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THE NON-UNIQUENESS OF SEMANTIC SOLUTIONS: POLYSEMY

I

'Radical pragmatics' appears to be an idea the time of whose name has come, but at this stage it is not clear what people mean the name to denote. For some, it is primarily a methodological predisposition: when in doubt about the explanation for a regularity in the use of an expression, treat it as due to pragmatic rather than semantic considerations, unless there is strong evidence to the contrary. For others, it involves a set of empirical assumptions to the effect that the semantics/pragmatics distinction cannot be validated in actual practice, and that linguistic description is better served if we do not attempt always to cut the cake so nicely, but content ourselves to record the relevant observations about language use without worrying too much about how to classify them. (This is roughly the view of workers in artificial intelligence, as well as of linguists and psychologists who prefer to work in an experimental framework.) Needless to say, neither of these positions is subject to empirical confirmation; they are determined rather by interests and sensibilities.

I will argue a third version of radical pragmatics here, where it is something more like a doctrine: the semantics/pragmatics distinction cannot be validated even in principle; there is no way to determine which regularities in use are conventional, and which are not. This is not to say that there are no purely linguistic conventions of use, but rather that the content of these (even construed transparently) is necessarily indeterminate. In order to make my case as strongly as possible, I will allow as severe an idealization as linguists are ever accustomed to make, though I will have more to say about the question of idealization at the end of this paper.

There is already a doctrine of the 'indeterminacy of meaning,' of course, and Quine has argued it so powerfully and persuasively that it has held philosophy at bay for twenty years or so.[1] And while its direct influence on linguistic theorizing about semantics has been slight, it is so familiar that I must stop here to make it clear why the two doctrines are quite distinct, for all that there may be points of comparison between them. (Ideally, I would have liked to avoid the term 'indeterminacy' altogether but it *is* convenient, and the available alternatives –

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'undecidability,' 'inscrutability,' 'indefinability' – are no less charged.)

What Quine is concerned about – along with many linguists of a more-or-less philosophical bent – is the question of what sorts of things 'meanings' are, about what it is that mediates between words and their extensions. But these 'meanings,' whether as intensions, senses, concepts, prototypes, markers, or what have you, are not what is at stake when linguists talk about the semantics of language, where 'meaning' is taken simply as a part of *langue*, or of competence, or of what is 'in' the language as an independent object of study. We may deny that *any* words have 'meanings' in the philosopher's sense; at the very least, there is a good case to be made against assigning intensions to such words as proper names and indexicals. But it makes no sense at all to say that the English words *Paris* and *that* do not have meanings in the linguistic sense, because it is obvious that it is only in virtue of the arbitrary rules of English that we use these words as we do, and not the (for us, meaningless) forms *Parigi* or *quello*. To say that linguistic meaning is determinate, then (and so, that the semantics/pragmatics distinction is well-taken) is just to say that we can determine what the rules are, granting that we can equivalently describe them as conventions or speakers' representations of conventions, and – because Quine's scepticism does have to be borne in mind – that we may have to allow that the 'same' convention may be representable in a number of empirically indistinguishable ways.

The reason that this determinacy is not foregone is that expressions are used in many more ways than simple convention could dictate. A particular sentence-type may be used to express any number of propositions (or 'accomplish any number of ends,' if you like), by Gricean implicature. And a given term may be used to refer to any number of things, by the processes of metaphor and metonymy. This is of course the absolute and insuperable difference between the use of natural and constructed languages, for no set of natural conventions can ever be sufficient to ordain behavior in all possible situations, so that speakers must always be generalizing from old precedents to new solutions[2]. And for this reason, the determination of the set of purely linguistic conventions governing the use of expressions is an empirical undertaking, since we require some means of distinguishing them from uses that are pragmatically derived.

II

I will talk here exclusively about word-uses, though all that I will say could apply to sentence-uses as well. How shall we distinguish

metaphors and metonymies – let us say, 'extended word-uses' – from uses that are licensed entirely by linguistic rules? Let's begin by considering some clear cases, such as the uses of *rock* in (1)–(3):

(1) He threw a rock at me.
(2) I enjoy listening to rock.
(3) He's a real rock; I don't know what I should have done without him.

There are a number of criteria we could bring to bear here to justify our saying that *rock* is used conventionally in (1) and (2), but non-conventionally in (3). For one thing, the first two uses would strike native speakers as 'normal,' 'acceptable,' or 'non-deviant'; moreover, there is no more ordinary way to go about referring to this kind of stone or music. Whereas the use in (3) is intuitively 'deviant' or 'peculiar,' and bears a decided affective import, and does yield before more banal paraphrases. For another thing, we can easily see that the use in (3) could be derived from the kind of use exemplified in (1), and how (3) might be understood on first hearing even by someone who had never heard *rock* used in this way before. Whereas there is no clear connection between the uses in (1) and (2), nor would we suppose that a speaker might understand either use solely on the basis of his familiarity with the other. (It is true that we have nothing like an adequate account of metaphor, so that we cannot show exactly *how* the use in (3) is to be derived from the use in (1), or why for that matter the use in (2) could not be so derived. For the moment, however, we can rest content on our intuition that if we did have such an account, it *would* explain the relation between (3) and (1), or we would probably say it was defective.)

There is a very large number of intermediate cases, however, for which these criteria yield mixed results. These involve the phenomenon called polysemy, where a form that is intuitively 'one word' has several normal uses. For example, we may use *newspaper* to refer either to a publisher or a publication, or *chicken* to refer either to a kind of meat or a bird on the hoof, or *game* to refer either to a kind of activity or a set of rules. Like the mineral and musical uses of *rock*, these uses are entirely acceptable. Thus a speaker would judge that there was nothing deviant about sentences like *I ate chicken last night*, or *John has been working for a newspaper*, and it is the standard collective practice to refer to chicken meat and newspaper publishers in this way. At the same time, we feel that these uses of *chicken* and *newspaper* would be recoverable on first hearing by a speaker who knew the uses of these words to refer to birds and publications; or at least, we are very much aware of the relations

between the uses. But then do we say that these uses involve two separate linguistic conventions, or only one?

In some instances of putative polysemy, we may be able to argue that a single convention ordains the use of a word to refer to a single extension, of course. These are the cases that have often occupied philosophers who were concerned with the question of how to say when two uses of a word are truly 'different.' It is obviously of some metaphysical interest to know whether the *true* of 'true north' is the same as the *true* of 'a true story,' or whether *good* is used in the same way when we predicate it of actions, books, and screwdrivers. But what will concern us is what to do when a word is normally used to refer to extensions that are of distinct orders or categories in anyone's ontology, which is why the examples of *chicken* and *newspaper* are more suitable for discussion than other, more celebrated cases.

The standard practice in both linguistics and lexicography has been to rely on intuition to distinguish conventional and non-conventional word-uses. (Even when such use of intuitions is not made an explicit part of the methodology – even, in fact, where it is explicitly rejected in favor of such criteria as text-frequency – its heuristic usefulness is pretty much unquestioned.) So we say that the lexicon shall be responsible for giving us the 'meat' use of *chicken* because speakers judge the use as 'normal', 'acceptable,' 'non-deviant,' or whatever, and that it should not list under *rooster* a use to refer to 'people who "strut" and "crow"' because speakers judge that word-use otherwise. (And there is no reason to suppose that we would not reach the same conclusion on the basis of a count of text citations of either use.)

Now I have no brief to make against the *use* of such intuitions as data, but it strikes me that the *interpretation* that is usually accorded to judgements about lexical entries is unthinking and naive, especially when we contrast the careful scrutiny that linguists have given to the interpretation of judgements about syntactic well-formedness, and their relation to the assignment of grammaticality. When a speaker judges that a word-use is acceptable, after all, the best we can assume is that he is reporting on the acceptability of a certain social practice – that the use is 'conventional' in the weak sense of 'conventional wisdom' or 'conventional weapons.' Certainly we have no grounds for arguing from such judgements to the conclusion that the use is conventional in the sense of 'The Geneva Conventions' – that it is ordained by a linguistic rule.[3] As it happens, syntacticians have been most concerned to show that sentences may be unacceptable but grammatical, but there is nothing in the general program to disallow the opposite case, where an

acceptable sentence may be analyzed as ungrammatical (or better, 'non-grammatical'), and it has in fact been argued (Langendoen and Bever, 1973) that there exists a class of just such cases. In any event, this concentration on unacceptable grammatical uses comes with the syntactic territory, where attempts to make the grammar conform too closely to acceptability judgements may force the introduction of undesirably powerful devices into linguistic description. Where the lexicon is concerned, however, questions of generative power rarely arise. A short list of items is to be preferred to a long list, of course; no one wants to multiply entities beyond necessity. But what is at stake is usually an economy, rather than a simplification: a long list is not a different *kind* of device from a short one. And since *some* multiple uses of forms must be accorded independent entries, as required by cases of true homonymy, it appears that the only arguments remaining are over individual cases. These arguments may have an independent interest, of course, especially when an important concept is at stake (as with *mean* or *true*). But there seem to be no general grounds for refusing to treat the multiple uses of polysemous words as governed by separate conventions.

III

Now 'polysemy' is a gradient phenomenon, and there are certainly large numbers of cases in which we would have to ignore our intuition that two uses of a form instance the 'same word,' in favor of postulating two separate lexical conventions. I don't think it is unreasonable to say that English has at least two words *land* ('nation' and 'ground'), or two words *hood* (of a coat, of a car). What connection we feel between these uses does seem to owe more to our apprehension of an etymological relation between them (whether real or imagined) than to any synchronic process that derives one use from the other.[4] In extreme cases, we may make a connection between two uses of a word solely on the basis of some formal resemblance; thus Aronoff 1976 gives us the example of the several uses of *stand* ('rise to one's feet,' 'tolerate,' etc.) which may be associated on the basis of their shared past tense *stood*. But there is a much larger class of entirely 'acceptable' word-uses which we have very good reason for supposing are *not* fixed by separate conventions, on the basis of several kinds of evidence.

Let's take as examples the uses of *window*, *newspaper*, *chicken*, *chair*, *book*, *radio*, *France*, *Plato*, *game*, and *vanity* found in the following

examples:

(4)(a) The window was broken. (= 'window glass')
(b) The window was boarded up. (= 'window opening')
(5)(a) The newspaper weighs five pounds. (= 'publication')
(b) The newspaper fired John. (= 'publisher')
(6)(a) The chicken pecked the ground. (= 'bird')
(b) We ate chicken in bean sauce. (= 'meat')
(7)(a) The chair was broken. (= 'chair token')
(b) The chair was common in nineteenth-century parlors. (= 'chair type')
(8)(a) The book weighed five pounds. (= 'book copy')
(b) The book has been refuted. (= 'book content')
(9)(a) We got the news by radio. (= 'medium')
(b) The radio is broken. (= 'radio set')
(10)(a) France is a republic. (= 'nation')
(b) France has a varied topography. (= 'region')
(11)(a) The game is hard to learn. (= 'rules')
(b) The game lasted an hour. (= 'activity')
(12)(a) Vanity is a vice. (= 'the quality of being vain')
(b) His vanity surprised my friends. (= 'the extent of his vanity')

These uses have extensions that are quite clearly distinct, and it is easy to find things that can be predicated of the referents of one use that cannot be predicated of the referents of the other. Why then should we not say that they are governed by separate conventions? For one thing, these very same patterns of use show up in most languages spoken in communities like our own; compare the German or French synonyms for these words. In fact, I know of no language in which the same form is *not* used to refer to newspaper companies and newspaper publishers, or window-holes and window-glass, or game-activities and game-rules, and so on. And this should make us wary of saying that these regularities *could* be otherwise, which is a prerequisite for saying they are conventional. (I sometimes hear people repeat this formula as if what were meant was that things could logically be otherwise, or that we could conceive that things might be otherwise. This is much too strong, of course; what is required is only that some other practice should have been equally as rational as the one we have adopted.)[5]

But we don't have to look outside of the English speech-community to find evidence against multiplying lexical conventions in these cases. Notice that these patterns of multiple word-use show up in what Quine has called 'deferred ostension,' as well. I can point at a copy of a

newspaper, for example, and say,

(13) That was bought by Hearst last week,

and my intended referent may be understood, depending on context, as either a newspaper copy or the company that published it. (But note that I cannot identify, say, Scribner's by uttering (13) while pointing at a book.) Or I can point at an apple and say,

(14) That is nature's toothbrush,

to refer to the type that the apple I am pointing at exemplifies. Or at a copy of *Bleak House* and say,

(15) He was born in the same year as Browning.

And so on for other cases, at least to the extent that one of the uses involves reference to the kind of thing that one can point at at all. The parallel between polysemy and ostension holds even for those specialized uses that a word may have in certain contexts. For example, a restaurant waiter going off duty might remind his replacement:

(16) The ham sandwich is sitting at table 20.

And in just those contexts, he could equally well point at a ham sandwich and say,

(17) He is sitting at table 20.

Now these ostensive utterances cannot involve any 'ambiguities'; demonstrative terms have no lexical content at all, and there is nothing else in them that we could ascribe any ambiguity to. Nor could we explain the parallels by recourse to some theory of 'semi-sentences,' a set of pragmatic principles whereby the uses of descriptive terms could be mapped onto the uses of demonstratives. In the first place, such an account would be of no help in the *ham sandwich* or *Bleak House* cases, where there would presumably be nothing at all in the lexical entry about restaurants or authors. So we would require an independently motivated pragmatic account of deferred ostension in any case, which would have to look at the properties of things, rather than of the words that designate them. And we can reasonably expect that an explanation of how we can get from a sandwich to a customer or from a book to its author will also tell us how we get from a newspaper to its publisher, or from an apple-token to an apple-type. A 'semi-sentences' account, then, would be entirely redundant. And even more important, it would have no independent motivation. For what it would say, in effect, would be: 'In some cases in which a word is

conventionally used to refer to two different kinds of things, we can point at an instance of one of the word's extensions to identify an instance of the other'. But it would be unable to explain why these cases should be different from fortuitous homonymies, where there is no parallel in ostension. (You can't point at a savings bank, say, to identify a river bank.) All it would do, in the end, would be to extend the domain of a set of arbitrary, and hence synchronically inexplicable conventions for word-use; it would be an anti-explanation. Whereas if we begin with the independent pragmatic account of extended reference, we will be able to explain polysemy without having to introduce any linguistic conventions at all. That is, once we can show how it is possible to point at a newspaper to identify a newspaper company, we will have no trouble showing how it is possible to describe a newspaper to the same effect.

I haven't finished with deferred ostension; the phenomenon will be very important when we come to actually working out the rationale of extended reference. But for the moment, I want to turn a to a third kind of evidence for saying that separate conventions should not be postulated to explain all normal word-uses. This involves an observation that has been kicking around in the syntactic literature for some time now, which is that anaphoric rules that normally require some condition of 'linguistic identity' or 'co-reference' may ignore the difference between the kinds of word uses we have been talking about, though they are blocked by real homonymy. Thus, neither (18a) nor (18b) can be used to mean that Bill gave Harry a tool, and received a dossier:

(18)(a) Bill gave Harry a file, and received one from Jane.
(b) Bill gave Harry the file he got from Jane.

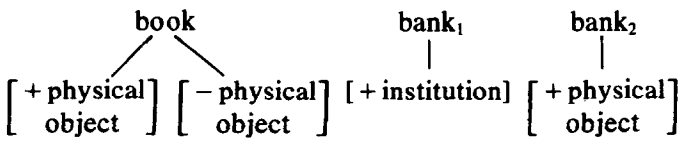
But alongside of (18), we have examples like (19)–(23), where an item has been deleted or pronominalized on one use under identity with another:

(19) Yeats did not enjoy hearing himself read aloud.
(20) The newspaper has decided to change its format.
(21) John's dissertation, which weighs five pounds, has been refuted.
(22) The chair you're sitting in was common in nineteenth century parlors.
(23) The window was broken so many times that it had to be boarded up.

At bottom, these syntactic regularities arise out of the same process that allows deferred ostension, but the connection has not been made by

syntacticians, who have for the most part regarded these cases as a fairly minor technical problem. There have been several proposals as to how sentences like (19)–(23) might be generated without compromising the notion of syntactic identity out of hand; see e.g., Langendoen (1966), McCawley (1968), Chomsky (1972), Postal (1969), Borkin (1972), and Green (1974). These differ considerably according to the syntactic frameworks that have been assumed, but we do not have to compare them on a point-by-point basis. What they all have in common is this: they take it for granted that all of the relevant word-uses are specified by the grammar, and try to establish some formal distinction between these uses and the uses of homonyms, to which syntactic operations requiring some kind of 'identity' can be made sensitive. For example, Chomsky proposes that the several senses of *book* be listed disjunctively under a single lexical heading, while the different uses of say *bank* would be given distinct lexical entries. We could schematize the difference as in (24):

(24)



In this way, Chomsky suggests, the requirement that rules of deletion and anaphora must operate on the 'same lexical item' can be preserved. Another proposal, due to Postal, would have these multiple uses generated transformationally. Thus (20) might have (25) as its underlying source:

(25) The publisher of the newspaper has decided to change the newspaper's format.

A 'beheading' rule would then delete 'the publisher of' prior to the operation of pronominalization, so that the identity condition – for Postal, co-reference – would be preserved.

I'll assume here that all of these proposals are descriptively equivalent: that there is no interesting set of cases that could be handled in one way and not another, whatever the cost to simplicity or explanatory adequacy. Nor is it relevant, for our purposes, whether we assume that these anaphoric devices are transformations or interpretive rules. What is important is that any attempt to distinguish formally between homonymy and polysemy in the grammar introduces unwarranted complexities, with no gain in explanation. Under any of these proposals, we

establish a new level of 'lexical identity,' which functions solely to facilitate the statement of identity conditions. (That is, rather than listing under every rule the sets of different items that will be treated as identical for purposes of application of the rule, we encode this information formally in the lexicon, and constrain the rule to look for this formal distinction.)

Now there are analogous maneuvers in morphology that are wholly justified. We may set up an independent morphemic level just to capture certain formal generalizations. So we say that there is one morpheme *stand*, which alternates with a past tense form *stood*, and which maps into several words *stand*, meaning approximately 'rise to one's feet,' 'tolerate,' and 'remain,' as well as into *withstand* and *understand*. Or that the *-mit* root of *commit*, *permit*, *admit*, etc., is a single morpheme that alternates with *-iss* in forms like *commissive* and *permissive*. And we establish this level solely on grounds of descriptive economy, to avoid having to list alternates for every word *affected*. (See Aronoff, 1976, for a discussion of these cases).

But these morphological alternations are purely arbitrary and language-specific; they have no synchronic explanation. Whereas the determination that two uses are instances of the 'same lexical item' for syntactic purposes clearly depends on the perception of some synchronic relation between them, as the observations about cross-linguistic patterns of use and ostension show. So we will naturally want to ask, 'Why should uses that are perceived as related in certain ways be formally distinguished from other patterns of use, and why should the syntax care?' That is, we are compelled to ask for a functional explanation for the difference. But if we have such an explanation, then why bother to postulate the formal distinction in the first place? If we can explain the difference pragmatically, why do we require as well an *ad hoc* syntactic account, especially at the expense of having to introduce another level in linguistic description?

We could point to other dissimilarities between the morphological and syntactic problems here. Regularities like the *-mit/-miss-* alternation are non-productive, in the sense that they affect only single forms. (So there is no temptation to generalize from *permissive to *edissive* or **flissive.*) But the set of relations between uses that can qualify them as instancing the 'same lexical item' seems quite general, and if we supposed that they were arbitrary, we would have a hard time explaining how speakers could learn just which perceived relations qualified. For example, consider the uses of *life* to refer to periods of time, as in 'His life was long,' and to living things, as in 'Is there life on Mars?' We can reasonably

ignore this difference for purposes of anaphora, as in:

(26) I'm not so much concerned with finding extraterrestrial life, as with prolonging ours on Earth.

But we more readily reject as jokes sentences involving anaphora across other uses of *life*, as in this example from Siegfried Sassoon:

(27) I prepared to abandon all that was dear to me in that countrified life for which I was to sacrifice my own.

Could this difference in judgement be due entirely to the fact that we happen never to have heard an example of deletion as in (27)? (For that is what we would have to say if there were no difference between the pairs of uses except in the arbitrary form of their lexical entries.)

All of these syntactic problems vanish the moment we assume that words like *newspaper* and *window* have only one conventional use, with other normal uses generated pragmatically. We can then establish as the only *syntactic* condition on identity that the element deleted or pronominalized must be the same word as its antecedent, with 'same word' defined intuitively as 'same form, same meaning.' The grammar may be indifferent to the fact that the word is used differently on one or the other occurrence, and the circumstances under which deletion across different uses is permitted can be explained entirely in terms of the communicative functions of the relevant operations; in particular, in terms of the account of 'identifiability' that we would need to handle deferred ostension. That is, if we have an account of how it is we can point at a newspaper to identify a publisher, we will also have an account of why identification of a newspaper copy with a relative clause is sufficient to identify a publisher, and so we will not have to worry about the contrast between examples like:

(28) John used to work for the newspaper (*book) that you're reading.

On the basis of these arguments, it seems clear that there are compelling reasons for supposing that we neither need nor want to postulate separate conventions governing all of the word-uses that speakers judge normal or acceptable. But this leads us only to conclude that the lexicon itself undergenerates the class of acceptable uses; all that we have to do, it would seem, is to restrict the number of lexical conventions to govern a subset of normal uses, from which the others can be pragmatically derived. To make the point I started with, then, a second argument is required, to the effect that there is no determinate way of accomplishing

this reduction, and that there is a substantial class of cases where we have no principled grounds for deciding which of several uses is conventional; no matter how severe an idealization we allow. But it will be easiest to describe the procedures we use to decide which of a word's uses is conventional if we first have an account of the schemata that allow the derivation of non-conventional uses in the first place. Moreover, this account of extended reference should have considerable interest for its own sake, since it is crucial to the understanding of a number of important phenomena that are not directly related to the question at hand.

IV

A pragmatic account of polysemy will explain to us how a name or general term can be used to refer to something in the absence of a linguistic convention for doing so (and as such, it will perforce be an account of those metaphorical word-uses that are not judged normal or acceptable, as well.) But as we have already seen, the uses of names and descriptions are paralleled by the uses of demonstrative terms, so an account of polysemy must in the end follow trivially from a general account of deferred reference, which will tell us, 'Under what circumstance can we point at, name, or describe some thing *a* so as to succeed in referring to some other thing *b*?' That is, we will presume that we have a way of getting from a name to its designatum, or from a demonstrative to a physically present demonstratum, and proceed to ask how we get from that thing to something else. So it will be distinct from a theory of linguistic meaning, of how we get from words to things; it will be concerned exclusively with speakers and things, and will have nothing to say about words at all. In order to ensure that considerations of meaning do not sneak into the picture, however, it will be easiest to set out the account exclusively in terms of ostensive reference, where descriptive content plays no role, and then generalize to the uses of other referring expressions.

I say 'account' rather than 'theory' because we cannot offer a satisfying explanation of deferred reference if we have to introduce any independently unmotivated axioms or *ad hoc* hypotheses. (That is where all accounts of metaphor have failed, in having to postulate 'principles of comparison' or the like that have no independent basis.) Accordingly, we will want to be able to derive all of the observations we need from a definition of reference itself, taken in concert with some uncontroversial assumptions about speaker beliefs and motives.

Let me follow Searle (1969) in saying that a speaker has succeeded in referring to *a* if he has enabled his hearer to identify *a*, to provide a description which is true of *a* and false of everything else; or if a speaker has given his hearer reason to believe that he *could* provide an identifying description of *a*. This is not a sufficient condition for successful reference, to be sure – the hearer must recognize the speaker's intention to refer, and so forth – but we can ignore for these purposes those aspects of reference that are part of the more general theory of speech acts. We need not ask, over and above this, that reference be 'fully consummated' or 'referential'; it will suffice that the hearer should be able to give only one identifying 'attributive' description of the referent.[6] (Thus, we will say that I have succeeded in referring when I point at a newspaper and say, 'Hearst bought that,' where my hearer could be expected to know of the intended referent nothing more than that it is 'whoever publishes that newspaper.')

In ostension, we can succeed in referring in any of three ways. We can point at the intended referent itself, or at a part of the intended referent, or at something that stands in a certain uniquely identifiable relation to the referent. Each of these methods introduces new wrinkles and uncertainties, but most of these we can ignore here. For one thing, speaker and hearer may not agree as to which of several objects is being pointed at (say, if one of them is astigmatic). For the present purposes, we will assume that in all cases, speaker and hearer can provide identical descriptions of the demonstratum. A more general and vexing problem is Wittgenstein's example of ostension of a red square, where we may be construed as pointing either at an example of red or an example of squareness, but this question won't affect our discussion either, since in clear cases of deferred ostension, the intended referent is in no way physically present in its entirety.

When we point at a part of the intended referent, the opportunity for other indeterminacies arises. For example, suppose I point at the Statue of Liberty from the rail of a ship and say, 'That is a heavily populated region.' Clearly, my intended referent is something of which the visible demonstratum is only a part, but my hearer may be uncertain as to how far the actual referent extends: I might be referring to Bedloe's Island, to New York City, to the Northeast, and so on. Goodman (1968) seems to suggest that all cases of deferred ostension can be reduced to this sort of situation, which he calls 'exemplification.' But this won't take us far enough, for there are clear cases in which the intended referent is not physically present even in part. Adapting an example of Quine's, I might point at a gas gauge and say, 'That is why I won't buy an eight-cylinder

car,' where the referent of the demonstrative is something like 'the price of gas.' Or consider the case we talked about earlier, in which I point at a newspaper copy and say, 'That was bought by Hearst last week,' where it cannot be that what Hearst bought included this very copy.

In discussing the gas gauge example, Quine (1969) suggests that we can sometimes succeed in referring when we have 'a correspondence in mind.' I'll devote the rest of this section to an elaboration of this notion. When we cannot point at the referent itself, we may equally well succeed in identifying it by pointing at something that stands in a certain unique relation to the referent, provided we can expect our hearer to be able to identify just what relation it is that we have in mind. We might do this explicitly, saying, for example, 'The author of that is a friend of mine,' combining description and ostension to identify the referent. But it is often the case that we do not have to describe the relation; our hearer, knowing that we intend to refer to something that stands in a certain relation to the demonstratum, may easily be able to infer what the relation is. Thus, we can point at a book and say, 'He is a friend of mine,' leaving the problem of identification of the relation entirely to the hearer. The problem of explaining deferred ostension really boils down to this: what governs the hearer's determination of this relation?

It will be easiest to talk about such relations as functions that take demonstrata into intended referents, but I should make it clear, if only for conscience sake, that I will talk of functions only by way of modeling propositional knowledge. (Thus, statements of the form '*H* knows that the intended referent is the value of the function "*x* is the publisher of *y*" at the point picked out by the demonstrative gesture' as meaning, '*H* knows that the intended referent is the unique thing that publishes the demonstratum,' and so forth.) Let me call the *referring function* (RF) that function that the hearer (correctly) selects from among an indefinitely large number of functions that take the demonstratum as arguments.

Let us assume that the referring function is derived from among a finite number of what we may think of as 'conceptually basic functions' – 'type of,' 'source of,' 'possessor of,' and so on – which are defined over 'natural' ranges and domains. These in turn can be combined to form a potentially infinite number of composite functions, such as 'source of type of,' which together with the basic functions exhaust the possible relations that may hold between a demonstratum and an intended referent. The problem facing the hearer, then, is to winnow this set down to one referring function.[7]

Several factors constrain this choice. First, the range of the referring

function must intersect the 'range of reference' – the set of things that the speaker might rationally be construed as intending to refer to in a given context. The range of reference is determined by the nature of the predication, by the morphology of the demonstrative pronoun, and by such contextual considerations as 'topic of conversation.' Thus, if I point to a book and say, 'he is popular,' my hearer may conclude that I am referring to an animate male (from the choice of pronoun), to something of which current popularity can be predicated, and, supposing that we have just been talking about authors, that I am more likely intending reference to the book's author than its subject or illustrator. So the RF must be some function that takes things like the demonstratum into some part of this range. Equally obviously, the demonstratum must itself manifestly fall within the natural domain of the RF. 'Manifestly' is the key here; the fact that the demonstratum falls within the domain must be a matter of what Lewis (1969) calls 'common knowledge.' So for example, I can point at a fifty-cent piece, or a at a 500 lira note, and say 'That's what they get for a subway ride now'; both demonstrata are manifestly in the domain of the function 'x has a fixed monetary value of y.' But I could not succeed in referring in this way if the demonstratum were a large stone wheel, unless I knew, and expected my hearer to recognize that I knew, and so forth, that the demonstratum was in fact a Yap fifty-cent piece. We might state these conditions formally as

(I) Given a demonstratum $a \in A$, and a range of reference R, and a possible referring function f: $A \rightarrow B$, f allows derivation of a referring function only if it is common knowledge that $a \in A$, and that $B \wedge R \neq \emptyset$.

But by itself, this scarcely constrains the number of possible RF's; there are an infinite number of functions, after all, that will satisfy condition (I) on a given occasion of reference. To narrow down this number, we will have to look more closely at the notion of 'identifiability,' and ask, 'Under what circumstances will the knowledge that an intended referent b is the value of a certain function f at a enable us to identify b?' By way of example, consider the function 'x was produced in y,' which takes artifacts into years. We could use this function in referring, pointing at a glass of wine or at a 1959 Chrysler and saying, 'We lived in Toledo then.' But we would be unable to use the function in this way if the demonstratum were a glass of beer, or a 1965 VW Beetle.[8] Of course, these things fall within the domain of the function as well; the difference is that we cannot easily distinguish a 1965 Beetle (or

Bud) from a '64 or '66. (I'll assume that the participants in the conversation are not brewers or mechanics.) That is, the knowledge that the intended referent is describable as 'the year in which that was made' will not be very helpful in identifying it, if neither the speaker nor hearer has any obvious way of determining what year that was. So a speaker who used this function in referring if the demonstratum were a beer or a Beetle would be at best uncooperative, and his hearer (who must after all assume the speaker's cooperativeness in trying to make sense of what is being said) would be unable to determine the function, or to calculate the reference.

We could most easily state this condition on selection of the RF as a condition that has to be satisfied by the inverse image set of the function, saying that an RF has to be such that we can discriminate the set of things for which its value is the intended referent from the other things in the domain of the function at which it yields different values. Put formally:

(II) Given a demonstratum $a \in A$ and an intended referent $b \in B$ in a range of reference R, and a possible referring function f: $A \rightarrow B$ that satisfies condition I above, f can be used in referring only if it is common knowledge that $f^{-1}(b)$ is discriminable from every x such that $f(x) \neq b$.

But this obscures more than it clarifies, and the point is put most simply in English: for practical purposes, something can be *identified* as standing uniquely in a certain relation to something else only if it is possible to *determine* in practice just what stands in that relation.

But this condition ensures only that an intended referent *can* be identified as standing uniquely in a certain relation to the demonstratum; it doesn't address the problem of how easy such an identification would be. Consider analogous uses of descriptions. We might describe the same individual in any of several ways, all of which pick him out uniquely, but some of which make practical identification easier than others do. For example, Searle (1969) contrasts descriptions like 'the junior senator from Nebraska' and 'the only man in Omaha with exactly 8756 hairs on his head.' Obviously, it would be easier to identify the same man in virtue of his official status than in virtue of the number of hairs on his head; the presence of the first property is more salient, and hence more easily validated. By the same token we may more easily identify Charles Dickens as 'the author of *David Copperfield*' than as 'the author of *The Mystery of Edwin Drood*,' since the first work is not so much better known than the second. Here, the difference is not so much one

of actual 'salience,' but rather in the degree to which the identifying property is likely to be associated with the referent in common knowledge; literal salience is only one of the factors that determine this. And finally, we are more likely to facilitate reference to something by virtue of properties that are generally considered intrinsic or criterial than by virtue of properties that are considered accidental or contingent. I could equally well refer to John Quincy Adams as 'the president who was the son of John Adams,' or 'the president who died on the same day as Thomas Jefferson,' but the first description would make identification easier, since in most cases criterial properties are more likely to be part of common knowledge.

Now with descriptions like these, there is no real question of failing to refer, so long as the description does pick out a unique individual, because the referring function is explicitly given. The best we can say is that some descriptions are more useful than others, so that a speaker who deliberately chooses a description that makes identification more difficult is violating Grice's cooperative principle, for whatever reason. But with deferred ostension, we have to figure out which function is being used, and considerations of the relative likelihood of identification enter into the determination itself. Suppose that we have a demonstratum a, and a range of reference R, and two functions f and g which take a into different members of R, and which satisfy condition (II) above. For example, let the demonstratum be a sports car, and let the range of reference be the set of male humans, as determined by an utterance of 'he is a friend of mine.' And let f be the function 'x is usually repaired by y,' and g, 'x was designed by y.' (Suppose also that there is nothing in the context to suggest that we are more likely to be talking about mechanics than designers, or vice-versa.) Now obviously, the function from car to designer is 'better' than the function from car to mechanic; the designer can be more readily identified as 'the person who designed that car' than the mechanic can be identified as 'the person who repaired that car.' So if the hearer assumes that the speaker is being rational and cooperative, he might reason as follows: 'The speaker *could* intend to refer either to the mechanic or the designer, and knows that I know this, and so on. If he intends to refer to the designer, identification would be relatively simple; if he intends to refer to the mechanic, it would be more difficult. Assuming that there is no independent reason for preferring one or the other referent, I must assume that he intends that I should select that referring function which makes identification easiest; otherwise he would be asking me to make a more difficult calculation in the absence of any reason for rejecting a simpler one.'

Allow me to borrow the psychologists' notion of 'cue-validity' to refer to the probability with which a given referent *b* can be identified as being the value of a certain function *f* at a demonstratum *a*, or more generally, to the relative usefulness of a given description for purposes of identification. All things being equal, we will assume that given an array of possible RF's which take a demonstratum into a range of reference, a rational speaker will intend that his hearer should select that function that has the highest cue-validity for its referent. In other words, when a demonstratum stands uniquely in several different relations to several members of a range of reference, and there is no reason for assuming that any one of these members is a more likely candidate for reference than another, we will assume that the intended referent is that member which is most easily identified in terms of its relation to the demonstratum.[9]

As we have already seen, the cue-validity of a function (or description) depends on a number of assumptions about the information in the contextual background; I'll return to these below. Even in a schematic form, however, this principle has general consequences of some interest. For one thing, the reader may have noticed that we did not define the range of reference above in such a way as to exclude the possibility that the demonstratum was itself a member of the range. Thus, if I point at a hat and say, 'That is a derby,' we would normally understand the range of possible referents as including the hat itself. And under these circumstances, the hat itself would be the only thing that I could actually be referring to, if I am being rational. The general point is this: when the demonstratum could be the referent, it must be the referent. Now this is no more than another instance of a principle that comes up repeatedly in discussions of metaphor, which is that a word-use can be construed metaphorically only when it cannot be construed literally. (On such grounds, Grice quite rightly assigns metaphorical word-uses a status as conversational implicatures.) But this principle follows from the more general observation about cue-validity that we made above. The identity is a function too, after all, and it is the only function whose value is trivially computable for all arguments in all domains. So where the identity could be the referring function, its cue-validity must be higher than that of any other possible RF, and it must be chosen. (A consequence of this way of approaching things is that we do not have to distinguish between the principles that govern direct and deferred ostension, or extended reference and reference *tout court*.)

The cue-validity principle has analogous consequences for the use of composite functions. Such functions are frequently used in both osten-

sion and description. For example, I could point at a photograph of Mick Jagger and say, 'Oh, have you bought tickets for that yet?' intending reference to a forthcoming Rolling Stones concert. In that case, the RF would be a composite of at least two functions, one of which takes the picture into its subject, and another of which takes that subject into an event.[10] Other uses of composite functions are equally normal. Consider the function involved in Figure 1, for example. The RF here is probably best described as a function that takes a picture into its subject, and its subject into a disposition into the circumstances that led to its formation. The referent, in other words, is something like 'the circumstances that led this woman to allow herself to pose for this,' though I admit a certain inscrutability here. (Note that a different context might lead to a different interpretation here as well; if this were an ad for Kodak film, the referent of the demonstrative term could be the blurriness of the photo.)

In both principle and practice, then, we have every reason for assuming that composite functions are used in referring. This might seem to lead to a problem, though: from the value of one function that takes us into a range of reference, we can always find another function that takes us to another member of that range. For example, we can't point at a picture of Mick Jagger and say, 'Have you bought tickets for that' intending reference to the Beach Boys concert following the Rolling Stones. But the use of such functions is ruled out by the same factors that force us to make the RF the identity when we are able to do so: the cue-validity is axiomatically lower than the cue-validity of their first-computed element. (Thus, the function from the picture to the Rolling Stones concert must be better than the function from the picture to the Beach Boys concert; the probability of success in identifying the value of the second cannot be higher than the probability of success in identifying the value of the first.) We can state in an informal way the principle that governs both these cases and the cases in which the identity itself is the RF as follows: once you have a function f that takes you from the demonstratum to a member of the range of reference, you cannot use in referring any non-trivial composite function that includes f as its first-computed element. Even more simply: once you get to a possible referent, stop.

We could continue to refine this account of ostensive reference. For example, there are reasons for distinguishing between 'attributive' and 'referential' uses of demonstrative terms, according to whether it is common knowledge that the thing that is a value of a possible RF has certain other identifying properties, which may give the hearer in-

Figure 1

dependent grounds for concluding that that value does or doesn't fall within the range of reference, and so would affect the way in which the restrictions on the use of composite functions would be stated formally.[11] (In Nunberg (1978), I went to considerable lengths to sort out

these cases as explicitly as possible, and to state all of these conditions formally. But while I don't regret the time spent on that exercise, I do think that it proved a bit distracting. In the end, as we noted earlier, this is not a *theory* of reference, but simply a working-out of what follows from a definition of reference, given some reasonable strategies of inference.)

I should note, however, that whatever conditions we may be able to derive will not be sufficient to predict the acceptability of many ostensive utterances. In presenting some of this material to linguists, I have often been told that certain of my examples were 'bad,' even once confusions about possible contexts of utterance had been resolved, and I see no reason to dispute these judgements, even though the cases in point involved no violations of the principles I have sketched out here. There are several other pragmatic factors that help to determine the felicity of a given ostensive utterance, and their interaction with these principles is impossible to reckon exactly. I don't want to spend time discussing these factors at length, because they affect only the uses of demonstratives, and not the uses of names or descriptions, and so they are irrelevant to the general account of extended reference I'm trying to develop. But two of them are worth mentioning, if only as a cautionary measure.

First, we should keep in mind that a speaker is rarely forced to use a bare demonstrative term. He can always resort to a description, like 'the newspaper on the table,' or a combination of description and demonstrative, like 'the publisher of that' or 'that newspaper.' So at a certain point, even if bare ostensive reference is possible in principle, the difficulty in calculating the reference of the demonstrative term makes it worth the speaker's while to go to the more explicit, if more prolexic descriptive expressions – the maxim of clarity overrides the maxim of brevity. And although this consideration obviously affects the acceptability of ostensive utterances, there is in principle no way to predict just how such clashes of maxims will be resolved on a given ocasion.[12]

Another consideration that affects acceptability is purely linguistic: the way demonstratives are used depends in part on what sorts of demonstrative and indexical paradigms a particular language makes available. In English, for example, we might want to distinguish between 'true' demonstratives like *this* and *that* and demonstratively used pronouns like *he* and *she*, if only on the basis of the fact that only the true demonstratives are inflected for the position of the demonstratum relative to the speaker and hearer (we may think of this as a difference in 'indexical character,' or in 'degree of deixis.') Note that the choice of

this or *that* is determined by the position of the demonstratum, but the choice of singular or plural is determined by whether the *referent* is an individual or set; thus, I can point at a single apple and say, 'Those are delicious.' When personal pronouns are used demonstratively, by contrast, the inflection is generally determined by properties of the referent.[13] Note also that it is not simply the animacy of the referent that determines whether a true demonstrative or a pronoun is used. For one thing, though *it* cannot be used demonstratively, most speakers accept the demonstrative use of *they* with inanimate referents.[14] And *this* and *that* can be used to refer to animates in 'identificational' copular sentences; thus we can say, 'That is my piano teacher,' if not 'That taught me piano.'[15] Moreover, there is a difference between uses of *that* and *he/she* to refer to animates. Pointing at a person, we could equally well say 'That's my piano teacher' or 'He's my piano teacher.' But if the demonstratum is the ring of a doorbell, we can use only *that*, even if the sex of the teacher is known to speaker and hearer.

It would be quite a job to set about explaining all of these regularities, and while the project would clearly be worthwhile, it wouldn't bear directly on the problem that concerns us here. We have been assuming throughout only that the choice of one or another demonstrative term enables the hearer to pick out a demonstratum, and that the demonstrative term itself gives him some information about the referent. Obviously, the more information he has about the referent, the easier deferred ostension will be, and so the acceptability of particular examples may be affected by what kind of demonstrative term is used, given what the language makes available. (It also follows that there will be cross-linguistic differences in the acceptability of particular examples. In some languages, like French, the demonstrative term may signal the grammatical gender of the name of the basic-level category to which the referent would be assigned, a piece of information that may be useful in calculating reference. In other languages, like Chinese, there will be no gender information encoded in the personal pronouns that are used demonstratively.) But what we are interested in is how reference is determined *given* the available information, as signalled in part by whatever inflection accompanies the demonstrative term.[16]

V

There are other refinements that we might want to make in this account of ostensive reference that are equally relevant to the uses of names and descriptions, and these we will want to present in the context of a

general account of extended reference. Ostension and description differ in several important, if obvious ways. (I'll use 'description' here only to refer to a kind of act, keeping it parallel to 'ostension,' and will use 'name' indiscriminately for the terms that appear as the head of an NP used to describe.) First, names have the same designata whenever they are used, while indexicals do not. And second, in ostension the argument of the RF is always physically present, whereas in description it need not be – indeed it need not even be a physical object at all. Now the first difference has always seemed the most striking, and I will first say something about it. But really it is the second difference that will be of most interest to us, in a moment.[17]

Let us simply assume that names 'designate' things and kinds or classes of things. By which I mean, I will assume that there is a convention whereby a name like *Nixon* is used to refer to a certain person, and *dog* or *baseball* to certain kinds of things, and that it is in virtue of these conventions that all of the referring uses of these terms are possible. I won't be concerned with the content of these conventions: in particular, with the question of whether these conventions relate words and concepts or words and things – that is, whether senses are part of language. Nor does it matter whether the designata are 'really' there; that question, like all of metaphysics, is irrelevant to linguistic description. And I will further assume that speakers ascribe certain properties to the designata of names, though again for our purposes it is unimportant that we be able to say which of these are considered criterial to establishing the identity of an individual, or membership in a class.

The second difference between ostension and description – that the argument of the referring function does not have to be present – is what leads us back to the problem that we set out earlier: how do we know which of the things a name can be used to refer to is the one it designates; that is, which of the uses of a word is conventional? In discussing ostension, we could confidently assume that we knew when the RF was the identity and when it was not; that is, we always know when the demonstratum is itself the referent. But with descriptions, the designatum of a term is at as much of a remove as any of its normal referents, so we have no easy way of establishing when the RF is the identity. All we can know for certain is the sum of normal uses a name can have; it requires analysis to determine which of these is conventional.

In many cases, of course, we have clear intuitions as to which uses of a word are 'basic' or 'central,' and which are 'secondary,' 'metaphorical,'

or 'derived.' We feel secure in saying that *chicken* names a kind of bird, rather than a kind of meat, or that *Dickens* names a writer, rather than an oeuvre. But we have already seen that judgements like these have to be interpreted before they can be taken as evidence for anything. And more important, there are many cases of multiple use for which we can't have clear intuitions that one or another use is prior. Take the use of *game* to refer to activities or sets of rules: of *window* to refer to holes or the things that go in them; of *book* to refer to inscriptions or contents; of *gossip* to refer to a kind of activity or a kind of information; of *captain* to refer to a rank or to the people who hold that rank. (I've asked a lot of people about examples like these; they react in two ways. Some – I am among them – confess their aporia. Others express confident judgements one way or the other, though with no consistency from subject to subject or case to case, and with each citing a different reason for his choice.)

Assuming that these last patterns of word-use are determined by a single convention – they do satisfy the tests of syntactic identity and ostensive identification that we talked about earlier – and that our intuitions about them are at best labile, then on what grounds do we decide what the relevant convention is? It isn't terribly important that we be able to come with an operational test that will make it possible to grind out consistent results with real subjects, so long as we can say what criteria we would use in principle to decide which of two word-uses is conventional, and that these criteria would in principle be applicable to all cases. (As I stressed at the beginning of this paper, I am not quarrelling with the necessity for some idealization, perhaps severe, when we set up the semantics/pragmatics distinction in the first place.)

We can best proceed here as we would when faced with similar uncertainties about well-formedness in syntax or phonology, by arguing first from the clearest cases. There is no reason to doubt that speakers are right when they arguing first from the clearest cases. There is no reason to doubt that speakers are right when they judge that the 'bird' use of *chicken* is basic – conventional – and that the 'meat' use is derived. After all, an ability to recognize and understand truly novel uses is required if speakers are to understand metaphors, and we know that they can do that, so we should be generally willing to trust them when they tell us confidently that one word-use is derived from another. But we will want to ask *how* they arrive at this judgement. Suppose a language-learner encounters two uses of a word *w*, to refer to both *a* and *b*. And suppose also that he has good reason for supposing that *w* is not simply homonymous – that there is only one convention governing the use of *w* – say by applying some version of

the tests we offered earlier in sorting out polysemy and homonymy. And finally, suppose he has no reason to assign *a* and *b* to the same category, no matter how generously he figures things, and so could not argue that *w* was simply 'vague.' (That is, suppose that the uses of *w* are much more distinct than the uses of *good* or *true* that philosophers have worried over.) What theory about the relation between the uses will he arrive at, and why?

In practice, of course, his decision that one use is derived from the other may be affected by such things as the relative frequency of the uses, or the order in which he has learned them. But I don't think such considerations will ever be crucial. For one thing, some words (*chimera, Waterloo, whitewash, pits, vanquish, golden, soul*) are probably used at least as often metaphorically as literally, yet we recognize the metaphor if we also know the 'literal' use. For another, we *do* reanalyze some word-uses as derived when we learn of other uses of the same word. (For most of us, *momma* was at one point a proper name.) In any event, I'll assume that our language learner takes no notice of such things in constructing his theory.

Let me start with the regularity whereby the name of any artist (*Dickens, Vermeer, Beethoven, Dior*) can be used to refer either to a person or an oeuvre. Take a case where we don't know much about the biographical details: say a speaker has heard the name *Caedmon* only twice, in reference to an Anglo-Saxon poet and a body of Anglo-Saxon peotry in a sentence like (29) for instance, which assures him that both uses follow from a single convention:

(29) Caedmon, who was the first Anglo-Saxon poet, fills only a couple of pages of this book of poetry.

And let's suppose also that the speaker has never heard this sort of use before, but comes armed with 'normal' theories about writers and poems. Then he must decide whether the references are made possible in virtue of a referring function that takes a writer into his works, or vice-versa. Note that functions are available either way which satisfy the conditions of reference we laid out above for deferred ostension: we could as easily point at a book and say, 'He is a neighbor of mine', as we could point at an author and say, 'That is difficult to read.'

But these functions are not necessarily equivalent. If the speaker assumes that others are being rational and co-operative, then he assumes that they must identify everything in such a way as to maximize the possibility of successful reference; that is, that they will identify everything as the value of the RF with the highest cue-validity for that thing. Now the function guaranteed to have the highest cue-validity is always

the identity, and if a word has only one use, we must always assume that it names the thing it is used to refer to. But we cannot assume that the RF is the identity for both uses here; that would be equivalent to saying that *Caedmon* was homonymous, with two conventions governing its use. So we have to ask which of the two possible RF's that are not the identity would make for more rational reference. That is, if *Caedmon* names the poet, how rational would it be to use it to identify the works? Would this use be more or less rational than its use to identify the poet, if it named the works?

We identify the members of an oeuvre by common properties of style, content, and sensibility, and we customarily explain these commonalities as the result of properties of its author, which inform his intent. Thus, if Caedmon (the poetry) is criterially pious and trite, it is because Caedmon (the poet) was (or feigned to be) pious and trite; it could not have been as it was if he were not as he was. So in the absence of a name for the oeuvre itself, there would be no better way to identify it than as 'the stuff that Caedmon wrote,' for we assume that nothing not written by Caedmon will truly have the properties that qualify it for membership in this oeuvre, even if we are not exactly sure what properties these are. On the other hand, suppose *Caedmon* named the works. It is true that we *could* then identify the man as 'the author of Caedmon,' but this would not be the best way of identifying him. It is hard to say just what criteria are most important for establishing personal identity in folk metaphysics, but they seem to be wrapped up with circumstances of birth, lineage, and physical properties. Caedmon – that very man – could have died in infancy, taken a vow of silence, or written *David Copperfield* without our ever being tempted to say that he was not the same person. And since in description we have free choice among all possible ways of identifying the man, we could do better here than to pick him out by a property so contingent or accidental as his having written these poems. In short, we have a much better function from the singer to the song than from the song to the singer. So if we assume that speakers are behaving in such a way as to maximize the probability of successful reference in both cases, we will assume that they analyze *Caedmon* as naming a man, and that they use the function from the man to the works because it is the best of all available means of identification.[18]

Schematically, we could put this as follows: given a word w which is used to refer to two distinct extensions a and b, where we have reason to believe that only one convention governs the use of w, we will determine which of $\{a,b\}$ is the designatum of w by computing the cue-validity for b of the best function f such that $f(a) = b$, and the

cue-validity for a of the best function g such $g(b) = a$. If the cue-validity of f for b is higher than the cue-validity of g for a, we will assume that w designates a; if it is lower, that w designates b.

Some other examples may help to clarify the procedure. Take the use of a name like *Stardust* to refer both to a song and to the class of phonograph records on which the song is recorded. All of the properties that distinguish these records from others – the name they bear, the configuration of their grooves, the series of sounds they record – are representable as the values of functions from properties of the song *Stardust*; they would not be as they are if it were not as it is. But the identity of the song is in no way dependent on properties of the records; it did not have to have been recorded at all, in fact. So it would be quite rational to identify the records as 'the class of records on which that song is recorded.' But there would be better ways of identifying the song than as 'the song that is recorded on those records,' since this does not pick it out in virtue of a criterial, or even relatively criterial property. Schematically, the cue-validity of the function from the song to the records is higher than the cue-validity of the function from the records to the song; so people are behaving most rationally if we assume they are using the first.

Finally, an intermediate case. Suppose we have have a body of poetry called 'Fungoids,' which we know to have been produced by a computer program also called 'Fungoids.' Which way we go here, I think, will depend on what we know about the software. If it is simply a black box that performs no transformations on the input, then we would likely say that *Fungoids* designates the poetry; all that distinguishes this program from others, after all, is that it represents this poetry. But suppose the program generated the poetry from a random input according to certain heuristics. Then we would say that *Fungoids* designated the program, since the properties that distinguished the poetry from other poetry would depend on the properties that distinguished the program from other programs. (In the first case, this same program could have porduced no other output, but this same output could have been produced by entirely different means. In the second case, this same program could have produced another output, but this same output could not have been produced by other means.)

In determining the meaning of a word, then, we assume no more than that speakers are operating in such a way as to maximize the probability of successful reference, by choosing the best from among the array of available referring functions. And this is just the procedure that we use in calculating reference in the first place, as we saw earlier. The

determination of meaning, then, is no more than the evaluation of the calculations that we make in determining reference.

To be sure, this evaluation is not easy in practice, and when we ask about real words in real speech-communities, several problems come up. First, we have seen that calculations of cue-validity are made against a set of background assumptions that may vary from context to context, from place to place, or from time to time. As Bede tells the story, for example, Caedmon was an illiterate shepherd who was visited in the night by an angel who gave him his songs. If we believe this – or any other theory about the relation between authors and their works that is radically different from our own – then we may come up with a very different analysis of the meaning of *Caedmon*, even given the same pattern of use. We might decide that the word named the poetry, or even the angel, rather than the shepherd. And the analysis of the uses of any other word may be equally dependent on assumptions that are no less variable.

We have up to now ignored the possibility of discrepancies in such background assumptions, and have assumed that presuppositions remain fixed from speaker to speaker and context to context. But it should be obvious that we can do this only against an idealization that is more severe than anything that Saussure or Chomsky ever dreamed of, since we would require not only absolute homogeneity in linguistic practices, but also in speaker assumptions about what constitutes the common ground against which all conversations take place. This is not quite the same as requiring homogeneity of beliefs, for beliefs are relevant to the determination of the background only to the extent that they are common knowledge. But it is nonetheless an unimaginable state of affairs, requiring a community in which there is no significant differentiation in roles, interests, norms, classes, or specialized knowledge. (I say 'unimaginable' because I find it impossible to say how such a community would be constituted, or indeed, what uses its members might put language to, beyond the expression of greetings and collective prayers.) In any real community, a speaker must face the possibility that others will come up with alternate analyses of the same pattern of word-use, according as their apprehensions of the background assumptions may vary. And this will obviously affect his own analysis, because *his* theory about the meaning of a word is necessarily a theory about what it is rational for other speakers to do, given their beliefs. (A use of a word that is rational only against a purely private belief is not rational at all, since we have no reason to expect that another speaker will understand it.)

Discrepancies in presupposition are indirectly behind the other main problem the learner of a natural language faces in determining meaning, for they are what lead diachronically to the processes of conventionalization and idiomatization, whereby a use of an expression is continued by a community even after a change in collective beliefs has altered its rationale so that the usage is licensed more by precedent than by purely synchronic considerations. (Thus we can assume that there was a time at which the use of *nylon* to refer to stockings would have been entirely rational in the absence of any convention, but speakers have continued to conform to that regularity even though the cue-validity of the function from nylon to stockings has decreased over the years, owing to the introduction of new elements into the scene.) The problems raised by partial motivation are pervasive, not only for semantics, but for syntax and morphology as well. (And I might add in passing that our intuitions are no better as guides in determining which usages are idiomatic than in determining which usages are conventional.) I won't go into any of this in detail, because we would then have to engage the problems raised by heterogeneities in belief-systems, which we have so far been able to avoid. But I do want to mention at least one example of the difficulties we face in analyzing the actual usuage of a fairly typical word, before returning to cases that are more suited to analysis in the austere world of our idealization.

The word *cell*[19] can be used to refer to a number of different kinds of things: to cells of the body, prison cells, battery cells, Communist cells, photocells, the cells of a matrix, and so on. And it is tempting to say that all of these are members of a single 'vague' extension – say, the set of things that are the uniform constitutive parts of a larger structure. But this won't do for all the uses. Biological cells do not have to be parts of structures, for example, since we can speak of single-celled organisms. And we can use *cell* to refer to the parts of a political organization only when they are clandestine; we can't talk about the 'cells' of the League of Women Voters. Similarly, prisons and monastaries can have cells, but libraries or cruise ships can't, for all that the latter may be divided into compartments. But at the same time, it is clear that all the uses of *cell* are perceived as related by some set of functions, even though some of them must have become in some measure conventionalized. And new uses can be coined whose sense is recoverable on the basis of familiarity with old ones. We do not have to be told which parts of matrix are its 'cells,' for example, and a cigarette manufacturer can confidently tell us that a filter is divided into thousands of 'tiny little cells,' without worrying that we have not heard *cell* used in this way before. Still, it

would be very hard to say just *which* of the other uses of *cell* these new ones are derived from; that is, which sort of thing *cell* designates. Only this much is sure: the proper analysis of *cell* probably involves some complex combination of the notions of vagueness, polysemy, and conventionalization, and speakers are unlikely to agree point-by-point as to just what that analysis should be.

But we can't point to the problems raised by heterogeneities to explain all of our uncertainties about how to analyze patterns of use. In particular, the uses of *window*, *game*, *book*, *gossip* and the like that we talked about at the beginning of this section remain perplexing. There are no great discrepancies from speaker to speaker in their presuppositions about what windows and books are (at least, none that we can't easily ignore). And while there may be more disagreement over what a game is, say, this is not relevant to the uses that concern us. We could assume agreement that a game is on the one hand an activity that is governed by certain rules (construing 'rule' loosely enough so as to cover all of the examples that troubled Wittgenstein, and ignoring such features as competition and recreational purpose), and on the other hand, that a game is the set of rules (again loosely) that govern such activities, still without being able to say which of these things the word *game* names. Nor are any of these uses at all conventionalized, as the syntactic and ostensive tests show. The fact is that these cases are indeterminable in principle; that even if we could state for certain a theory of each of the referents of these words that was absolutely uniform from speaker to speaker, we would still be unable to say which use of the words was conventional.

The reason for this is that in each of these cases, the cue-validity of the best function from one referent to the other is exactly the same as its inverse: we would best define *a* in terms of *b*, and *b* in terms of *a*. Thus books ('inscriptions') are criterially used for the purpose of representing certain contents, but books ('contents') are criterially intended for inscription in a certain way. (Whether or not they are *actually* so used; a blank book may never be written in, and a book-length text may never be published.) This same indeterminacy pervades all words that name kinds of linguistic expressions. A sentence ('form') criterially expresses some content.[20] And a sentence ('content') must be expressible in some sequence of sounds or symbols. We are no better off identifying the content as 'the thing that is expressed by sentences' than identifying the form as 'the thing that expresses sentences.' Note that we fare no better if we say that *sentence* names an ordered pair of contents and forms. In the first place, then we must say that its use to refer to either alone is

derived. And in the second place, we could just as easily define the pair as the value of a function from either of its members.

Similarly for our other examples. What defines game-activities, among other things, is that they are governed by a set of rules (whether codifiable or not; we require *some* way of distinguishing games from mere play.) And what defines those rules is just that they ordain those activities. Again, the probability of successful identification is the same whichever way we go. And as with *game*, so with a whole class of rule-governed activities (or activity-governing rules), such as what we refer to with *language*. Or take *captain*. If it names a rank, then its use to refer to a kind of person would involve the best function to that kind; but if it names a kind of person, then its use to refer to the rank would be equally well-motivated. And as with *captain*, so with many other rank and role names, such as *bachelor*. If *gossip* names a kind of information, then its use to refer to the activity of conveying that kind of information follows rationally; if it names a kind of speech-act, then its use to refer to the characteristic content expressed in that act also follows. (The existence of the verb *gossip* adds another candidate, but we are sticking to nouns here.)

We could add many other examples of this sort, where an indeterminacy affects a certain portion of the lexicon. But so long as the indeterminacies involved are local in this way, the force of our argument as involving a thesis about language in general must be somewhat dissipated. It is important to note, therefore, that there are other indeterminacies that appear whenever we consider the uses of any general term. The most salient is the indeterminacy that leads to two interpretations for a sentence like *There are three trees on that island*, where *tree* may be construed as referring either to a tree-type or a tree-token. Closely allied to this is the indeterminacy that comes up with words that can be used to refer either to a mass or to members of the set of its instances, as with *fire/a fire*, *force/a force*, *foreign policy/a foreign policy*, and so on. In either case, the pattern of use would be entirely rational no matter what the words conventionally designated: we could derive 'type' interpretations if words named classes of tokens, and vice-versa. And we might note that this indeterminacy affects not only all common nouns, but verbs and adjectives as well, since the latter can be used to 'refer' either to first- or higher-order properties, states, and activities. Note that here as well, no purpose is served in saying that the words designate pairs (or triples) consisting of types (and sets of types) and sets of tokens, in which case we would say that both of the interpretations of *There are three trees on that island* would be pragmatically

derived. As with words like *sentence*, we could just as easily identify such pairs by conventional reference to any of their members.

We conclude then that there are virtually no words (except a few proper names)[21] for which we can 'give the meanings'; while we can be assured that only one of the uses of the word can be conventional, we have no empirical grounds for saying *which* use it is, since exactly the same pattern of use would be generated under any of several analyses. We can anticipate several lines of objection to this thesis. First, it could be argued that speakers decide among alternative analyses on grounds independent of use, say because one use is 'cognitively prior' to another. In some cases, we could make developmental sense of this claim; it may be that token reference is present at an earlier stage of language acquistion, for example. But it is hard to see how this could bear on the synchronic analysis, especially since we have already seen that re-analysis does accompany development, as when *momma* is made a general term. And even if cognitive development were actually relevant to the analyses that real speakers make in other cases – but what sort of evidence would show this? – it would not affect our thesis about what ideal speakers would do, any more than it affects similar theses in syntax. If might be possible to interpret the notion of 'cognitive priority' non-developmentally, of course, on the order of 'basic-level concepts,' or some such. But it's hard to imagine what would count as evidence that knowledge of types was 'prior' to knowledge of tokens, or vice-versa. And it makes no sense at all to talk of the cognitive priority of game-activities over game-rules; differentiation of either requires differentiation of the other. Then what notion of priority is it that is relevant here, and why should it affect the analysis of use?

It is still possible to make a weaker claim, saying that speakers choose among equivalent analyses for no good reason; that they throw dice, so to speak, in order to be able to come up with a single meaning for every word. Thus, suppose that Speaker A analyzes *captain* as designating a rank, but recognizes that there is another equally plausible 'dialect' – whose speakers are undistinguishable – in which it is analyzed as designating a class of persons. He has no way of knowing which of his utterances are interpreted 'literally' and which are assigned a derived sense, nor do others have any way of knowing how he intends them. Nor is he necessarily aware of having made the analysis he has; the important thing is that at some level, he has only one use in his 'grammar.'

There's no disproving so weak a claim as this, and one could take it simply as an article of faith: no matter what speakers do, or act as if they believe, or report themselves as believing, their beliefs about the

meaning of a word must really have such-and-such a form. But still, there is more to be said, because while we can't falsify the position, we can show how it looks quite implausible once we begin to ask about the relation between the idealization that we have adopted for argument's sake, and the reality that we invoke that idealization to explain. The problem is this: the idealized view of 'competence' that has been generally adopted to explain the speaker's knowledge of phonological and syntactic rules comes all to pieces when we consider the speaker's knowledge of word-meanings. Faced with an analogous situation in phonology or syntax, where two rules or sets of rules might grind out the same strings with equal elegance and economy, and where both were entirely consistent with whatever assumptions we made about the intrinsic capabilities of the language learner, it would not be too disturbing to say that the individual speaker may choose either of them for incorporation into his grammar. The differences between regularities in form and regularities in use appear when we look at the role of performance factors in determining the forms of rules.

We noted earlier that the speaker's theory about the meaning of a word has to be a theory about what would be the most rational practice given the background assumptions that are presumed to hold in the contexts he has heard the word used in. That is, it is a theory about other people's theories about other people's theories and so on. (We can short-circuit this by saying that it is a theory about the collective theory about the use of the word.) And we also noted that this theory could be equated with a purely 'private' theory – a theory about what would be rational given only the speaker's private beliefs about the world – only in that community in which everyone had the same beliefs, and knew that everyone had the same beliefs, and so on again – a community in which common knowledge was the only kind of knowledge there was. In such a community, a speaker would not have to consult the beliefs of other speakers in constructing his theory of the meaning of a word, and could choose arbitrarily among empirically indistinguishable alternative theories without worrying about what others would do.

But we also noted that this idealization was so severe as to be almost unimaginable. Where there is *any* discrepancy in beliefs among members of a community, a speaker is no longer free to ignore the beliefs of others in making his decision about which of the uses of a word is conventional. (For example, *game* would not be inderminate in meaning if not all of the activities we called 'games' were rule-governed even in a weak sense. But I cannot rationally make a private decision to call things like sneezing or stammering 'games,' whatever I may believe. So if *game* is

indeterminate for *them*, it must be indeterminate for me as well.) It follows that in any real community, when we say that the meaning of a word is indeterminate, we mean that it is collectively indeterminate; that *they* could not decide. In which case we *can't* choose arbitrarily among equally plausible hypotheses, since we have no choice in the matter at all. We are constrained to represent only those theories of the language that we believe others have represented.

This last may put us in mind of what Saussure and Meillet said about *langue*, in a Durkheimian turn: that it is a system of constraints imposed on the individual, in which he has no choice at all. At the very least it is a point at which the idealizations to *langue* and competence part ways abruptly. And to be sure, what we have said about theories of meaning holds as well for theories of form. An individual speaker's grammar should strictly be characterized as a theory of the collective theory of the phonology and syntax of his language, as well. But it is only when we come to meaning that the discrepancy between the different idealizations has any empirical importance.

The collective presuppositions that we take into account in constructing our theories of meaning are analogues of the performance factors that we consult in constructing our syntax and phonology. We decide that English does not require a rule prohibiting multiple self-embedding, for example, in part because of assumptions we make about the memorial characteristics of its speakers. If we should find a community of machines whose short-term memories were much better than our own, but who spoke English exactly as we do, we would have no choice but to assume that their grammars *did* contain counters, at whatever cost to simplicity. By the same token, we construct our phonologies against a set of implicit assumptions about phonetics; a community of speakers whose mouths were quite different from our own might produce exactly the same phonetic output from a very different set of phonological rules, since what was 'marked' for us would not be 'marked' for them.

If there were systematic and significant differences between speakers in mouths or memories, and if these differences were accessible to us, then we should have to take them into account in constructing the phonological and syntactic components of our grammars. In practice, however, such differences tend to be relatively slight and hard to observe; for the most part, we can assume that other speakers are constrained by performance factors pretty much as we are.[22] If a sentence is hard for me to process or pronounce, I will assume it is hard for you as well, and I can put together my phonology and syntax solely

in the light of my own abilities and limitations, secure that you will make the same assumptions of performance homogeneity, and proceed as I have. In syntax and phonology, then, there is not much difference between the answers to the questions 'How can I most efficiently go about generating these strings?' and 'How would others go about most efficiently generating these strings?' It is only in semantics that the idealization to homogeneous performance factors becomes intolerably unrealistic.[23]

Let me close the body of this paper by returning to the comparison of this thesis with one of Quine's – this time, the 'inscrutability of reference.' We have been assuming throughout that reference is scrutable, and that we can know for certain, say, that a native intends to refer with his utterance of 'gavagai' to a rabbit, and not a rabbit-stage or collection of rabbit-parts. (And that on another occasion, say when he says, 'Gavagai common here,' that he intends reference to a rabbit-type, and not to a rabbit-part-collection-type.) Yet even granting that reference is scrutable, we have seen that meaning is not determinate; we may know what somebody is referring to without knowing the linguistic conventions governing the uses of terms to refer. And it will not help at all if we can gain certain access to the other's conceptual scheme, for while that may assure us as to what he means, it will not tell us by what means he means it – on that point, he is in no less a quandary than we are.[24]

In the end, my point is simply this. Linguists have postulated that words have 'meanings-in-the-language' because it seemed necessary to do so, in order to explain how novel utterances are understood. It turns out, however, that this hypothesis is too strong, and that it would force us to make assumptions about our language over and above what simple understanding would require. We do not have to know what a word names to be able to say what it is being used to refer to. (Sometimes, we do not even have to know what sort of thing it refers to in order to be able to say what the sentence that contains it is being used to do; see note 24.) I do not think that this is exactly what Wittgenstein had in mind when he cautioned that we should look for the use, rather than the meaning, but his apothegm is apt. All that linguistic semantics should be asked to provide is an account of how languages come to be used and understood, and we can do that, it turns out, without having to say that speakers know what words mean.

APPENDIX

The apparatus that we have constructed to explain extended word-use will have some interest even to the reader who finds our conclusions unsavory, and I would very briefly mention some of the problems it can be brought to bear on. For example, problems of 'use/mention' appear in a new light when we observe that the 'mention' uses of words are no different from other extended uses we have talked about. Thus, I may hold up a pen to a phonetics class and say, 'What kind of sound does this begin with?' (or even, 'What sound does this begin with in German?'). And I can utter a sentence like:

(30) Beer, which is a liquid, begins with a stop; Lexington, which is a stop, begins with a liquid.

(Though I have no idea how such a sentence should be punctuated, I confess.) And this should make us wary of supposing that the distinction is in any wise semantic. (Similar arguments are available for opacity-in-general, but would require some space to develop.) By the same token, we can assimilate to this treatment of polysemy the uses of words to refer to representations of their designata, as when we say, 'He painted a unicorn.' Note that we could point at someone and say, 'He has blue eyes in John's portrait,' and that we can delete across the different uses in a sentence like:

(31) John painted a number of unicorns, which are mythical beasts.

Again, the distinction should not be represented in the semantics.

This treatment of polysemy also opens the way to a univocal treatment of the copula (in its main-verb uses, at least.) For we can now treat the '*be* of predication' as involving a certain kind of referring function from the designatum of the subject term. Thus a sentence like *John is six-two* can be understood as equivalent to something like 'John's height is six-two,' where the *be* is the *be* of identity. The function involved (from objects to their dimensions, in this case) is no different from that in 'He can punt half a football field, and jump the Eiffel Tower.' This analysis is not without its problems, but it would be nice to be able to do for *be* what Grice did for the binary operators like *and* and *or*.

Taking a quite different tack, we should observe that it is not only words that are systematically polysemous: multiple use is equally a problem with inflectional and derivational morphology, whether we

consider the uses of the English progressive, or the Latin ablative, or the Italian pronoun *si*. (And quite analogously, a construction like raising-to-object may be used to indicate a variety of relations between a verb and complement.) Linguists have discussed such patterns of use in far greater detail than they have lexical polysemy, but always from one of two points of view: either they have tried to show how all the uses instanced a single 'vague' meaning, or they have assigned to a particular use the status of '*Hauptbedeutung*' or 'prototypical use,' and have tried to show how all other uses follow from it. This account of lexical polysemy provides a third possible analysis, however, since we could equally well assume that all of the several uses of a form are connected by a network of referring functions, without having either to say that they instance the 'same meaning,' or that any of them is prior. In the absence of detailed analyses, however, I have no suggestions to offer as to where this latter sort of anylysis might prove useful. Certainly inflectional and derivational morphology presents special methodological problems, since we cannot begin the analysis, as we could with words like *chicken* and *newspaper*, with commonsense talk about the normal referents of the terms.[25]

Finally – though by no means exhaustively – this treatment of polysemy leads almost trivially to an account of metaphorical word-uses, and it would suffice if it did nothing more. What is wrong with all discussions of metaphor that I know about, as I mentioned above, is that they draw the line in the wrong place, between deviant, stylistically marked word-uses and normal word-uses, and proceed to assume that only the former are pragmatically generated. In consequence, they have had to rely on *ad hoc* hypotheses about how words could be used metaphorically ('principles of comparison' and the like.)

We have seen, however, that pragmatic schemata generate normal word-uses as well, and how these schemata follow from the definition of reference itself. What distinguishes normal and metaphorical word-uses, then (using 'metaphorical' to refer only to the marked cases that have ordinarily been treated as metaphors), is not that some are 'literal' or conventional, but rather that the two classes of uses are licensed by different sorts of beliefs. A true metaphor, we would say, is an (interpretable) word-use that is not licensed by the assumptions that constitute the conversational background, ***assuming that the speaker is interested solely in maximally efficient identification of the referent***; with metaphor, the purpose of exchange of information is subordinated to other, affective goals.[26] Metaphor can thus be assimilated to particularized conversational implicature, as Grice suggests, or even better,

to indirect speech acts, where the parallel of 'reference' and 'assertion' is compelling.

A satisfying account of metaphor, however, requires that we be able to talk about motives as well as means, and that would take us far afield. That is, we have to be able to show *why* a speaker may choose to refer inefficiently, and what he may communicate in doing so. (Here again the problem is not particular to metaphor; Grice's account of irony is inadequate, for example, because he supposes that it can be explained entirely in terms of the *rational* assumptions that speakers make.) Without going into detail, I would suggest that the appropriate line to pursue is this: we construe metaphorical word-uses by making a set of assumptions about how the world would have to be for the use to be entirely rational and efficient, much as we construe ironical utterances by reference to the world in which the utterance might be intended sincerely. So while it is true that metaphors gain their affective import by giving us 'two ideas for one,' as Dr. Johnson suggested, the two ideas are not meanings, but sets of presuppositions: the ones that actually hold, and the ones that the speaker pretends to observe.

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NOTES

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[1] See, eg., Quine 1960, 1969.

[2] Thus, whether we think of conventions as specifying what behavior is to be conformed to in certain 'recurrent situations' (see Lewis 1969), or, as is more appropriate to linguistic convention, as specifying how we shall go about referring to certain kinds of things, we cannot expect that they shall cover every possible kind of situation or thing with which we may be confronted. This is not to say that we cannot *describe* everything, but rather that everything can't have a *conventional* description. For the present purposes, however, it is sufficient to observe that we *don't* have names for every kind of thing, leaving necessity aside.

[3] Note, for example, that we are not tempted to reject the 'asymmetric' uses of conjunctions like *and,* nor do we have any intuitive sense about whether or not they are conventional, as the history of the literature on the subject makes all too clear.

[4] Many speakers may perceive a synchronic relation between such words as *shoot* (a gun) and *shoot* (the rapids), or *ear* (on the head) and *ear* (of corn), though there is no etymological connection. The historical criteria that have sometimes been used to define 'polysemy' have served lexicography well, but they are obviously strictly irrelevant to synchronic description. Curiously, philosophers like Quine (1960), Ziff (1960), and Katz(1972) have sometimes invoked them as well.

[5] Or, following the line developed by Lewis (1969), that a given coordination problem ('how to refer to such-and-such'), should have two co-ordination equilibria, given the state of the participants' knowledge. Note also that the possibility that there should have been another, *conventional* way or resolving the problem does not mean that the way we have chosen is conventional. (So our use of *chicken* to refer to chicken meat is not conventional simply because we *could* have called it 'pullet.') If this were so, then all behavior in situations, where we *could* have a convention for acting must be conventional.

[6] Nor will we be concerned with the kinds of trick cases that philosophers of language are apt to worry over, such as whether the identifying description must be independent of the inferences that follow from the mere recognition of the speaker's intention to refer to something. (Thus, we might be unsure as to whether a speaker has successfully referred to

John Jones with an utterance of 'John Jones is a friend of mine', if the hearer can identify Jones only as 'the person that the speaker is talking about,') But we will not discuss any examples like these, and their status will not affect our our argument.

[7] I do not intend that the schemata I present shall have any too-literal psychological interpretation, as I have stressed. It may be, however, that they can be implemented procedurally. See Kaplan (1977) for some ingenious suggestions in this line; his proposals are based on a version of this treatment that eventually appeared in Nunberg (1978).

[8] Speakers may disagree as to the degree to which these examples are 'good.' It doesn't matter so much; see below.

[9] Clark and Clark (forthcoming) use the notion of cue-validity in a remarkably similar way, to explaining the principles by which we are enabled to interpret novel de-nominal verbs ('He porched the newspaper,' etc.)

[10] I say 'at least' two functions because we may also want a function to take us from Mick Jagger to the Rolling Stones. It depends upon whether the concert was best identified as 'the event at which *he* will appear' or 'the event at which *they* will appear.' Note that if the picture represented one member of a group that dressed alike – say Kiss or Devo – we would require the latter function. Note also that under slightly different circumstances, the first-computed member of the composite could be a function that takes photographs into photographers – I could point at the photo and say 'Are you going to that?' to refer to an Avedon opening, provided it was either evident or common knowledge that the photo was by Avedon.

[11] 'Attributive' and 'referential' ostension exactly parallel the different uses of descriptions. For example, suppose I point at a car that is parked at a hydrant and say 'He will get a ticket,' where I intend to refer simply to 'whoever put that car there', or suppose I start at a loud noise and say 'That must have been a backfire.' intending to refer simply to 'whatever that noise was.' (Reference is deferred in the first case, and direct in the second.) In neither case can I substitute any other 'description,' nor could I succeed in referring if the 'description' I have given is 'false.' The quotes are necessary because these examples show to what degree all of the received views of this distinction are inadequate. In particular – though I won't pursue the matter here – they show the hopelessness of treating the distinction in the semantics of the language, and point up a serious difficulty in Kaplan's (1978) suggestion that the referential uses of descriptions can be assimilated to the uses of demonstrative terms.

[12] The problems of reckoning the degree to which performance factors will affect acceptability are much more intractable in semantics than in syntax, precisely because the constraints on performance are imposed, not by the fixed limits of the phonetic and perceptual faculties (wich can be calculated to some degree of accuracy), but by the willingness of free agents to abide by social rules. Thus, it is easier to determine when a sentence is too complicated to process than when a sentence is too long to be interesting. And of course the conversational maxims may be violated in the service of, say humor, while perceptual limitations are not subject to will at all. (Speakers have said of some examples I have given that they sound like jokes.)

[13] Though not in all circumstances. I may point at a writer and say either 'He is hard to read,' or 'That is hard to read.' But I cannot point, say, at the inventor of dynamite and say, 'He is a curse and a blessing,' intending reference to the invention.

[14] For example, most speakers find it odder to point at the author of a recent novel and say 'It's a best-seller' than to point at a well-known cartoonist and say, 'They always make me chuckle.'

[15] Though passive *be* can't be used with demonstratives; cf. 'That was praised for a recent recital by the *Times*.

[16] There are other, still more mysterious differences between languages according to the way in which 'indexical character' is exploited. For example, French and English have 'identical' paradigms for locative demonstratives, yet the French will use *là* where we

might use *here*. (Thus, 'Is he staying here tonight?' = 'Il reste là ce soir?' *Voilà* is used analogously.)

[17] A third difference is this: there is nothing that can't be identified descriptively (which is not the same thing as saying that everything is effable), but in the average context, there are few things that can be identified ostensively. This is why, I think, we are more charitable about allowing referring functions with a middling cue-validity in ostension than description; why we can more easily get away with pointing at a book and saying 'He lived next door to me,' than with naming a book and saying '*Bleak House* was born in 1812.'

[18] Even if we know nothing of the author than that he wrote these works, we nonetheless make these assumptions about the causal relation between him and them, and so that the best way of identifying the works in principle would be by reference to their author. It might seem as if the possibility of utterances like 'Caedmon was two people' should lead us to assume that *Caedmon* names an oeuvre, since it here appears to be used to refer to 'whoever wrote those works.' In fact however, I would argue that this use of the term really involves a composite function, from the putative author to the works to the real author, and so should be analyzed along the lines of 'the author of the works ascribed to Caedmon.' (This line of inquiry has important consequences for the logical analysis of sentences involving negative existentials, which are vexing to all current accounts of meaning. But I won't pursue it here.) I should also note that there are particular problems involving names like *pseudo-Caedmon*, and that I am not sure just how such terms should be analyzed. All of this is beside the point of the argument here, in any case.

[19] This example is borrowed from Bolinger (1975).

[20] The existence of 'semi-sentences' that express aberrant contents is irrelevant; they are sentences *to the extent that* they express something, or could be used to express something.

[21] Personal names usually have only one possible analysis. But place names may often be analyzed as naming either a geographic region or a political entity. And names of literary works may be analyzed, like *book*, as naming either inscription- or content-types or classes.

[22] In phonology, of course, we do encounter situations in which a speaker or group of speakers may be systematically constrained by phonetic difficulties unlike our own – if they have a speech impediment, for example, or if they are toothless. But these are obviously abnormalities, in the view of both the affected and normal speakers, and so *should* be ignored by all in the construction of a phonological theory. If such heterogeneities were more widespread, of course, we should have to take them into account for general purposes, as the linguist interested in the spoken language of the speech-disabled or deaf does. But I trust that I will give no offense in saying that these problems are not important from the very abstract standpoint of phonological theory.

[23] One consequence of this observation (*pace* our reservations of the previous footnote): in syntax and phonology, virtually all differences in categorical linguistic behavior (as opposed to the frequencies with which alternating variants are used) can be laid to differences in linguistic rules. Thus, when we find an Englishman saying 'Have you the time?' Instead of 'Do you have the time?' of/haf/ instead of /hæf/, we are never tempted to explain the difference by appealing to differences in the performance factors that determine the linguistic behavior of Englishmen and Americans; rather we asssume differences in the rules of British and American English. Whereas with use we cannot be so sure. Bolinger (1975) cites the example of Northern British 'cook by gas,' and suggest that it is hard to tell whether the use of *by* instead of *with* may not be due to a difference in local theories about fuels. (Cf. 'heated by New York Steam, powered by electricity,' etc.)

[24] There are occasions, however, when we do not even have to be able to recover the references of terms in order to understand an utterance, and then we have to give Quine the right on inscrutability as well. Consider an utterance of a sentence like 'She's not that

sort of person.' There is no way to tell, I think, whether the utterance would involve asserting the identity of two types (in which case the term 'she' is being used to refer to 'her type') or of two tokens (in which case the phrase 'that sort of person' is used to refer to an instance of a type, as it would in 'I have that sort of dog at home'.) But even here. I would be reluctant to say that we have not understood 'what was said.' Rather, we would say that it is unneccessary in these cases to know how – by what compositional process – the sentence means what it does. It is in this way that we might extend the thesis about word-meaning to the 'meanings' of compositional processes, but I will leave the argument here.

[25] At the other end of the scale, we can say that non-linguistic practices are 'polysemous' as well, so long as they are 'meaningful' in the sense of Weber or the hermaneuticists. Thus handshakes can be used to signal agreement, greeting, valediction, congratulation, and so on, where there is no reason to suppose that these are 'homonymous' practices, or on the other hand, that they instance only one 'meaning.' In the end, we will not want to say that either 'reference' or 'meaning' are terms that have application only to natural-language use.

[26] Not only word-uses can be metaphorical. I won't make the case here, but we may say that the uses of other morphology and constructions may be metaphorical (see note 24), as well as the uses of non-linguistic signals, like handshakes (see note. 25). Thus consider a sentence like *John found the theorem wrong.*' As Borkin (1974) noted, '*to be* deletion' is customarily used with verbs like *find* only when sensory experience is involved. (Contrast *He found the chair comfortable, He found the theory unpalatable.*) Its use in the example, accordingly, suggests that John's judgement did not have sufficient logical basis, even if we might be chary of saying that John *literally* intuited that the theorem was incorrect. That is, there are circumstances in which we might best read the sentence as if '*to be* deletion' was metaphorically used.