--appealing to Schiffer (1981)--was that one way of performing referential speech acts with descriptions is by performing speech acts that can be specified by a complete, object-independent proposition. This is, in fact, borne out by our practices of assertion and belief reporting, which commonly provide *de re* paraphrases of acts of meaning that also admit of *de dicto* paraphrases. Nothing said so far necessarily favors a referential account; it shows simply that the referential account is compatible with the data. Yet, I note that the Russellian has no general account of incomplete attributive descriptions consistent with his meaning rule. In some contexts--the Donnellan case is an example, as is [4]--the Russellian assumption, together with contextual information, fails even to implicate a Russellian proposition that can

plausibly be considered the content of the utterance. It simply provides the incomplete, vacuously false expansion of the sentence produced; and this cannot plausibly be taken to be its content.

1.3 Incompleteness and the Referential Thesis.

I have just argued that R2 is not fully compatible with our use of descriptions. In a certain class of contexts the assumption that R2 provides the uniquely correct meaning rule for sentences of the form 'the F is G' does not seem compatible with their possessing any meaning, literal or otherwise. Do such contexts also provide a difficulty for R1? I will argue here that they do not. The following is the only serious argument that such contexts do provide a difficulty for the referentialist.

'The speaker uttering [3] fails to make manifest precisely to whom he is referring. Yet an act of reference is not successful unless the speaker somehow succeeds in making manifest to which object he is referring. That is, information sufficient to identify the referent must be made available by an utterance for it to properly count as referential. Consequently, the utterance of [3] fails to qualify as referential.'

There is a simple response to this argument. The assumption that 'reference cannot succeed unless ...' is unwarranted. Surely, it might be objected, I do not have to

provide my audience with a means for identifying my referent in order to succeed in referring. (What I *must* do is enable them to make the identification relative to the discourse, but that is a different point.) I might, for example, utter the following: 'I have a friend--John. John is an artist. He lives downtown.' Surely I succeed in referring even if I have another friend who is named John, is an artist and lives downtown. That is, it seems clear that I can refer, and can be understood as referring, to John without providing information individuating John in my utterance.¹¹

In sum: The assumption that descriptions are devices of direct reference is fully compatible with the use of incomplete attributive descriptions. I now turn to a comparison of the two theories with respect to belief reports.

2. Evaluating the Referential Thesis.

2.1 Definite Descriptions in That-Clauses.

I will now argue that the referential interpretation of definite descriptions is compatible with their behavior in that-clauses. I will also suggest some ways in which the assumption that that-clauses are referential singular terms favors the referential interpretation of definite descriptions over the Russellian account.

When speakers report the content of utterances of the form 'the F is G', they often abstract from the description used. Either an alternative description is used in its place, or a proper name or demonstrative is substituted for the description. Sometimes, of course, the speaker will adhere to the precise words the person uttered or would assent to. The hidden-indexical theory, as we have seen, can explain the fact that the substitution of a that-clause *t* by co-referential *t'* in a belief report 'A believes *t*' can alter its truth value. (Recall that we assume that that-clauses are referential singular terms.) For, according to the hidden-indexical theory, how one reports a belief affects the content of the report itself. What I want to argue is that the fact that belief reports permit only certain co-referential substitutions for descriptions is entirely explicable on the assumption of R1. The hidden-indexical theory provides an explanation of such substitutivity failures.

Consider the following example. Geena utters the sentence:

The first lady is an attorney.

I can report this as follows:

Geena believes that Hillary Clinton is an attorney

Geena believes that she [pointing to Hillary Clinton]
is an attorney

Each report, it would seem, is at least potentially true given the verbal evidence. If so, then we have support for

the claim that abstracting away from a description δ in reporting the assertion made or belief expressed in an utterance of ' δ is G' is (in some cases) semantically permissible.

Yet, it may be said that there are cases where such abstraction is not permissible. Geena might utter the following:

Clinton's successor will be a Democrat.

It seems that this cannot be reported as a belief about an individual. For, let us assume, implausibly perhaps, that Gore will succeed Clinton to the presidency. Surely we cannot report Geena's contemporary assertion eight years hence as follows:

Geena believed that Gore would succeed Clinton.

Geena believed that he [pointing to Gore] would succeed Clinton.

But notice that the falsity of these reports can be explained by appeal to the hidden-indexical theory. It is quite possible that, for some m , Geena believes [Gore will succeed Clinton] under m . But the m in question will not be of a type that a tokening of either of the above reports makes salient. For, what they make salient is a style under which Geena *fails* to believe the relevant proposition.

Notice, also, that while failure of abstraction (so to speak) is explicable for the referentialist (he need only appeal to HI), a related failure is not explicable for the Russellian. That is, the referentialist can explain why

some belief/assertion reports can fail to abstract away from the linguistic contexts that constitute their evidence.

These reports raise to salience styles that misrepresent how the ascribed proposition is being represented. The

Russellian does not have available a parallel strategy. For example, Geena utters:

[1a] Hillary will succeed the president.

According to Russell, the following reports will always be false (given the stated evidence):

[1b] Geena believes/said that the first lady will succeed the president.

[1c] Geena believes/said that Clinton's wife will succeed the president.

Neither of these reports relate Geena to the proposition that the Russellian takes her utterance to be asserting.

Another example: Jones sees Martin Amis sitting in a diner on 44th Street. He reports this by saying 'Martin Amis is sitting in a diner on 44th Street.' I report *his* assertion by saying:

Jones said that the author of *Money* was sitting in a diner on 44th Street.

Surely, we want to say that there are some contexts in which Jones's utterance makes my report true.

It seems, then, that the behavior of that-clauses containing descriptions is best explained on the referential account.

Now a Russellian may suggest a "wide scope" solution. Given Geena's assertion [1a], he may claim that [1b] can be

true according to his reading, if 'the first lady' takes wide scope. This results in [1b]'s being assigned the following logical form:

[1d] [the x: first lady(x)] Geena believes/said that x will succeed the president.

Moreover, [1d] implies that 'Geena believes that Hillary will succeed the president' is true. Thus, the Russellian seems to have a parallel strategy: There are contexts in which we can report an assertion of 'the F is G' by using (e.g.) 'the H is G' (where 'F' and 'H' are co-denoting); but these are cases where the quantifier takes wide scope. That is, reports like [1b/c] which appear to relate Geena to a descriptive proposition may not in fact relate her to a full proposition at all. Nonetheless, such a report might directly imply--by detaching the quantifier and assigning the appropriate referent to the variable left free--a report that relates Geena to a complete (singular) proposition.

There is no knock-down argument against such a response. But the following two considerations show the response to be decidedly unattractive. First, for our purposes--namely, to accommodate definite descriptions within a directly reference semantics--it is unacceptable. Recall that a fundamental feature of the semantics of direct reference is that that-clauses are referential singular terms. This seems the only view of the semantics of that-clauses compatible with the assumption that English and other natural languages possess a compositional meaning

theory. Thus, since on the Russellian wide-scope strategy some that-clauses are not referential singular terms--namely, those occurring in belief reports whose descriptions take wide scope--we are obliged to reject the strategy. This is not to say that the Russellian must accept my view of that-clauses. My claim here is simply that, assuming that that-clauses are referential singular terms, the wide scope strategy is unacceptable. I have already argued for the assumption.¹²

Secondly, as Kripke (1977) and Neale (1990) have shown, the wide-scope reading of contexts containing definite descriptions is not equivalent to a referential reading. Recall that to interpret a context referentially is to interpret it as expressing an object-dependent proposition. If a context can be given a wide-scope reading that is object-independent, we have evidence that the notions are not equivalent. (Both of the following examples are derived from Neale and Kripke.¹³)

Consider a case where a speaker (i) knows that there is an object-dependent proposition *p* such that an individual *A* believes *p*, but (ii) cannot specify the particular object-dependent proposition that *A* believes. For example, suppose that Jones has taken some unidentified persons hostage. The police enquire about the well-being of the hostages. Jones responds: 'A few of them are alive.' Clearly the following would be true:

[few x: x a hostage] Jones said that x is alive
That is, Jones has said of a few hostages that they are
alive. We can see that this sentence is object-independent:
it would be true at a world in which the set of Jones's
hostages is disjoint from the set of his actual world
hostages--so long as he said of a few non-actual hostage
that they are alive. Thus we have evidence of a wide-scope
belief report that is nonetheless non-referential.

Another example involves indefinite descriptions. I
overhear Geena say, about a movie that she has just seen,
that it starred Anthony Hopkins. Consequently, I have
evidence that Geena believes an object-dependent
proposition, but I cannot identify the proposition in
question. This gives rise to the following belief report:

Geena said that a movie she just saw starred
Anthony Hopkins.

[an x: x is a movie] Geena said that (she saw x
and x starred Anthony Hopkins).

That is, Geena said, of some movie that she had seen, that
it starred Anthony Hopkins. Again, the report is non-
referential. Had Geena said of another movie that she had
seen it and it starred Anthony Hopkins the utterance would
have been true. Yet, the quantifier intuitively takes wide
scope.

These examples show quite clearly that a referential
reading of a quantifier fails to follow from a wide scope
reading. Consequently, a wide-scope reading of a definite

description cannot be construed referentially. Thus, we have some motivation for the referential theory. It explains the behavior of descriptions in that-clauses in a manner compatible with our assumption that that-clauses are referential singular terms. The Russellian theory, on the other hand, seems incompatible with this assumption.

Yet, a Russellian might use the fact that *de re* reports of *de dicto* attitudes are permissible to argue as follows. 'You are correct in maintaining that a referential report of 'A believes that the F is G' does not follow from a wide-scope reading of 'the F'. But you have granted that we can give a *de re* specification of A's belief. Given that a referential report gives a *de re* specification of belief contents, it would follow that such reports are permissible.'

Now it is open to the Russellian to adopt my rather deflationary reading of the *de re*. She may argue that, since I allow for *de re* readings of descriptions in content clauses, she can do the same. I, of course, take the fact that such readings are possible to motivate the referential account. But the Russellian can respond that our "talk about beliefs" is just so much talk; that arguing from our practices of belief reporting to claims about the contents of belief is questionable. There is no logic of belief as such. There are just contextually admissible ways of characterizing beliefs. The fact that the relevant

ascriptions are often referential is simply a by-product of the fact that, in general, *de re* ascriptions are admissible.

The force of this argument is undeniable. Even so, if we deny the assumption that we can make inferences about the contents of attitudes based on facts about attitude *reports*, the referential theory is still descriptively adequate, if not uniquely so. On the other hand, if the assumption is made, the referential theory seems preferable to the Russellian alternative.

2.2 Definite Descriptions and Demonstrative Pronouns.

Another positive consideration favoring the directly referential analysis is that it explains the surface similarity of the description 'the F' and the corresponding demonstrative construction 'that F'. Each has a referential qualifier. Yet intuition suggests that an utterance of 'he is wearing a turban' (or '*that man* is wearing a turban') can literally mean a proposition of the form 'a is F' even if a is not male. That is, an utterance containing a pronoun is not defective--does not fail to make an assertion--simply because its referential qualifiers are not satisfied by the intended referent. Similarly for descriptive utterances on the referential view. An utterance of 'the F is G' does not necessarily fail to make a true assertion even though the object referred to is not, in fact, F.

There is the following objection: 'On your analysis 'the F is G' does fail to make an assertion if no object is referred to. You maintain that descriptive utterances assert object-dependent propositions. Yet, one positive feature of Russell's account is that it explains the intuition that 'the F is G' conveys a proposition even in the absence of a denotation--that it is object-independent.'

Yet, this seems easy to respond to. Consider an utterance of:

[1] The man just left.

Here is the context: My friend is delirious, having a temperature of 104 degrees Fahrenheit. No one besides the two of us has been in the room for quite some time. She tells me that a man has brought her flowers. I ask her where the man is. She responds by uttering [1]. Now perhaps someone has the intuition that what she said was that a unique man that is ... just left (where the ellipsis indicates a relevant completion), but most will have the intuition that she expressed no proposition in uttering what she did--that [1] and its demonstrative counterpart, [2], are equally devoid of (literal) content.

[2] That man just left.

Moreover, if the hypothetical Russellian was adamant about her intuition, the referentialist could simply respond that all that the intuition implies is that the Russellian expansion of [1] was implicated by the utterance, and not

asserted. That is, the Russellian cannot argue solely from intuition that [1] is not semantically referential. For the intuition can be explained without appeal to that sentence's semantic properties. Thus, there really is no serious Russellian objection to the claim that [1] and [2] are content-identical.

Neale thinks otherwise. First, he denies that [2] could be used to express a descriptive, object-independent proposition. For the intuition is strong that the relevant utterance of [2] could be true at worlds where the best descriptive candidate for its content is false. (Although, again, intuition itself cannot settle the matter. Perhaps what we are evaluating relative to alternative possible worlds is a singular proposition that the utterance has merely *implicated*.) From this consideration alone Neale draws the conclusion that the utterances of [1] and [2] are not content-identical:

It is clear, then, that a sentence of the form 'that F is G' is semantically very different from a sentence of the form 'the F is G'. An utterance of the former expresses an object-dependent proposition; an utterance of the latter expresses an object-independent proposition (Ibid, 70, emphasis added).

The final claim, of course, has not been established, and this leaves the conclusion unsupported.¹⁴ Neale does not offer an argument for the claim because he explicitly assumes that descriptions are quantifiers. This assumption will be considered in detail below.

An utterance of a definite description ... will not take an object as its semantical value, *because a description is a quantifier not a referring expression*. The semantical value of an utterance of 'the F is G' is a descriptive proposition to the effect that there is one and only one thing that is F and that one thing is G. There is no object for which the grammatical subject 'the F' stands that is a genuine constituent of that proposition (Ibid, 71-72, emphasis added).

Yet, Neale has no argument against the assimilation of 'the F' to 'that F' independently of the assumption that descriptions are (uniformly) quantifiers. Dropping the assumption, it is open to us to take both [1] and [2] as expressing object-dependent propositions. Moreover, we can see that even independently of the quantificational assumption, the Russellian's argument is weak. It relies on intuitions about modal properties of descriptive sentences (such as [1]) that are not clearly semantic, as opposed to pragmatic.

For example, the Russellian would argue that the proposition expressed by an utterance of 'the F is G' is object-independent, since it could be true at a world *w* at which the actual denotation of 'the F' either does not exist, or is not F. Consequently, specification of its content or its truth condition--relative to *w*--is not dependent on the existence of a particular object at *w* or on what attributes it might possess there. As an example, take:

[3] The first postmaster general had a pot-belly.

The Russellian holds that this would express the same proposition it does at the actual world at a world in which the first postmaster general was Thomas Paine and not Ben Franklin. Moreover, it would be meaningful at a world in which the description is vacuous. If the Russellian is correct, then [3] is object-independent by definition. But note that the Russellian's evidence for his claim does not include evidence that what [3] is used to assert is object-independent. But without such evidence it is open for the referentialist to maintain that the Russellian's modal intuitions concern a proposition that [3] implicates and does not in fact express.¹⁵

Yet, the Russellian may have such evidence. Consider the following sentence:

[4] Clinton might not have been the president.

Clearly, this sentence can be used to state a truth. Yet, according to the directly referential account, it must be false, as it says of Clinton that he might not have been Clinton. Now, while this (necessary) falsehood is an admissible reading of [4], it is not the only available one. Nor, for that matter, is it the most salient.

The argument is answerable, however. 'the ϕ ' as it occurs in constructions of the form 'x is the ϕ ' is not necessarily a singular term.¹⁶ Consider, first, the indefinite article in such a context:

[5] John is a philosopher.

Surely 'a philosopher' does not have its familiar quantificational sense here. Nor does it function referentially, making [5] an identity sentence. For if [5] were an identity sentence, it would be equivalent to 'a philosopher is John'. But the latter is not even intelligible.¹⁷ Rather, 'a philosopher' simply combines with the copula to form a predicate (what traditional grammarians call a "predicate nominative"). Thus, [5] has the form:

Philosopher(John)

It seems that the definite article has a similar function in the following sentence; that is, it also combines with the copula to form a predicate:

John is the author of *Waverly Place*.

But in that case it would seem strained to say that 'the author of *Waverly Place*' is referential here. Nor would it seem right to take 'the ...' as expressing a quantifier in this context. Rather, the sentence seems to have the following logical form:

$F(\text{John}) \ \& \ \exists x(\forall y(F(y) \supset y = x))$

That is, John is the author of *Waverly Place*, and there is exactly one author of *Waverly Place*.¹⁸

These considerations strongly suggest that expressions of the form 'the F', when not in singular term position, are not definite descriptions. Consequently, the fact that such

occurrences are not referential does not affect my thesis that definite descriptions are directly referential.

This dispenses with [4], but the following has a reading according to which it is non-referential:

[6a] The president might not have been Clinton.

That is, [6a] has a reading according to which it means the following (I use Neale's notation for convenience):

[7] [the x : x is president] $\Diamond \neg(x = \text{Clinton})$

[6a], then, seems genuinely ambiguous. That is, it seems to *literally mean* two distinct propositions. This is not explained by the referential account, which only supplies the necessary falsehood that Clinton might not have been Clinton.

But there is a response to this. Recall that demonstratives have generalizing uses. Loar (1976) provides an extension of the Donnellan example to show this. I see Smith's foully-murdered body and say: 'He must be inhuman.' Under the circumstances, I will be taken to mean an object-independent proposition (say, 'The person who murdered Smith must be inhuman'), whether or not I am taken to mean an object-dependent one. Schiffer (1981) provides another example. One comes across an enormous footprint in the sand and utters: 'He must be a giant.' Clearly the pronoun has no referential sense here. There are obvious variations on these cases. One might continue Loar's example as follows:

[8] And to think that this might have been avoided if handguns were illegal. *He might not have been a murderer.*

The latter sentence exhibits the same kind of ambiguity as did our previous sentences: between '*the x such that x murdered Smith might not have murdered anyone,*' and '*a might not have murdered anyone.*'

Or imagine the following. A business associate and I are to meet Jones, with whom neither of us are acquainted, at the airport. I see a man vaguely conforming to the description I have been given and greet him. Luckily, it is Jones. Later, my associate says to me:

[9] You were lucky you didn't embarrass yourself. *He might not have been Jones.*

Clearly 'he' has a descriptive sense here: '*the man you greeted*'. This latter example is derived from Nunberg (1990) (cited in Recanati, 1993).

Finally, one can imagine a use of the following in which it has the reading represented by [7].

[6b] He might not have been Clinton.

For example, I drive past the White House shortly after the president has taken residence there. I look significantly to my friend and say, '*Things might have been different. He might not have been Clinton.*'

The upshot is that, while the sentence [6a] strongly suggests that descriptions have a semantically distinct non-referential role, the same arguments seem to support the claim that demonstratives have a semantically distinct non-

referential role. And this conflicts with our assumption that demonstratives are terms of direct reference. I will not rehearse the arguments for the claim that demonstratives are directly referential. But I take it to be obvious that this claim is not refuted simply by the observation that demonstratives have non-referential uses. Consequently, the intuition that a descriptive utterance such as [6a] asserts an object-independent proposition is, in itself, insufficient to establish that it does make such an assertion.

The following interpretation of the data--that both demonstrative pronouns and descriptions have non-referential occurrences--is also available. Contrary to what I have suggested, it might be conceded that [6a] does literally express an object-independent proposition. When asked how this concession is compatible with [Th], the referentialist can respond as follows. 'Most philosophers working in semantics would agree that demonstrative pronouns are directly referential. Yet that should not preclude them from acknowledging that sentences like [6b] can *express* object-independent propositions. Similarly, while the referentialist takes descriptions to be directly referential, this should not stop him from admitting what is obvious--that [6a] expresses (in the assumed context) an object-independent proposition.' Yet, I am dubious about the coherence of this response. Until it is made clear just

how one can hold that, while α is directly referential, it can have a primary occurrence in a sentence expressing an object-independent proposition, the response seems unacceptable.

Sentences like [6a] can also receive another analysis. As we have observed, [6a] is ambiguous. And one meaning seems incompatible with the referential interpretation of definite descriptions. *Prima facie*, the interpretation in question is captured by [7]. But it is arguable that this interpretation could equally well be rendered as saying of the proposition that the president is not Clinton that it is possibly true.¹⁹ That is:

Poss(that the president is not Clinton)

Of course, the *that*-clause here is problematic: if 'the president' fails of reference, then it is unclear what semantic value to assign to the *that*-clause, since its reference is a compositional function of the referents of its constituents. But problems of this sort are shared by all theorists working in the direct reference framework. For example, the *that*-clause 'that Allah is merciful' cannot be assigned a referent in the obvious manner. Yet, 'Rushdie said that Allah is merciful' is nonetheless meaningful. In sum: I maintain that definite descriptions are semantically referential. Yet, the fact that they appear meaningfully in contexts where they fail to refer is no more of a problem

for my view than the problem of vacuous reference is for direct reference theorists generally. 'Allah' appears in meaningful utterances, yet this is no longer widely perceived to be a stumbling block to the theory of direct reference.²⁰ In any event, it is a problem that we have already acknowledged and consequently does not provide new considerations against the theory.²¹

In any case, we have two responses to the Russellian challenge concerning alleged non-referential occurrences of definite descriptions. The first argues that the Russellian has no evidence that such referential uses have semantic significance. The second shows how a certain construal of the logical form of the offending sentence as non-descriptive is compatible with the Russellian's claim that the proposition expressed is object-independent.

Yet, the Russellian has another example which seems to resist either strategy of response. Jane, let us assume, believes that Bavaria is still a monarchy, and that the present king of Bavaria is vacationing in New York. She expresses her belief by uttering 'the king of Bavaria is in New York.' Her utterance seems to provide evidence for the following claims:

- [10] Jane believes that the present king of Bavaria is in New York.
- [11] Jane said that the present king of Bavaria is in New York.

Intuition suggests that both [10] and [11] are literally true. But this entails that the that-clauses in [10] and [11] refer to an object-independent proposition. For, if they did not, the that-clause would not refer to any proposition, thus resulting, it would seem, in both assertions' being meaningless.

Note that this objection seems to preclude a pragmatic response. For, what seems clear is not simply that [10] and [11] can both be used to convey a proposition, but that they are literally meaningful. A pragmatic response can only explain the first fact. Notice also that explaining away the apparent meaningfulness of 'the present king of Bavaria is in New York' is not similarly problematic. For the response can be made that in such a case the speaker mistakes an implicature for literal meaning. The reason that this will not work for [10] and [11] is that what is being *reported* is literal meaning. Since there is no literal meaning to report, the utterance will be meaningless.

I have two lines of response to this objection.

(1) Consider the following example. Jane has a belief that Bavaria is still a monarchy and that whoever is currently the monarch is visiting the United States. Jane's beliefs are the subject of much discussion among her friends. At one point, having just spoken to Jane, one of the friends says to another:

[12] Jane said that *he* is in New York.

Again, we have here the same problem as before. There is no literal meaning to report; consequently, [12] must be--strictly speaking--literally meaningless. This fact about the use of 'he' would not cause us to abandon the assumption that demonstrative pronouns are directly referential. Consequently, similar uses of 'the F' should not cause us to abandon the thesis that definite descriptions are directly referential.

(2) The response can also be made that the intuition that [10] and [11] are meaningful in itself settles nothing. Obviously [10] and [11] can be used to mean object-independent propositions. But this is open to pragmatic explanation. The intuition that these sentences are literally meaningful should be given no more authority than the intuition as to what their meaning is. Obviously the latter kind of intuition is unreliable; otherwise doing semantics would be a simple matter of canvassing intuitions. As we know from the various uses of 'and', distinguishing meaning from conditions of use is a highly theoretical undertaking, relatively independent of questions of direct semantic intuition. Given that intuitions about an utterance's semantic meaning are not decisive, we would be well-advised to be skeptical concerning intuitions that a given utterance is semantically meaningful. Such intuitions are, after all, not immune to Gricean explanation.

In fact, a more decisive case can be made against this line of argument. For, as we have already seen, there are sentences that intuition judges to be meaningful that are undeniably meaningless--they express no proposition. If this is the case, then the claim that the referential thesis entails that certain *prima facie* meaningful utterances are in fact meaningless will be defused: this consequence will not count against the referential theory.

Consider, then, the person who uttered 'he might not have been a murderer' in our previous example. Call him 'Al'. Now assume that Smith died of natural causes; no one murdered him. We imagine someone uttering the following: (Assume that the context makes it clear that 'he' means 'Smith's murderer' without its being anaphoric on another expression.)

Some people think that all this could have been avoided. For example, Al said that he might not have been a murderer.

According to the direct reference theory of indexicals, the final claim (at least on one reading) is meaningless: 'he' has no referent. Or consider again [6b]. In this context 'he' has no referent, although its descriptive force is clear. But, as before, the (second) sentence is literally meaningless, contradicting our intuitive judgments. This shows that intuitions concerning literal meaning are fallible. Speakers can judge utterances to be meaningful which are not, in fact, literally meaningful. Consequently,

such intuitions cannot be used as evidence against the referential thesis.

2.3 Relativized Descriptions.

The observation that pronouns have a descriptive use can resolve a problem posed by sentences like the following:

[13] Everyone loves the woman who raised her.

Following Neale (34), we can call descriptions in such contexts "relativized definite descriptions". Formally, a relativized definite description is simply a descriptive phrase containing a variable bound by an external quantifier. The Russellian account has no difficulty accommodating such contexts. For example, [13] becomes:

$$\forall x(Px \supset Lx(\iota y)(Wy \ \& \ Ryx))$$

Yet, the referentialist, it seems, can supply no reading of this context that both conforms to his thesis and accords with [13]'s truth conditions. One should note, however, that relativized pronouns can have the same effect as relativized definite descriptions. (By "relativized pronoun" I mean a pronoun itself containing either a variable or pronoun bound by an external NP.) For instance:

Not everyone avoids her whom he fears.

Everyone remembers him best who treated her best.

If he who murdered Desdemona was murdered by
Desdemona's murderer, then he committed suicide.

God is that *than which nothing greater can be
conceived*.

The smallest positive integer is *that positive integer which when multiplied by itself equals itself*.²²

Of course, the occurrence of relativized pronouns used descriptively does not warrant ascribing to them a semantically distinct descriptive role. But then neither should the description-theoretic occurrence of 'the F' warrant the same ascription.

This response seems to work against any challenge exploiting the fact that descriptions can be used as logically complex singular terms--whether relativized or not. For example, consider [14]:

[14] Al loves the woman who married Jones
 $\exists x$ Al loves the woman who married x

[14] appears valid, but the referentialist must ascribe to the premise a logical form according to which the conclusion is not, in fact, a logical consequence of it.

Alternatively, if we construe the description along Russellian lines--as [15]--the inference emerges as formally valid.

[15] Loves(Al, (ιy)Married-to(y, Jones))

But consider a parallel argument, based on [16]:

[16] Al loves that woman who married Jones
 $\exists x$ Al loves the woman who married x

'Surely [16] is valid, if [14] is. And yet, while the description theory of demonstratives can account for the validity of [16], the direct reference theory of demonstratives cannot. Consequently, the description theory

of demonstratives provides the correct analysis of contexts containing demonstrative pronouns.'

Again, the observation that demonstratives have descriptive uses does not suffice to refute their analysis as terms of direct reference. By parity of reasoning, [14] should not suffice to refute the claim that definite descriptions are directly referential.

Thus, the referentialist can respond that, while it is obvious that the premise in [14] can be rendered in the standard Russellian manner, it does not follow that this rendering corresponds to the premise's literal meaning. After all, demonstratives can take on descriptive senses in certain contexts without this implying that their literal meaning is anything other than what the direct reference analysis entails.

In the next four sections I will draw some negative conclusions concerning the major Russellian alternative to the referential view--Neale's theory. In the final section I will again consider an argument in favor of the referential thesis. Since this argument relies on a comparison of the referential account with Neale's theory, I thought it best to place it after an exposition of Neale's views.

3. Choosing between Rival Semantic Hypotheses.

The referential thesis quite obviously conflicts with Neale's account as I have presented it in Chapter 2. I will argue here, however, that Neale has provided no reason for preferring his own account to a uniformly directly referential treatment (in contrast to the ambiguity account suggested by Wettstein). Consider, first, Neale's claim that descriptions are quantifiers and that the definite article is a determiner. What recommends this view? One form of response, given by Neale (and Davies, 1981), is that the standard Russellian expansion of 'the F is G', namely:

[1a] $\exists x(Fx \ \& \ \forall y(Fy \supset x = y) \ \& \ Gx)$

is "*definitionally equivalent*" (italics in text) to:

[1b] [the x: Fx] Gx

He writes: "This is not to propose an alternative to Russell's theory; it is just to find a more congenial method of stating it" (Neale, 1990: 45).

I will consider Neale's reasons for preferring the restricted-quantifier notation shortly (section IV). Before addressing the merits of this notation I want to address the general question how one decides under which logico-semantic category (e.g. *quantifier*, *singular term*) a certain grammatical category (e.g. *definite description*) should be classified. For Neale's claim seems to be that the very possibility of representing 'the F is G' as [1b] while retaining the truth conditions of [1a] is a *prima facie*

consideration for taking seriously the quantificational analysis. Yet, one should note that names can also be interpreted quantificationally. Indeed Richard Montague, and more recently, Barwise and Cooper (1981),²³ have provided analyses of contexts involving proper names in which names are classified as quantifiers. Intuitively, a quantifier is a property of properties. ' $\forall xFx$ ' asserts, of the property of being F , that it is instantiated by everything; ' $\exists xFx$ ', that it is instantiated by at least one thing. And ' $F(\text{Jones})$ ' can be construed as saying of F that it has the property of being true of Jones. The propositional content of ' $\text{Jones is } F$ ' is, on this view: $\langle F\text{-ness, the property of being a property of Jones} \rangle$. (Compare this with the standard direct reference analysis of the same sentence: $\langle \text{Jones, } F\text{-ness} \rangle$. Note that these propositions have the same possible-worlds truth-conditions.) As in the case of definite descriptions, a quantificational analysis is readily available. ' $\text{Jones is } F$ ' becomes:

$[\text{Jones } x] Fx$

This receives the obvious truth definition:

$'[\text{Jones } x] Fx'$ is true iff $\text{Jones} \in \{x: Fx\}$

At the level of logical syntax the proper name is shown to be analogous to a quantifier expression ' $[\text{det } x: Fx]$ ', rather than the constituent determiner ' $\text{det } x$ '.

Of course, that such an account is possible does not mean that it is inevitable: we do not classify names as

quantifier expressions simply because a quantificational analysis of names is available. There may be--and in fact are--competing accounts that are equally adequate.²⁴ How do we make the decision? In this case, how do we decide between a formalism that represents names as referential singular terms and one that represents them as quantifiers? The only reasonable answer is that we decide by weighing various intuitive and theoretical considerations. If a treatment of names as quantifiers comports with semantical treatments of contexts containing names better than the competing singular-term account, this would be a good reason for choosing the former analysis, all other things being equal. Of course, there are intuitive considerations to weigh against this analysis; and the intuitive judgments might give expression to theoretical considerations of another sort. For example, it might be the case that our intuitive judgments take proper names and noun phrases generally to constitute a distinct class. On the other hand, it might be argued that our best syntactical treatment of names and quantified noun phrases takes them to be members of the same syntactic category, and that any analysis of their respective logical forms should preserve this classification.

In sum, the answer to the question broached does not provide a method for achieving a unique solution to the problem: making the decision depends on the importance one

assigns to various theoretical considerations, and to intuition. Still, we have a means of approaching an evaluation of Neale's suggestion: the correct analysis strikes the best balance between theoretical considerations (simplicity and non-arbitrariness) and intuition.

What then is the theoretical/intuitive motivation for classifying descriptions as quantifiers? The case of names suggests that the mere *availability* of a quantificational analysis is not in itself sufficient to support that analysis. That is, the analysis is not to be recommended simply because the appropriate notation is available. Neale realizes this, and attempts to defend the thesis that *descriptions* are semantically quantificational--that his notational innovation should be taken at face value--by showing how his notation provides a general setting for restricted quantifiers and how descriptions, construed quantificationally, have a natural place (syntactically and semantically) within this setting. I will now turn to these considerations.²⁵

4. The Thesis that Definite Descriptions are Quantifiers.

4.1 The Syntax and Semantics of Restricted Quantification.

In this section I will criticize Neale's claim that definite descriptions are quantifiers, but I will do so independently of any challenge to the classical Russellian view that 'the F is G' is correctly rendered by [1a]. My reason for this

is that Neale's argument against the referentialist relies crucially on the claim that descriptions are quantifiers. If it can be shown that descriptions are *not* quantifiers, then the Russellian has lost his central defense against the referentialist. (I show how the Russellian defense works in 3.6). Thus, rather than attacking the Russellian head-on, I will simply deny him a key strategic maneuver.

Let us consider, then, the positive arguments for the quantificational analysis--in particular, the theoretical advantages of the restricted-quantifier notation.

One such advantage Neale cites is that [1b] exhibits a formal similarity between singular definite descriptions and plural definite descriptions. The truth clause for [1b] is as follows:

[2a] '[the x : Fx] Gx ' is true iff $|F - G| = 0$ and $|F| = 1$

For a context containing a plural description ('the F s are G s'), the following truth clause is provided:

[2b] '[the_{pl} x : Fx] Gx ' is true iff $|F - G| = 0$ and $|F| > 1$

Russell, of course, could easily accommodate plural descriptions. Indeed, as Neale notes, he could have defined a plural description operator ' (πx) ' in a manner similar to his definition of ' (ιx) '. For example, ' $G(\pi x)(Fx)$ ' becomes:

[2c] $\exists x \exists y (Fx \ \& \ Fy \ \& \ \neg(x = y) \ \& \ \forall z (Fz \supset Gz))$

Yet, the two Russellian expansions [1a] and [2c] (of the respective singular and plural contexts) do not "illuminate the relationship between singular and plural descriptions."

In contrast, the relationship is illuminated by the pair [2a] and [2b].²⁶

Furthermore, the notation accommodates descriptions with relative clauses in a syntactically perspicuous manner (47). For example, it provides a clear representation (in [4]) of the sentences in [3]:

[3a] The man who loves Martha is an entomologist.

[3b] The man whom Martha loves is an entomologist.

[4a] [the x : x is a man & x loves Martha] x is an entomologist.

[4b] [the x : x is a man & Martha loves x] x is an entomologist.

A semantics for these simple cases follows the definition

[2a]. For example, [4a] is true just in case:

[4c] $|\{x: x \text{ is a man \& } x \text{ loves Martha}\} - \{x: x \text{ is an entomologist}\}| = 0$ and $|\{x: x \text{ is a man \& } x \text{ loves Martha}\}| = 1$

Whereas [6b] is true iff:

[4d] $|\{x: x \text{ is a man \& } \text{Martha loves } x\} - \{x: x \text{ is an entomologist}\}| = 0$ and $|\{x: x \text{ is a man \& } x \text{ loves Martha}\}| = 1$

Unlike standard first-order renderings of [3a] and [3b], [6a] and [6b] are faithful to the syntax of the originals, as are the respective semantical interpretations [4c] and [4d].

Thus we have some considerations that favor Neale's notation in general, and his analysis of descriptions in particular. As we see, the notation gives a syntactically perspicuous representation of logical form. For example, it

exhibits the logical complexity of sentences containing relative clauses in a manner that harmonizes with their syntactic analysis. Consequently, the analysis allows for a truth definition that comports with the intuitive grammatical structure of the sentence. In addition, the representation of descriptions in this notation shows their semantical similarity to other natural language quantifiers. Finally, Russell's analysis of definite descriptions is preserved in this new semantic treatment.

Each of these points needs to be kept separate. Let us list them:

- [a] The restricted quantifier notation exhibits the logical structure of quantified sentences in a syntactically perspicuous manner.
- [b] Moreover, the semantical interpretation suggested by the notation exhibits semantical similarities among natural language quantifiers.
- [c] Descriptions are naturally analyzed as natural language quantifiers: when represented as restricted quantifiers they display semantical similarities with other natural language quantifiers.
- [d] This analysis retains the essentials of Russell's theory: the respective truth conditions for 'the F is G' converge.

All things being equal, [a] - [d] suggest:

- [e] Definite descriptions are natural language quantifiers.

[a] and [b] provide a motivation for a language of restricted quantification. Certainly [a] is true: the restricted quantifier notation is a far more perspicuous tool--in point of syntactic representation--than first order

logic. But [a] is inert without [b]: the claim that the semantics intuitively suggested by the formal language shows semantical regularities among natural language quantifiers. If [b] is established, and it is shown that descriptions possess semantic and syntactic traits in common with other natural language quantifiers, then [c] will be established as well. [c] claims simply that the analysis of descriptions as restricted quantifiers is theoretically plausible. In what follows I will be concerned ultimately with [c]--the claim that intuitive and theoretical considerations favor the analysis of descriptions as natural language quantifiers. All that [d] does is state that a certain constraint has been met--Russell's theory has not been abandoned. The failure to meet [d] would be a failure of the new theory to meet pre-existing semantic constraints. As such it is not a premise in an argument for [e]. It simply states a necessary condition for [e].

I concede [a]. But I will argue that [b] is misleading: there really is no compositional semantics for the language of restricted quantification in the manner presented by Neale. The similarities referred to are present only in very simple cases and fail to generalize to more complex forms. I will show, however, that a compositional semantics for (a fragment of) Neale's language is possible. Yet, this semantics directly falsifies [b]: it exhibits no uniformities among the various natural

language quantifiers. Thus, we will show that [c] is in fact false.

I will proceed by showing how Neale's own semantics for the language of restricted quantification (RQ) is inadequate.²⁷ I will then provide an alternative semantical treatment, and argue that this fails to support his view that descriptions are quantifiers.

As we have seen, RQ gives syntactically perspicuous representations of sentences with relative clauses. But there are two problems with this claim, one obvious, one rather technical. The obvious point is that the RQ treatment of descriptions is not syntactically well-motivated. The reason for this claim is that syntactically similar NP's, such as 'those Fs' and 'that F', are not given quantificational treatment. Now it is clear that these NP's are not quantifiers. But the pronouns 'that' and 'those' are, syntactically, determiners. This strongly suggests that simply because an NP has the form 'det + N' is no reason to classify it as quantificational. The only alternative is to remove 'that' and 'those' from the list of determiners. But, again, the move would not be based on syntactic considerations. The upshot, then, is that the class of quantified NP's is syntactically primitive. And this renders illegitimate any argument for the claim that something is, by virtue of its form, a quantified NP.

This suggests that classifying 'the F' as a quantifier cannot be based on its syntactic similarity to other quantifiers, for it bears this similarity to expressions which are clearly not quantifiers.²⁸

Another point, which Neale fails to acknowledge, is that, while his syntax can accommodate descriptions (and quantifiers generally) with *quantified* relative clauses, his semantics cannot. Consider, for example:

[5a] The woman who spanked each child is a philosopher.

A natural representation of this would be:

[5b] [the x: x is a woman & [Each y: y a child] (x spanked y)] (x a philosopher)

Let us state a preliminary sketch for [5b]'s truth conditions:

[5c] $|\{x: x \text{ is a woman \& [Each } y: y \text{ a child] (x spanked y)}\} - \{x: x \text{ a philosopher}\}| = 0 \text{ and } |\{x: x \text{ is a woman \& [Each } y: y \text{ a child] (x spanked y)}\}| = 1$

That is, the set X of child-spanking women minus the set Y of philosophers is empty, and the cardinality of X is 1.

Surely [5c] requires further analysis: syntactically complex quantifier restrictions (i.e., relative clauses), themselves containing quantifiers, must be treated as semantically complex. Otherwise the formalism will fail to accommodate certain intuitively valid inferences--inferences exploiting the meaning of the internal quantifier. In addition, it will fail to show how a speaker, knowing only the meanings of the constituents of '[Each y: y a child] (x

spanked y)]', can determine the property it expresses. But Neale's formalism has the resources to treat a syntactically complex restriction as semantically complex only where it is quantifier-free. In a sentence of the form '[The x: Fx & Gx] Hx' (with 'F' and 'G' atomic) the set assigned to the quantifier can be determined in a straightforward manner: the intersection of the sets assigned to the respective predicate letters 'F' and 'G'. In the case of [5] this maneuver reaches an impasse when we attempt to determine the extension of {x: [each y: y a child] (x spanked y)}. The extension of this set is not a function of the extensions of the parts of the abstracted predicate.²⁹ Thus, our treatment of [5b] is not fully compositional.

In general, quantifiers of the form:

$[Q_1x: Fx \ \& \ [Q_2y: Gy] \ (Rxy)]$

do not receive a fully compositional analysis in Neale's semantics. The relative clause (here, the constituent in bold-face) must be taken as primitive, as its extension is not determined by its structure and the extensions of its syntactic constituents.

One consequence of this feature of RQ is that sentences of the following form will not be logically true:

[6a] Each child who saw the lion saw a lion.

[6a] is symbolized as:

[6b] [each x: Cx & [the y: Ly] (Sxy)] ([some y: Ly] (Sxy))

which has the following truth definition:

[6c] $\{x: Cx \ \& \ [\text{the } y: Ly] \ (Sxy)\} \subset \{x: [\text{some } y: Ly] \ (Sxy)\}$

Clearly the set of things that saw *the lion* is a subset of the set of things that saw a *lion*. Consequently, the set of children that saw the lion is a subset of the set of things that saw a lion. But Neale's semantics cannot show the former. The relative clause 'x is such that x saw the lion' must be, as we have seen, semantically primitive.

In contrast, a Russellian analysis of these contexts is readily available. For example, [5a] becomes:

[5d] $\exists x(x \text{ a woman} \ \& \ \forall z(z \text{ a child} \supset x \text{ spanked } z) \ \& \ \forall y([y \text{ a woman} \ \& \ \forall z(z \text{ a child} \supset y \text{ spanked } z)] \supset x = y) \ \& \ x \text{ is a philosopher})$

What is most important, however, is that an adequate (Tarskian) truth definition for [5d] is also available. [5b], on the other hand, does not have a fully adequate truth definition.

4.2 A Tarskian Truth-Theory for Natural Language

Quantifiers? One might wonder whether one can define truth in terms of satisfaction for a language containing restricted quantification, and whether this would allow a fully compositional treatment of [5b]. Neale does not develop such an account, although he does mention it (at 59, note 49)). But in fact, an account, following Davies (1981), can be constructed which does allow for a fully compositional treatment.

To see how satisfaction works here, let us stipulate that a sequence s satisfies

$[\text{each } v_i: \Phi] \pi$

just in case, for each sequence s' that (i) satisfies ' Φ ', and (ii) differs from s at most in what it assigns to ' v_i ', also satisfies ' π '. (For convenience, let us call the sequence that meets condition (ii) the " i -variant" of s .³⁰)

This suggests the following axioms:

- [Q1] s satisfies ' $[\text{each } v_i: \Phi] \pi$ ' iff all i -variants s' of s that satisfy ' Φ ' also satisfy ' π '
- [Q2] s satisfies ' $[\text{an } v_i: \Phi] \pi$ ' iff some i -variants s' of s that satisfy ' Φ ' also satisfy ' π '
- [Q3] s satisfies ' $[\text{no } v_i: \Phi] \pi$ ' iff no i -variants s' of s that satisfy ' Φ ' also satisfy ' π '

If ' π ' is a truth-functional compound, satisfaction is defined in the standard manner:

s satisfies ' $\neg\Phi$ ' iff s does not satisfy ' Φ '.

s satisfies ' $\Phi \vee \pi$ ' iff s satisfies ' Φ ' or s satisfies ' π '.

s satisfies ' $\Phi \& \pi$ ' iff s satisfies ' Φ ' and s satisfies ' π '.

s satisfies ' $\Phi \supset \pi$ ' iff either s does not satisfy ' Φ ' or s satisfies ' π '.

Now for a complication. Take a case where the restriction ' Φ ' contains a quantifier. Following our stipulated definition, a sequence s satisfies

$[\text{each } x: Fx \& [\text{some } y: Gy] (Rxy)] \pi$

iff for each x-variant s' of s that satisfies ' $[\text{some } y: Gy] (Rxy)$ ' s' also satisfies ' π '. s' satisfies the former open sentence just in case some y-variant s'' of s' that satisfies ' Gy ' also satisfies ' Rxy '. We have, then, achieved a fully compositional analysis--something Neale's semantics did not provide.

But, if we return to [5b], things are once again problematic. For how does one provide satisfaction conditions for sentences of the form ' $[\text{the } v_i: \Phi] \pi$ '? Our semantics, modeled on standard model-theoretic semantics for first-order logic, has no axiom for such contexts. In fact, it only treats of quantifiers that have logical quantifiers as their constituent determiners ('each', 'some' and 'no'). But 'the' is not a logical quantifier. One possible response to the question is that we must first translate the sentence into a form containing only the logical quantifiers and truth-functional connectives (the sole non-logical vocabulary consisting in terms and predicates). Yet, this response must either give up the idea that descriptions are quantifiers, or trivialize that claim. Let me explain. If [1b] cannot be interpreted without our first translating it into [1a], then it seems that [1b] really does not capture the logical form of 'the F is G'. For to say that Σ is a representation of S 's logical form is to say that Σ exhibits S 's truth condition, or S 's inferential properties. But [1b] does neither.

Another possible response is that we assign satisfaction conditions to '[the v_i : Φ] π ' based on the following clause:

[Q4] s satisfies '[the v_i : Φ] π ' iff the unique i -variant s' of s that satisfies ' Φ ' also satisfies ' π '

In fact, the suggestion works. What is problematic, however, is that [Q4] is equivalent to the satisfaction condition for the Russellian expansion of 'the F is G '. 'the F is G ' is true just in case something is F , no more than one thing is F , and each F is G . On the one hand, this shows that we have things just right. On the other, the question arises as to what we have gained. If we define 'the unique Φ ' as it occurs in the meta-language in terms of 'all' and 'some', then the suggestion that descriptions be analyzed in terms of [Q4] reduces to something like the first suggestion: that we analyze a descriptive sentence by first translating it into the appropriate expansion (only in this case the translating goes on in the meta-language). We analyze the sentence's semantic interpretation by first translating it into a sentence containing only primitives of the meta-language. Either way it is hard to avoid the conclusion that at the final level of semantical analysis 'the F ' is simply not a quantifier. It receives the truth definition of a sentence containing no such expression--the Russellian expansion of 'the F is G '.

If, however, we take 'the (unique) Φ ' as primitive--not definable in terms of 'all' and 'some'--then we can no longer exhibit the validity of arguments such as:

The man who lost his tickets left.
Some man who lost his tickets left.

The winner received a prize and a new pair of skates.
Each winner received a prize and a new pair of skates.

Nor will we be able to exhibit the logical truth of sentences such as:

If the winner of the current match will receive a prize, then all winners of the current match will receive a prize.

This should be enough to force a Russellian to abandon the second strategy. For a Russellian (unlike a referentialist) would maintain that the above arguments are valid and that the above sentence is true in virtue of the meaning of the logical constants 'the' and 'some'. Yet, he would be unable to explain the validity of the former or the necessity of the latter if he took 'the' as it occurs in the right side of [Q4] as undefined.

Thus far I have attempted to show (1) that Neale's semantics for RQ is defective, and that consequently it provides no reason for taking [b], and hence [c], seriously. I then went on to suggest (2) that an adequate semantics for RQ could be developed, but that the claim that descriptions are quantifiers within the context of this semantics was either false, wholly insignificant, or had consequences that a Russellian could not countenance. I now want to make a

more general criticism of [b], and claim that RQ (together with our semantics) fails to exhibit any interesting semantical similarities among natural language quantifiers.

4.3 Interpreting Restricted Quantifiers.

The quantifier 'most' must be interpreted as restricted. It simply does not make sense to say that 'most Americans drink weak coffee' asserts, about *most things*, that (1) if they are Americans, then they drink weak coffee, or that (2) they are Americans *and* drink weak coffee. Rather, the sentence must be analyzed as asserting about *most Americans* that they drink weak coffee. More generally, sentences of the following form:

Such-and-such quantity of all F's are G's

cannot be assigned their correct truth conditions if analyzed as making an assertion about a given quantity of objects in a given universe.³¹

[Q1] - [Q3] provide us with satisfaction clauses for sentences with restricted (logical) quantifiers. Extended to non-logical quantifiers such as 'most F's' it yields:

[Q5] A sequence *s* satisfies '[most $v_i: \Phi$] π ' iff most *i*-variants *s'* of *s* that satisfy ' Φ ' also satisfy ' π '.

[Q6] A sequence *s* satisfies '[few $v_i: \Phi$] π ' iff few *i*-variants *s'* of *s* that satisfy ' Φ ' also satisfy ' π '.

It seems that a fully compositional semantics for Neale's system can be provided. But we should note that

[Q5] and [Q6] state their truth conditions with sentences that themselves contain restricted quantification. This presents a difficulty. For these contexts must themselves be analyzed. Moreover, the analysis of these contexts (containing the non-logical quantifiers 'most Fs' and 'few Fs') cannot proceed in terms of the semantic primitives of the meta-theory ('all' and 'some'). Nor can the new meta-theoretical primitives define the primitives of the initial theory. But then arguments of the following form will not be shown to be valid:

Most actors are waiters.
Most actors are rude.
Therefore, some waiters are rude.

Few senators fail to get re-elected.
Therefore, some senators fail to get re-elected.

Most dogs bark.
Therefore, some dogs bark.

If this is the case, then the new semantics is inadequate. It fails to show that, in each case, the conclusion is a logical consequence of the premises--that it is true at all models at which the premises are true.

Furthermore, if we restrict our attention to the fragment of Neale's system that contains only sentences with logical quantifiers as determiners (thus avoiding this problem) it will be hard to escape the conclusion that this fragment is simply a notational variant of first-order logic. That is, a language RQ with 'each', 'an' and 'no' as

comprising the set of determiners is expressively equivalent to first-order logic.

Nonetheless, one might argue as follows. 'Perhaps, as you have shown, Neale's semantics fails for his system of restricted quantification. Yet, the alternative Tarskian semantics for this system does exhibit similarities among various quantified sentences (although this holds only for the fragment whose sole determiners are the logical quantifiers). For example, a sequence s satisfies a singular description '[the x : Fx] Gx ' iff

[S] the unique x -variant s' of s that satisfies ' Fx ' also satisfies ' Gx '

Whereas a sequence s satisfies the plural description '[the_{pl} x : Fx] Gx ' iff

[P] the x -variants s' of s that satisfy ' Fx ' also satisfy ' Gx '

This exhibits a similarity between singular and plural descriptions in exactly the way [2a] and [2b] did. Thus, Neale's notation does indeed exhibit similarities among 'the' and other quantifiers, and, to this extent, vindicates his claim that descriptions are quantifiers.'

Yet, the problem with this argument is that the satisfaction clauses are not stated in their reduced form; they merely paraphrase this form. To arrive at the reduced form, the sentences must first be translated into their first-order counterparts. The resulting satisfaction

conditions for the respective sentence forms will exhibit no significant similarities. So the similar treatment of the two sentences is deceptive: it appears only at an intermediate level of analysis.

It might be responded that [S] and [P] should be treated as primitive. One rejoinder to this is that arguments of the following form will no longer be formally valid: .

The winner received a prize and a new pair of skates.
Each winner received a prize and a new pair of skates

The winners celebrated.
At least two people celebrated.

Now, it might be suggested that it is not the business of semantics either to provide a logic of English, or to provide a framework for such a logic. Neale is simply providing a meaning theory, and this cannot be evaluated on the basis of its accommodating certain patterns of inference. And this is no doubt a reasonable response. But then it is unclear what interest attaches to the claim that descriptions are quantifiers. For, we seem to have no way of evaluating that claim.

I conclude that whatever generality Neale's notation affords him, it is due solely to features it inherits from standard first-order logic. Thus, as I have said, nothing is to be gained by opting for his analysis. Yet he writes:

The formalism of *Principia Mathematica* is dispensable. The Theory of descriptions has a natural place within a general theory of natural language quantification ... (47).

We have seen that each claim is misleading: While Neale's notation has the virtue of syntactic perspicuity, his proposed semantics is inadequate. Yet, as I have tried to show, even a general semantical theory for RQ simply recapitulates the standard Tarskian semantics for *Principia Mathematica*. Thus, Neale has not established that there is any significant sense in which Russell's formalism is "dispensable". But, if so, then we really have no context in which to judge the claim that descriptions are quantifiers. The theory of descriptions can be interpreted in RQ. But, as I have suggested, if the descriptive sentences of RQ are simply abbreviations of sentences of first-order logic, then this claim is void of any real significance.

I conclude, then, that the claim that descriptions are quantifiers has not been established.

5. How Secure is the Traditional Russellian Account?

Another important argument for the correctness of the quantificational account is the argument for the general adequacy of Russell's version of the theory of descriptions.³² Neale's central argument for the general Russellian strategy (as opposed to the specific claim about its interpretation) seems to be as follows. If we allow ourselves the claim that the traditional analysis and the quantifier analysis are equivalent, then descriptive

incompleteness is not evidence for descriptive referentiality any more than the incompleteness of 'everyone' as it occurs in an utterance of 'everyone got sick' is evidence for *its* referentiality. Quantified sentences are often incomplete, yet we do not take their incompleteness as somehow forcing a referential interpretation.

Now as an argument against Wettstein this is successful--as I suggested above--only *assuming the equivalence*. Let us formulate the equivalence assumption as follows:

- [A] The quantificational treatment of Russell's theory of descriptions is essentially correct: the restricted quantifier form, and the Russellian expansion, respectively, of 'the F is G' are interchangeable.³³

To block Wettstein's inference from descriptive incompleteness to descriptive referentiality Neale exploits [A]. He points out that *quantificational* incompleteness is compatible with *quantificational* irreferentiality. That is, a context of the form '[Q + F] is G' cannot be interpreted as being semantically referential (or object-dependent) simply in point of its incompleteness. But according to [A], 'the F is G' exemplifies precisely this form. Consequently, its incompleteness provides no evidence for its referentiality.

That Neale assumes [A] is clear. Commenting on Wettstein's version of the argument from incompleteness

(which I have reconstructed above, Chapter 2, I) Neale writes: "The first sign that something is amiss here is the fact that the argument seems to go through just as well with other quantifiers" (Neale, 1990: 98).³⁴

On the other hand, one might argue that Neale intends to support [A] indirectly by showing how it vindicates Russell's theory. Yet, for this converse argument to count as supporting the equivalence it would have to be assumed that the general Russell strategy is correct. The claim of equivalence, that is, would be vindicated by its entailing something we already accepted as true. Given that the latter claim (the correctness of the Russell view) is one of the points at issue, the strategy is a non-starter.

Either way, Neale is hardly in a comfortable dialectical position: for each strategy has him making an assumption as significant as the conclusion upon which it is based. One strategy assumes that descriptions are semantically quantificational (assumption [A]), the other, that they are not semantically referential (Russell's theory). In fairness to Neale it should be conceded that I may not be capturing Neale's intention in thus reconstructing the argument. But if I am wrong then I fail to see how the considerations he puts forward can form a response to the referentialist.

6. Modified Ockham's Razor and the Referential Interpretation.

What Neale takes to be crucial in defending the general Russellian analysis (i.e. independent of notational considerations, which, as we have seen, are far from compelling) is the principle Grice labeled Modified Ockham's Razor (MOR). As we recall (Chapter 2), Neale does not deny that descriptions can be used referentially. He simply denies that descriptions are semantically referential singular terms. In other words, a speaker can mean an object-dependent proposition of the form *a* is *G* in uttering 'the *F* is *G*', but what he says is captured by the corresponding Russellian expansion. (As stipulated above, δ occurs referentially in an utterance *u* of ' δ is *G*' iff the proposition expressed by *u* is object-dependent.) But, significantly, appeals to MOR, and to the distinction between *means* and *says*, are not to the point here. Recall that MOR enjoins the semantic theorist to avoid multiplying senses of an expression *e*--in the face of a pattern of uses which do not conform to *e*'s assigned meaning--when that usage can be explained pragmatically. But rather than violating MOR, the uniformly referential interpretation is in explicit conformity with it: it assigns a single, referential sense to definite descriptions. Without a doubt some descriptive utterances convey attributive propositions. But the attributive proposition conveyed can be taken to be an implicature of the proposition uttered, and not part of

its literal meaning. (This uses the same strategy as the Russellian, but with an opposing goal: to vindicate the uniformly referential analysis, and not the Russellian one.) The MOR appeal, therefore, does not decide between the two available non-ambiguity hypotheses.

Kripke has suggested a heuristic which is relevant to evaluating counterexamples to semantic proposals: imagine a language just like the language under consideration, except that the proposal in question is stipulated to be true for it. If we are not forced to assume that the resulting language must be used differently than the original language, then the semantic hypothesis is immune to use-invoking (or intuition-invoking) counterexamples.

Let us follow this suggestion. Imagine a language--call it "R-English"--in which all expressions of the form 'the F' are, in their non-predicative occurrences, referential. In all other respects R-English is just like English (or as close to English as possible given the hypothesis). Would we speak any differently--in particular, would our use of definite descriptions be any different--if we spoke R-English as opposed to English? As I have suggested, 'the F is G' can be used to convey the object-independent proposition that there is one F, and that all F's are G's--even assuming 'the F' is semantically referential. To say we speak R-English is simply to assume that 'the F' is in fact semantically referential.

Consequently, if my former claim is true, then the assumption that we speak R-English is fully compatible with the fact that descriptive sentences can be used to convey object-independent propositions. The answer, then, is that we would not speak any differently if we spoke R-English. Consequently, appeals to how descriptions are used cannot be used to decide the semantical question.

One might respond to this argument with the claim that, while the supposition that we speak R-English is compatible with our linguistic practices, it is incompatible with our semantical intuitions. For example, an R-English speaker would judge a contemporary utterance of 'the present king of France is a socialist' to be meaningless, whereas a speaker of English might judge it to assert an object-independent proposition. But the response makes an assumption that is far from obvious, namely, that the supposition that we speak R-English implies that our intuitions reflect the semantical rules of R-English. But, if this were true, Kripke's test would be pointless. For, in evaluating a semantical hypothesis, we could bypass such thought experiments and appeal directly to intuition. Moreover, it seems perfectly possible for speakers to have imperfect semantical knowledge. After all, we do assume that there is a fact of the matter as to the semantics of definite descriptions even in light of the fact that English speakers have no reliable intuitions about this fact. But, if so, we should not

expect speakers of R-English to have authoritative intuitions about descriptions simply given the assumption that there is such a fact with respect to R-English.

That some uses of descriptions are Russellian, or object-independent (or "attributive") was never challenged by ambiguity theorists such as Donnellan and Wettstein. Given this assumption, together with MOR, Neale's claim against the ambiguity thesis that descriptions are uniformly quantificational (hence uniformly Russellian) has a *prima facie* motivation. But if, as I am suggesting, we deny the assumption, then the claim is unmotivated. In sum: MOR, plus pre-theoretical facts about descriptions, delivers nothing particularly congenial to the Russellian.

7. Descriptive Pronouns and the Referential Thesis.

I will now compare the quantificational and referential theories on their treatment of pronominal descriptions. I will argue that the assimilation of definite descriptions to the class of demonstratives is preferable to their quantificational interpretation in that the assimilation does not leave it a mystery as to why descriptions can often function as pronouns anaphoric on an antecedent noun phrase. That is, it explains the existence of pronominal descriptions. 'he', 'she', 'they' and 'it' have a

pronominal role in addition to a demonstrative role. And it is well-known that descriptions also function as anaphoric pronouns--that there is a class of pronominal descriptions.

There are two classes of anaphoric pronouns: those bound by their antecedents, and those not bound by their antecedents. As is familiar to linguists, a pronoun P is bound by an antecedent A just in case P is preceded and c-commanded by A.³⁵ Pronouns not bound by their antecedents are called, following Evans, E-type pronouns. The classic example of an E-type occurrence of a pronoun is the following sentence.

[1] John owns some sheep and Harry vaccinates them.

Clearly, on the preferred reading of this sentence, 'them' is anaphoric on the quantifier expression 'some sheep'.

Yet, analyzing the pronoun as bound by 'some sheep' provides the incorrect truth conditions:

[1'] [some x: Sheep(x) & Belongs-to-John(x)] Harry-vaccinates(x)

Giving the quantifier wide scope provides a reading of [1] which says that the class of John-owned Harry-vaccinated sheep is non-empty. But notice that [1'] could be true in a situation in which Harry failed to vaccinate *all* John's sheep. Such a situation would make [1] false.

Definite descriptions occur as both bound and unbound--i.e. E-type--anaphors. For example, 'the man' in [1b] plays the same role as the bound pronoun 'he' plays in [1b]; this

is clear given the intuitive content-identity of [1a] and [1b] (relative to certain contexts):

[1a] If a man with a deep voice calls, he will ask for John.

[1b] If a man with a deep voice calls, the man will ask for John.

These both receive the following first-order representation:

$$\forall x(M(x) \ \& \ D(x)) \supset A(x)$$

In addition, 'the man' can occur as an E-type pronoun:

A man with a deep voice left a message on my answering machine, but he didn't leave his number.

A man with a deep voice left a message on my answering machine, but the man didn't leave his number.³⁶

If we assimilate definite descriptions to the class of demonstratives, it will hardly be surprising that they also play a dual role, moonlighting as anaphoric pronouns. If, on the other hand, descriptions are analyzed along Russellian lines, then their role as anaphoric pronouns is inexplicable--a sheer linguistic accident. This holds whether we opt for Russell's own analysis, according to which descriptions--at least at an intermediate level of analysis--are variable-binding term-operators, or Neale's quantificational analysis. But it is undoubtedly more odd on Neale's view, as descriptions would be the only quantifiers possessing a pronominal function. For example, 'a man', as it occurs in an utterance of the following sentences, simply cannot function anaphorically:

A man with a deep voice called, but a man didn't leave a message.

A man with a deep voice called, but a man with a deep voice didn't leave a message.

Nor can 'each man' and 'some man':

Each man with a deep voice called, but each man didn't leave a message.

Each man with a deep voice called, but some man didn't leave a message.

Nor, for that matter, can any other quantifier, no matter what the antecedent. Moreover, a quantifier cannot be bound by an antecedent quantifier.

It might be argued that this proves nothing. 'Perhaps definite descriptions are the only quantifiers that have a semantically distinct anaphoric role. What needs to be shown is that the anaphoric function of 'the F' admits of no explanation, that this function *must* be a linguistic accident. But it is not at all clear that there isn't a deep semantic relation between expressions possessing the quantificational sense Neale ascribes to 'the F' and anaphors--bound and unbound.' I will now show that this response is mistaken: there is no such deep semantic relation.

First, some methodological remarks are in order. In the following passage, Kripke proposes an intuitive diagnostic for ambiguity:

"Bank" is ambiguous; we would expect the ambiguity to be disambiguated by separate and unrelated words in other languages. Why should the two separate senses be reproduced in languages unrelated to English? First, then, we can consult our intuitions... Would we be surprised to find languages that used

two separate words for the two alleged senses of a given word? If so, then, to that extent our linguistic intuitions are really intuitions of a unitary concept, rather than that of a word that conveys two distinct and unrelated senses (268).

I will follow Kripke's suggestion in showing that the quantificational and anaphoric readings of 'the F' do not comprise a unitary concept. Showing this is easy. I simply find an expression that possesses the quantificational sense Neale ascribes to definite descriptions but which has no pronominal role. If there is such an expression, it will show that no deep semantical relation holds between the two functions

Consider the quantified NP 'exactly one F'. This has precisely the same sense Neale claims 'the F' has; they are alternative expressions of the same restricted quantifier.³⁷ Yet, significantly, 'exactly one F' has no anaphoric role.

A man with a deep voice called, but exactly one man didn't leave a message.

A man with a deep voice called, but exactly one man with a deep voice didn't leave a message.³⁸

This seems to provide all the evidence we need that the alleged quantificational sense of 'the F' is independent of its anaphoric sense. For, there exists in English an expression with precisely the quantificational sense Neale ascribes to 'the F', but which has no anaphoric function whatever. Thus, it seems unavoidable that if we assume the quantificational interpretation of definite descriptions,

the anaphoric function of 'the F' is a sheer linguistic accident.

Note that Neale could respond that 'the man' as it occurs in:

[2] A man arrived this morning, but the man left before returned.

is a quantifier. For, in Chapter 5 of his (1990), he develops a rule that would provide completions for (E-type) pronominal descriptions. The rule states that an occurrence of an E-type pronoun is to be interpreted as the most "impoverished" definite description "directly recoverable from the antecedent clause..." (182). In the case of [2] the rule yields 'the man who arrived last night' as the interpretation of 'the man'.

This provides the basis of a response to my criticism. For, it can be argued that Neale's rule shows how anaphoric occurrences of 'the F' are actually quantifiers. Thus, the quantificational and anaphoric senses of 'the F' do form a unitary concept. It is no longer inexplicable how these two senses converge on a single expression.

The first response to make to this rejoinder is that the bound variable use of descriptions is still left unexplained. Surely bound variables cannot be understood as quantifiers. Thus, we still have a mystery: why the bound variable and quantificational senses of 'the F' converge on a single expression.

Secondly, the argument proves too much. It interprets all E-type pronouns quantificationally. But, since 'the F' is like an E-type pronoun in its E-type occurrences--it has a quantificational sense--there is no special reason to assume it is unlike non-E-type occurrences of pronouns in its non-E-type occurrences. That is, there is no special reason to suppose it does not function demonstratively in these occurrences. Thus, at best, his claim fails to motivate a quantificational--as opposed to demonstrative---reading of non E-type occurrences of definite descriptions; at worst, it contra-indicates such a reading, since it suggests an assimilation of 'the F' to pronouns generally. And pronouns do not have a quantificational sense outside of their E-type occurrences.

Finally, Neale's rule does not provide completions for descriptions--and E-type pronouns generally--anaphoric on proper names:

Cuomo arrived this morning, but the governor left before I returned.

Thus, we have here a prima facie objection to the quantificational analysis and prima facie support for the demonstrative analysis: (1) Descriptions perform a function that no other restricted quantifiers perform. (2) In particular the very quantifier expression ('exactly one F') that is held to be definitionally equivalent to the definite description has, unlike the definite description, no anaphoric role.

These are points that a Gricean ought to address. If we are to take MOR seriously, we should resist ascribing a dual sense--or two distinct rules of use (corresponding to quantified noun phrase and pronoun)--to an expression if we can explain its behavior equally well without assuming ambiguity.³⁹ Now, as is well-known, some uses of 'the F' are not at all descriptive, for example:

The whale is a mammal.

The indefinite article has a similar, universal, use:

A whale is a mammal.

These uses of the definite and indefinite article, respectively, cannot be explained by appeal to their standard senses--*whether one takes those to be referential or attributive*. But in such cases multiplication of sense becomes necessary. The point is that we should resist the move where it is not necessitated by the relevant data. And it is simply not the case that the data necessitates the claim that definite descriptions and demonstratives belong to distinct categories.

Conclusion.

I began by arguing for the descriptive adequacy of the referential thesis and suggesting ways in which it is preferable to the Russellian alternative. In the last few sections my focus has primarily been on pointing out some difficulties for the latter account--in particular,

difficulties attaching to the claim that descriptions are quantifiers. The assumption is crucial to the Russellian position. For, without the assumption that descriptions are natural language quantifiers, she has no defense against the referentialist.

My results about descriptions are largely deflationary. Thus, any interest that attaches to my thesis is ultimately negative. If correct, it shows, in effect, what definite descriptions are not. They are not quantifiers; they do not receive "contextual definitions" which distort the grammatical form of the contexts in which they occur; they do not combine with predicates to express object-independent propositions. Russell's theory, in this context, can indeed be seen as a paradigm of philosophy--the postulation of an elaborate "deep structure" lurking behind all instances of a given phenomenon, focused solely on one aspect of that phenomenon, and motivated largely by the discovery of a technical innovation to express that aspect. The aspect in question is the implication of uniqueness that definite descriptions possess, the technical innovation is the ability to formally represent uniqueness. Yet, the theory explains only one aspect of descriptions, an aspect that is equally well explained by the assumption of their referentiality. Moreover, it is limited to one pattern of use of descriptions. Finally, it fails to comport with other aspects of descriptions: their *prima facie* similarity

to pronouns, their role as anaphoric pronouns and the incompleteness they commonly exhibit. Each of these is either problematic or wholly inexplicable on the Russellian account.

If we have learned anything from Grice and Kripke, it is that distinguishing senses of definite descriptions is to be avoided. But we have seen that such methodological scruples fail to provide the correct theory of descriptions; they simply constrain what form such a theory must take. As Kripke writes, such considerations

show in the present case that the argument Donnellan actually presents in his original paper shows nothing against a Russellian or other unitary account, and they make it highly probable ... that the problems Donnellan handles by semantic ambiguity should instead be treated by a general theory of speech acts. *But at this time nothing more definitive can be said* (Kripke, 1979: 271, emphasis added).

Kripke does not specify what such an other unitary account might be, but presumably it has to be an account that takes the other candidate "sense" of definite descriptions to be their literal sense. In this chapter, I have tried to defend this alternative unitary account. If I have not succeeded in providing decisive considerations in favor of such an account, I have at least shown how the Russellian's key defense against the argument from incompleteness must fail. This hardly provides a decisive victory over the Russellian; but it certainly moves the debate concerning the proper theory of descriptions beyond the recent stalemate.

Notes To Chapter Three.

1. Schiffer (1981) is perhaps the only previous defense of such a view in the literature, although the view of Strawson (1950) is quite similar. There are divergences between the current view and Strawson's, however. (1) It is unclear whether Strawson holds a view according to which utterances containing vacuous descriptions fail to express a proposition, or whether they simply express propositions that, at the context, are devoid of truth-value. Consequently, it is unclear whether Strawson construes referentiality in terms of object-dependency. (2) While Strawson can be read as proposing a view according to which the linguistic rule associated with a description is to be kept distinct from its referential content, he does not say whether it is the referent alone which is the description's propositional contribution. On the current view, it is the referent alone which is the descriptions propositional contribution. (3) Strawson does not specify whether non-referential uses of descriptions are to be given semantic significance. The current view explicitly denies semantic significance to this pattern of use.

2. See Grice (1969), (1981); Kripke (1977).

3. There is some controversy as to whether or not Frege held that thoughts or propositions expressed by sentence-tokens containing singular terms are indeed object-independent. But those arguing for the alternative reading (e.g., Evans, 1982) seem clearer on what Frege meant than Frege himself seems to have been.

4. The meaning rule device is derived from Schiffer (1994).

5. Note that nothing I say assumes that *de re* reports are either referential or that they involve quantifiers taking wide scope.

6. Due to Schiffer (1981: 70-71).

7. It does not follow, however, that *de re* belief is irreducible to *de dicto* belief. Indeed, given a suitably sophisticated version of the *de dicto* (i.e., as allowing *de dicto* attitudes to object-dependent propositions), the reduction would seem to hold for belief.

8. I assume here, following Grice (1981), that it is a constitutive feature of implicatures that they be justified. Consequently, if the Russellian is to claim that a completion is implicated by a descriptive utterance, she must show how it can be justified given mutually available facts.

9. My use of the terminology should not be taken as an endorsement of the referential-attributive distinction, which I reject. It simply allows me an easy way of labelling the sort of case I have in mind.

10. It should also be noted that an indeterminist response to the problem of attributive incompleteness is unavailable for the Russellian. For the indeterminist maintains that the announcer *indeterminately* meant each of the two completions in uttering [2]. But, on the assumption, for example, that the announcer *indeterminately* meant (iv), it is unclear that he could have (*indeterminately*) meant (iii).

11. I think that a defense of R1's compatibility with the relevant data is available even granted the questionable assumption that reference must provide individuating knowledge. In the case of [3], for example, the audience can take the speaker to be referring to the unique person bearing some complex, as yet unspecified relation to him. Perhaps: *the person who caused things to be such that the announcer felt compelled to utter what he did*. It does not seem at all unreasonable to suppose that such identifying information will always be available. However, the Russellian cannot respond that if such information is available, then a unique completion is available is well and that, consequently, R2 is ultimately compatible with such contexts after all. The reason is that it is simply false to hold that the speaker had any such completion in mind when he uttered what he did.

12. See Chapter One, 1.1 and 1.2.

13. See Kripke (1977: 258-59); Neale (1990: 122-23).

14. Nor, as I have intimated, has the claim that 'that F is G' must be object-dependent; but I leave that concern aside.

15. On my view descriptions are directly referential. This suggests the following counterexample (due to Russell Dale). 'the first postmaster general' directly refers, on my view, to Benjamin Franklin. But in that case, the following sentence expresses a necessary truth:

[i] Benjamin Franklin = the first postmaster general.

For it expresses the proposition: <<BF, BF>, Identity>. Yet, [i] appears contingent--it appears to ascribe a property to Franklin that he might not have possessed. Yet, I am not denying that [i] typically communicates a contingently true proposition. I am simply suggesting that the property of being false at some possible worlds attaches not to what [i] expresses, but rather to what it can be used to mean. For the

Russellian's argument to be effective, he must show that the appeal to implicature is inappropriate here.

16. The following discussion relies on Wilson (1978).

17. This point is due to Robert Fiengo.

18. This, of course, is equivalent to the preferred Russellian rendering:

$$\exists x(F(x) \ \& \ \forall y(F(y) \supset y = x) \ \& \ x = \text{John})$$

(Which is simply the expansion of ' $(\iota x)F(x) = \text{John}$ '.)
But if, as seems correct, predicative occurrences of 'the' are not descriptive, then an account that distinguishes between the two kinds of occurrences (i.e., the referential theory) is preferable to one, like Russell's, that does not.

19. Here I am indebted to Stephen Schiffer.

20. It might be objected that interpreting sentences of the form 'the F might not have been F' as ' $\text{Poss}(\neg(\text{The } F = \text{The } F))$ ' leaves out another reading, intermediate between a narrow scope and referential interpretation. For example, 'the president might not have been Clinton,' has a wide reading which is inequivalent to a referential reading. This is perhaps easier to see in cases like

The first child born in the twentieth century might not have been the first child born in the twentieth century.

This, it is maintained, has a non-de dicto, non-referential reading. But, even if the intuition is reliable, it is unclear why we should assume that this is anything more than an intuition concerning an implicature of such an utterance.

21. Almost every theory of singular terms faces this problem. For example, even those theorists who deny that singular terms are directly referential (for example, the neo-Fregeans mentioned in Chapter One) almost invariably take sentences of the form 'N is G' (where N is a name) to be object-dependent.

22. The last sentence is taken from a series of exercises in Kalish and Montague (1964: 242), in a chapter entitled 'The'. The authors intend the sentence to be translated into a form containing the iota-operator.

23. See section 1.6 ('Proper Names and Other Noun Phrases are Natural Language Quantifiers').

24. The same question arises with Neale's treatment of expressions of the form (NP + *poss*) + N' (see Neale, 1990: 47). For example, expressions like: 'the murderer of Smith', 'Smith's wife', etc. These have traditionally been characterized as descriptions. The problem is that it is not at all obvious that expressions like the latter are inevitably descriptions, let alone quantifiers. For example, an expression like 'yesterday' could be analyzed descriptively: [The x: x is a day that immediately precedes α] (where ' α ' is a demonstrative that refers to the current day). But this descriptive material might simply be part of the rules for use of 'yesterday' -- its character, following Kaplan -- rather than its propositional content at a given context. Indeed, this is the intuitive analysis of contexts containing 'yesterday'. Why should it not extend to contexts containing 'Smith's wife' or 'my mother'? I am not necessarily recommending this alternative. I am simply suggesting that the quantificational analysis of such contexts can hardly be adduced as an advantage of a given notation if it is unclear that the semantical classification of the relevant expressions is descriptive.

25. One consideration which strikes me as quite significant is suggested by Reimer (1992: 360-61). Quantifiers can naturally occur as responses to questions such as 'How many Fs?' Yet 'the F' seems a distinctly unnatural response.

26. It should be noted that Russell never explicitly takes descriptions to be quantifiers. (Which is not to say that his theory does not suggest a quantificational treatment, nor that it has not been interpreted quantificationally.) Recall that Russell maintained that descriptions can only be provided contextual definitions. That is to say, nothing in the analysis of contexts containing the definite article will contain an element corresponding to it. For Russell to maintain, in light of this, that descriptions are quantifiers would be unlikely: logical quantifiers, after all, do not simply disappear under analysis. (Although quantified noun phrases do.)

Russell did use a notation that may suggest that he treated descriptions as quantifiers, but the notation is misleading. Recall that he used [i] and [ii] to indicate the scope of the description relative to that of the negation sign. In [i] the description takes wide scope, in [ii], narrow scope.

[i] $[(\iota x) Fx] \neg G[(\iota x) Fx]$

[ii] $\neg[(\iota x) Fx] G[(\iota x) Fx]$

But these are simply conventions for scope indication. The fact that the description precedes the formula does not

indicate that it binds anything within the formula. It would equally suffice to prefix the iota-operator with numerical subscripts. For details on this approach, see Grice (1969: 139).

27. My argument is not intended to challenge the use of Neale's semantics for purely syntactic purposes. It has been suggested that a sentence of the following form will be meaningless just in case the quantifier *Q* is not monotone-increasing:

John has *Q* and *e* is/are *F*.

(Where *e* is a pronoun that agrees in number and gender with *Q*.) For example:

John has no sheep and George vaccinates them.

See Neale (1990: 171). May (1985: 9 and *passim*) adduces further purely syntactic reasons for providing a semantics of the sort Neale develops. Following Ladusaw (1981), he suggests that only quantifiers that are not monotone-increasing can occur grammatically in the following context:

Q who ever read anything about phrenology attended Gaul's lecture.

For example, [i] is intelligible, but [ii] is not:

[i] Every student who ever read anything about phrenology attended Gaul's lecture.

[ii] Some student who ever read anything about phrenology attended Gaul's lecture.

Yet, the fact that such interpretations are required for syntactic purposes does not imply that these interpretations can play the role of sentence meanings. To play this role, interpretations must be compositionally determinable. And I will show that Neale's interpretations are not, in fact, compositional. (Note that failure of compositionality does not affect the syntactic utility of Neale's semantics.)

(A quantifier '[det *x*: *Fx*]' is monotone-increasing iff the following is a valid argument form:

[det *x*: *Fx*] *Gx*
[all *x*: *Gx*] *Hx*
[det *x*: *Fx*] *Hx*

See Neale (1990: 206, note 7) for this formulation and Barwise and Cooper (1981: 184-87) for detailed discussion.)

28. There is an argument that suggests that 'that F', 'those F's', etc. are not determiners. For determiners cannot be iterated: 'Most each girl', for example, is not an acceptable NP. Yet, demonstrative pronouns can in some cases be attached to determiners: 'Those few girls that like Ed treat him badly.' Yet, this classification groups 'the' with demonstrative pronouns and not determiners, for 'the' can attach to a determiner--for example 'The few men that left early got lost'; 'The many workers affected by the decision remain hopeful.'

29. It is significant that Barwise and Cooper (1981) provide a semantics for a language that does not contain quantified relative clauses.

30. This is related to, but different from, Benson Mates' notion of a β -variant.

31. On this point see Sher (1991), especially Chapter 2, Section 3, and references therein.

32. By Russell's version of the theory, I mean the theory that takes 'the F is G' to possess the logical form Russell ascribes to it. The correctness of this theory does not necessarily hinge on the correctness of the quantifier analysis. Kripke (1979), for example, defends Russell's theory independently of any appeal to its quantificational interpretation.

33. To say that the two forms are equivalent or interchangeable I am not making a trivial notational claim, but rather the claim that Russell's analysis is equivalent to an analysis of descriptions as natural language quantifiers--that in some significant sense descriptions are such quantifiers.

34. He also writes that "The phenomenon [of context-sensitivity] is one that is not specific to descriptions but something we find with quantifiers quite generally" (Ibid, 102), which also suggests the assumption.

35. "A phrase α c-commands a phrase β if and only if the first branching node dominating α also dominates β (and neither α nor β dominates the other)" (Neale, 1990: 173).

The relation of c-commanding is a generalization of the notion of quantifier scope in first-order logic. In fact, relative to the syntax of some standard formalization of first-order logic, a quantifier Q c-commands a variable v iff v is within the scope of Q . See further, (Neale, 1990: 206, note 10).

36. Wilson (1991) shows that descriptive pronouns do not have to be derived from their antecedents. In fact, this is necessarily the case when a descriptive pronoun has a proper name--and not a quantifier--as antecedent:

[i] If Clinton calls, the president won't leave a message.

Obviously, in some contexts, the descriptive pronoun in [i] is attributive (any context prior to 1993). But in other contexts it is bound by its antecedent. This is evidenced by the fact that in those contexts, [i] and [ii] are content-identical:

[ii] If Clinton calls, he won't leave a message.

In such a context, both will ascribe the property $\lambda x(x \text{ calls } \supset x \text{ won't leave a message})$ to Clinton.

37. Note that 'exactly one' also functions as a logical (i.e., unrestricted) quantifier. '(exactly one x) ϕ ' receives the following truth definition:

'(exactly one x) ϕ ' is true just in case
exactly one object satisfies ' ϕ '.

Contexts in which it functions as a restricted quantifier receive the following truth definition:

'[exactly one x: Fx] Gx' is true just in case
exactly one object satisfies 'F', and all F's are G's.

Similar remarks apply to 'just one'.

38. One might argue that 'exactly one F' functions as an anaphor in some contexts. For example:

[i] A number of suspects were apprehended. Exactly one suspect was kept for further questioning.

Now surely, the response continues, it will be granted that assigning an interpretation to exactly one suspect depends, on the suggested reading, on how we interpret a number of suspects. Nonetheless, it can be said in response that there is no reading of [i] according to which a pronoun can replace exactly one suspect. That is, no matter what pronoun is substituted for PRO, [ii] will not be content-identical to [i].

[ii] A number of suspects were apprehended. PRO was kept for questioning.

But if [i] is not equivalent to a sentence in which exactly one suspect is replaced by an anaphoric pronoun, then the latter expression cannot be said to be anaphoric on the antecedent quantifier expression.

What is occurring in [i] is that the restriction on exactly one (i.e., 'suspect') is elliptical for a restriction explicitly containing an anaphoric pronoun:

[iii] A number of suspects were apprehended. Exactly one of them/of the suspects was kept for questioning.

Both sentences in [iii] possess the following logical form:

[iv] [a number of x: x is a suspect] (x was apprehended) & [exactly one x: x is a suspect & x was apprehended] (x was kept for questioning)

[iv] reveals that exactly one as it occurs in [iii] is a determiner, and that it attaches to the suggested restrictions to form a quantifier expression. Thus, [i] fails to provide a counterexample to my general claim that natural language quantifiers do not have a semantically distinct pronominal function.

39. Actually, my appeal to MOR assumes that the dual roles of pronouns--as anaphors and as demonstratives--are somehow connected. Such expressions provide not only language-world links, but also inter- (and intra-) sentential links. Perhaps these roles are connected due to their common feature of being noun phrases with no context-independent meaning. As such they are perfect tools for both place-holding and ostending. One might argue that this hypothesis fails, since other context-dependent noun phrases do not function as anaphors--namely, indexicals. But this is just wrong; indexicals do have an anaphoric function:

John wants to visit the east coast, but he won't like it here.

John wants to see me at three o'clock, but I have no time now.

Sure John wants to see the president of the United States. But I don't want to see John.

Each of these sentences has a reading (which is particularly strong in the first two) in which the indexical is bound by the antecedent.

If we assume that both roles are linked, then the appeal to MOR works as follows. Analyzing 'the F' as a quantifier would

require positing an ambiguity between this quantificational sense and the anaphoric sense. (To argue that the anaphoric use can be explained pragmatically is belied by the fact that no other quantifier has such a use.) But analyzing 'the F' as a demonstrative does not require positing a genuine ambiguity between a demonstrative sense and an anaphoric sense, since the senses are linked. Thus, all things being equal, MOR recommends the latter strategy.

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