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**The Theory of Meaning**

**by**

**Russell Eliot Dale**

**A dissertation submitted to the Graduate Faculty in Philosophy  
in partial fulfillment of the requirements for the degree of  
Doctor of Philosophy, The City University of New York**

**1996**

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This manuscript has been read and accepted for the Graduate Faculty in Philosophy in satisfaction of the dissertation requirement for the degree of Doctor of Philosophy.

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Abstract

The Theory of Meaning

by

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This dissertation is concerned with that notion of meaning which is most commonly understood to apply to linguistic expressions. "Snow is white", it can be said, is a linguistic expression which means among English speakers that snow is white. But for an expression to mean something among a group of people, certain facts about the psychological states of the people in the group must hold. Meaning, it is maintained, is primarily to be understood in terms of some regularity of communicative practices among the members of a group, and these communicative practices, in turn, are to be understood in terms of certain psychological states of the members of the group. When the notion of meaning is conceived of in this way, the task of providing a theory of meaning will be understood as the two-fold task of stating which communicative practices are relevant to meaning and then saying what sort of regularity of these practices will constitute meaning.

Chapter 1 is a general introduction by way of a discussion of H. P. Grice's important contribution to this subject.

Chapter 2 is a history of the theory of meaning in the twentieth century tracing important ideas associated with Grice to the works of Victoria Welby, Alan Gardiner, and others.

Chapter 3 contains a more detailed statement of the general conceptual framework of the discussion to follow.

In chapter 4 it is argued that no one has yet given a compelling case that the notion of a compositional-semantic theory is a necessary notion for the theory of meaning.

Chapter 5 is a critique of the "convention"-based theories of meaning of David Lewis and Brian Loar.

Chapter 6 is a critique of a recent "translation"-based theory of meaning due to Stephen Schiffer.

Chapter 7 contains discussion of proposals for avoiding problems found with earlier theories. Grice's notion of "speaker-meaning", the idea, due to Chomsky, that natural languages are infinite in magnitude, and the importance of the notion of "conventional" meaning are all challenged and alternative notions are argued for. To accept these alternatives is to accept a novel conception of the theory of meaning.

### Acknowledgments

There are many people to whom I feel grateful for their help in one form or another while writing this dissertation. Thanks first of all to Stephen Schiffer whose help, patience, and great philosophical sense were absolutely crucial to me. Thanks also to the other members of my dissertation committee, Hartry Field and David Rosenthal. Thanks to Stephen Stich and Richard Mendelsohn for being readers. I also thank Jerry Fodor whose encouragement has been extremely important to me. And thank you to the CUNY Graduate School for its support through a Dissertation Fellowship for 1993-1994. The discussion, encouragement, and friendship of Gary Ostertag have been invaluable to me and I warmly thank him for these. I am deeply thankful to Catherine Joos for her thoughts, support, patience, and friendship while I wrote. Thank you to my dear friend Rose Petrunich for all of her friendship and support without which I can't imagine having gotten through the past few years. And thanks to Ron Baron for his friendship and support as well. Thanks to Adam Vinueza for his friendship and the many discussions that we have had on philosophical topics through my years at CUNY. I am also deeply grateful to Amy Ruth Ullrich-Marano for her devoted friendship and support. I thank David Schauffler for our many and much-valued years of friendship and discussion. Thanks also to Tom Skwar for his friendship and support through my years at CUNY. Thanks to Ivan Bresgi who has been a great help to me for the past 8 years. I thank Hiram McLendon for the much that I have learned from him which has been indispensable to me in the writing and living of the past years. And thanks to my new friends and colleagues at MicroEdge for their encouragement and support while I made final preparations to defend my dissertation: Dov Torenberg, Ximena Florez, Evie Manieri, Lou Flees, J. P. Cheuvront, as well as the rest of the staff there. Thanks also for support and friendship to Joseph Cobitz, Robert Cowell, Elliot Feingold, Carrie Figdor, Paulette Giguere, Ching-Ching Lin, Maureen Linker, Rita Napolitano, Eleanor Williamson, and Patricia Younge.

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## Chapter 1 The Theory of Meaning: General Introduction

### 1.1 Introduction

What is it about some of the marks, sounds, and gestures that people produce and attend to that makes these marks, sounds, and gestures mean what they do among the people who produce and attend to them?

It is hard to believe that an essential property of the sound-type "lumi on valkoista" is the property of meaning among the Finns that snow is white. It seems just as hard to believe that the sound-type "lumi on valkoista" contingently has the property of meaning what it does among the Finns, but that it does not have that property contingently on its having some other property describable in physical and/or psychological terms. If that could be so, then two possible worlds could be identical in every physical and psychological respect, but differ in that in one world "lumi on valkoista" means among the Finns that snow is white, but in the other it means among the Finns that it's good to floss. It seems to me that if the sound-type "lumi on valkoista" has the property of meaning among the Finns that snow is white, it has this property in virtue of having some other property the description of which would require some sort of talk about how the Finns might use that sound-type to effect each other's psychological states. It seems, I think, inviting to suppose somewhat along the following lines: the sound-type "lumi on valkoista" means among the Finns that snow is white rather than that it's good to floss because it has the property of being such that for any two Finns, for example, Päivi and Martti, Päivi in producing a sound of that type in the presence of Martti will generally expect that Martti will come to believe that Päivi said that snow is



white, and not that she said that it's good to floss.<sup>1</sup>

I take this sort of description as pointing the way toward what a general account of the nature of linguistic meaning should be like. But there are lots of problems standing between this simple description and a general theory of linguistic meaning.

The present dissertation attempts to examine ways of trying to formulate a general account of meaning which are more-or-less in line with the simple kinds of thoughts about meaning I just glossed. The ways of attempting such a general formulation that I am interested in work within a certain general conception about the nature of meaning. On this conception *meaning* can be understood wholly in terms of propositional-attitudes notions<sup>2</sup>.

The conception that I have in mind is due in the first instance to H. P. Grice<sup>3</sup>, but it is importantly elaborated by Stephen Schiffer<sup>4</sup> and David Lewis<sup>5</sup>. Other philosophers as well, among them P. F. Strawson, Jerry Fodor, Brian Loar, and Jonathan Bennett, have all, at least at some time or another, taken the conception I allude to as at least a working assumption.<sup>6</sup>

The general strategy of this dissertation will be as follows. In this chapter I will introduce Grice's conception of a theory of meaning in a general and somewhat informal sort of way, discussing various aspects of Grice's seminal paper "Meaning". Chapter 2 contains a sketch of what I take to be important aspects of the history of theorizing about meaning in the twentieth century, a rather overlooked area today. In chapter 3 I will state in somewhat more detail one way of thinking - or talking, anyway - about a general theory of meaning that will be employed by theories of meaning that I will consider in

later chapters. Chapter 4 will contain a discussion of *compositional-semantic theories* and their relation to theories of meaning. I will argue, among other things, that it cannot be supposed that a language must enjoy a compositional-semantic theory in order for the language to be used by a group of people. But, most importantly, I will discuss an important counterexample to the view that language-processing must proceed via a compositional-semantic theory of some sort. In chapter 5 the issue will be theories of meaning that employ the notion of *convention*. I will begin with a discussion of David Lewis's various accounts of meaning. I will find a number of faults with these. First, I will argue that Lewis's theories presuppose that for a group of people to speak a natural language there must be a compositional-semantic theory for that language. Second, there will be problems for Lewis's theory that attend to his use of the notion of *convention*. Third, I will discuss a crucial problem, originally discussed by Stephen Schiffer, for Lewis's theories. After discussing Lewis's theories I will briefly discuss a theory due to Brian Loar. Loar's theory uses the notion of *convention*, but seems to avoid the crucial problem that Schiffer raised for Lewis's theories. But I will point out that in addition to inheriting all the problems associated with using the notion of *convention* and to requiring that usable languages enjoy compositional-semantic theories, Loar's theory has other important problems. In chapter 6 I turn to a recent theory of meaning offered by Schiffer which at first seems hopeful since it avoids commitment to compositional semantics from the outset. But I will present a serious counterexample to this theory. I will try to argue that the counterexample presents problems that are so serious for the type of theory that Schiffer has presented that there is really not much hope of trying to provide an adequate

theory much like Schiffer's. Finally in chapter 7 will present a number of suggestions concerning the path I think a theorist of meaning might best take, given the discussion in the preceding chapters.

In the present chapter, then, I will get things started with a discussion of Grice's classic paper "Meaning" in section 1.2, followed by a brief discussion of the general goals of a theory of meaning in section 1.3.

## 1.2 Grice's "Meaning"

There are four themes that I consider central to Grice's seminal 1957 paper "Meaning" and that I would like to focus on in this introductory discussion. Any general understanding of the Gricean conception of meaning will require some understanding of each of these four themes. But they are important to look at here since they will arise in what follows in important ways. Here I will give a brief inventory of these themes, and then I will discuss each in a section of its own.

(1) The leading theme in "Meaning" is the thought that the semantic properties of conventional expressions are to be understood in terms of propositional-attitude psychology. I will call the notion of *an expression meaning something* the notion of *expression-meaning*.

(2) A second theme is about the nature of the relations between notions of *meaning* and notions of propositional-attitude psychology: do semantic notions *reduce* to notions of propositional-attitude psychology, and if they do, can we come to know of this reduction through *philosophical analysis* as this has been traditionally conceived?

(3) Another theme concerns the need for and nature of a notion of *a speaker meaning something on a particular occasion by an utterance* to achieve the understanding, mentioned above, of expression-meaning in terms of notions of propositional-attitude psychology. I will call the notion of *a speaker meaning something on a particular occasion by an utterance*, the notion of *speaker-meaning*. The idea behind this theme, then, is that the notion of expression-meaning can be analyzed neatly in terms of a notion of speaker-meaning (along with other notions that need not be worried about right now), and that the notion of speaker-meaning can be neatly analyzed wholly in terms of notions of propositional-attitude psychology. The overall result will be an analysis of expression-meaning wholly in terms of notions of propositional-attitude psychology.

(4) And the fourth theme that I am interested in is that of the possibility that notions of *meaning* that are specifically associated with language and, more generally, communication - like the notions of *expression-meaning* and *speaker-meaning* just alluded to - are interestingly related to notions of meaning that do not have to do with language or communication at all: Grice called one such non-linguistic, non-communication-related notion of meaning *natural meaning*. This theme ties into interesting questions that have been raised in objection to the Gricean program which I will also mention.

I will now discuss in turn and in a little more detail some aspects of each of these themes and their appearance in Grice's "Meaning" and, in some cases, in others of his works.

### 1.2.1 The Reduction of Expression-Meaning

The most important idea in "Meaning" is, roughly stated, that the semantic properties of linguistic expressions<sup>7</sup> can be understood in terms of propositional-attitude notions. I would like to discuss two matters with respect to this idea. The first is the generality with which Grice held the idea that semantic notions are to be understood psychologically: he seems to suppose that *all* the important semantic properties that linguistic expressions have are to be understood in psychological terms. The second is that Grice gave very little by way of the *details* of the story of how the psychological reduction of semantic properties of expressions is supposed to go; his sketchy remarks in "Meaning" really amount to little more than the suggestion of a program.

1. The specific way that Grice thought that the understanding of semantic notions<sup>8</sup> could be accomplished was by way of a notion of *speaker-meaning* (which I will discuss separately in section 2.3 below). He thought that semantic notions could be analyzed in terms of the notion of *speaker-meaning* (and other notions unimportant to us just now) and that the notion of *speaker-meaning* could in turn be analyzed in terms of propositional-attitude notions. The over-all effect would be the analysis of semantic notions in terms of propositional-attitude notions.

Thus, toward the beginning of the paper he criticizes a certain theory about meaning - that given by C. L. Stevenson in his book *Ethics and Language*<sup>9</sup> - on the grounds that it

...ignores the fact that the meaning (in general) of a sign needs to be explained in terms of what users of the sign do (or should) mean by it on particular occasions; and so the latter notion...is in fact the fundamental one.<sup>10</sup>

Here he shows his commitment to the view that expression-meaning, can be understood in terms of speaker-meaning. When we couple with this his view that speaker-meaning is to be understood in terms of propositional-attitude notions, we have the view that expression-meaning is to be understood in terms of propositional-attitude notions.

But the idea being discussed now is that *all* semantic notions that apply to linguistic expressions can be understood in terms of propositional-attitude notions, not just the notion of expression-meaning. Grice suggests this more general claim in the following passage:

If we can elucidate the meaning of

"x meant<sub>NN</sub> something (on a particular occasion)" and  
 "x meant<sub>NN</sub> that so-and-so (on a particular occasion)"

and of

"A meant<sub>NN</sub> something by x (on a particular occasion)" and  
 "A meant<sub>NN</sub> by x that so-and-so (on a particular occasion),"

this might reasonably be expected to help us with

"x means<sub>NN</sub> (timeless) something (that so-and-so)," and  
 "A means<sub>NN</sub> (timeless) by x something (that so-and-so),"

and with the explication of "means the same as," "understands," "entails," and so on.<sup>11</sup>

Grice seems, then, to hold that the notions of *synonymy* and *entailment*, notions that philosophers often seem to take to be the ken of logic and not of psychology, are really founded in the notion of speaker-meaning, and therefore, by way of analysis, in the notions of propositional-attitude psychology. But the important point now is that the "and so on" in the above passage indicates that Grice took *all* semantic notions to be

understood as psychological notions.

2. When Grice arrives at an analysis of speaker-meaning in terms of propositional-attitude notions, he only vaguely suggests how the notion of the meaning of a sentence, what I have called *expression-meaning* but Grice refers to as the "*meaning<sub>NN</sub> (timeless)*" of a sentence, might itself be analyzed in terms of speaker-meaning:

"x" means<sub>NN</sub> (timeless) that so-and-so" might as a first shot be equated with some statement or disjunction of statements about what "people" (vague) intend (with qualifications about "recognition") to effect by x.<sup>12</sup>

This is rather obscurely stated. I take it that the "qualifications about 'recognition'" are simply the details of his analysis of speaker-meaning. So we can, I think, safely restate as follows Grice's suggestion about the analysis of the notion of expression-meaning:

"x" means<sub>NN</sub> (timeless) that so-and-so" might as a first shot be equated with some statement or disjunction of statements about what "people" (vague) mean<sub>NN</sub> by x.<sup>13</sup>

But Grice does not discuss in Grice (1957) any details of the sort of "statement" that he has in mind in this suggestion. And he doesn't mention at all how speaker-meaning can help with other semantic notions like synonymy, understanding, and entailment which, as I pointed out above, he says it can be expected to help with. So what we have in Grice (1957) is but the barest outline of a conception of a general account of meaning, a suggestion for a program. In later chapters I will be scrutinizing various ways of trying to give the details of how expression-meaning can be understood in terms of propositional-attitude notions.

### 1.2.2 Reduction?

Above I talked about the *reduction* of expression-meaning to propositional-attitude notions. In her recent book, *Meaning and Mind*, Anita Avramides argues that it is perhaps best not to understand the analyses of the Gricean program as *reductive analyses* as followers of Grice such as Schiffer and Loar have.<sup>14</sup> She suggests that the Gricean conception of meaning would still be interesting if the analyses offered in it were understood as what she calls *reciprocal analyses*. In a reciprocal analysis the notions on the right side of the analytic biconditional are not to be taken, as in a reductive analysis, as somehow either *clearer than* or, in some other way, *more basic than* other notions in the "circle" that includes the analyzed notion on the left side of the biconditional:

The concepts mentioned on either side of the analytic biconditional have to be thought of as on a par.... Neither set of concepts would be more basic than the other.<sup>15</sup>

A reciprocal analysis remains interesting by showing "that understanding [the analyzed concept] is to be gained only by discerning its place in a system of interrelated concepts."<sup>16</sup> Thus, circularity - so long as it is not too blatant - in a reciprocal analysis is not necessarily cause for alarm:

Circularity, then, is a problem for the analysis of meaning only if our aim was to exhibit logical priority among concepts. Discovery of a circularity need not force us to abandon the project of analysis; we need only modify our claims.

... The problem of circularity plagues only *reductive analyses*. [footnote: "Reciprocal analyses are not open to the charge of circularity; or, one might say, circularity in a reciprocal analysis is never vicious. Of course, even reciprocal analyses must beware of traveling in circles that are *too small*."] One cannot have succeeded in breaking up a concept into simpler or more basic components if those components require the original concept for their explication. If beliefs *are* attitudes towards sentences, the goal of achieving a new level of concepts must be abandoned, but this



need not compel us to abandon analysis altogether. Rather, we must accept that our analysis is of another kind, namely, reciprocal. What the analysis shows is how in precise detail our psychological and semantic concepts fit together. Here the analysis is like "a closed curve in space."<sup>17</sup>

Avramides also claims that "it is not altogether clear which interpretation [i.e. reductive or reciprocal] Grice himself intended of his work."<sup>18</sup> She quotes part of a passage from a later work of Grice to suggest that at least later on in his thinking he was not friendly to the reductive interpretation of his work.<sup>19</sup>

I will now briefly assess these views of Avramides concerning Grice's understanding of his own work.

Grice nowhere in "Meaning" uses the term 'reduction' (or some cognate). Furthermore, Grice nowhere in "Meaning" uses the connectives "if and only if" or "just in case" or some equivalent. Indeed, he doesn't seem to be consistent with respect the sort of glosses he is trying to supply in the article. Here is how he states a proposal for understanding the notion of an utterance meaning something on a particular occasion:

..."x meant<sub>NN</sub> something" would be true if x was intended by its utterer to induce a belief in some "audience" and...to say what the belief was would be to say what x meant<sub>NN</sub>.<sup>20</sup>

This, if true, states a *sufficient* condition for an expression to mean something on a particular occasion of utterance. After considering a counter-example to this proposal Grice seems, in the very same paragraph, to switch to concern with necessary conditions for an expression to mean something on a particular occasion of utterance:

...for x to have meant<sub>NN</sub> anything, not merely must it have been "uttered" with the intention of inducing a certain belief but also the utterer must have intended an "audience" to recognize the intention behind the utterance.<sup>21</sup>

Again, later Grice seems concerned with necessary conditions:

Perhaps we may sum up what is necessary for *A* to mean something by *x* as follows. ...<sup>22</sup>

But consider the connectives used when Grice summarizes his thoughts in the paper:

"*A* meant<sub>NN</sub> something by *x*" is (roughly) equivalent to....

"*x* meant<sub>NN</sub> something" is (roughly) equivalent to....

"*x* means<sub>NN</sub> (timeless) that so-and-so might as a first shot be equated with...."<sup>23</sup>

Why is there this apparent inconsistency in Grice's discussion? Is he simply speaking loosely?

Answering these questions, I think, is complicated by reading Grice and Strawson (1956), "In Defense of a Dogma". This paper, famously, tries to defend against Quine's attacks something like the legitimacy of the family of expressions that include "analytic", "self-contradictory", "necessary", "synonymous", and "semantic rule".<sup>24</sup> In this paper, Grice and Strawson seem to be very clear about what form the statement of a reductive analysis should take. In talking about what Quine would allow as making satisfactory sense of one of these notions<sup>25</sup>, they say:

To make "satisfactory sense" of one of these expressions would seem to involve two things. (1) It would seem to involve providing an explanation which does not incorporate any expression belonging to the family-circle. (2) It would seem that the explanation provided must be of the same general character as those rejected [in Quine (1951)] explanations which do incorporate members of the family-circle (i.e., it must specify some feature common and peculiar to all cases to which, for example, the word "analytic" is to be applied; it must have the same general form as an explanation beginning, "a statement is analytic if and only if ...").<sup>26</sup>

A paragraph later, we are told the following:

It would seem fairly clearly unreasonable to insist *in general* that the availability of a satisfactory explanation in the sense sketched above is a necessary condition of an expression's making sense. It is perhaps dubious whether *any* such explanations can *ever* be given. (The hope that they can be is, or was, the hope of reductive analysis in general.)<sup>27</sup>

It would seem that if Grice in 1956 is this clear about the form that a reductive analysis has, then in 1957, if he intended such a thing, he could have been a lot clearer. Furthermore, the passage just quoted suggests that Grice in 1956 took the project of providing reductive analyses of notions to be a dubious one. So it may be tempting to suppose that Grice in "Meaning" didn't want to provide reductive analyses of the semantic notions he discusses: whatever he was up to, it wasn't the attempt to provide reductive analyses.

So, so far it looks like Avramides might be correct in saying that it is not clear what Grice's original sense of his own work was. But consider now the passage quoted by Avramides to suggest that Grice in his later work was perhaps against the reductive understanding of his program:

As I thread my way unsteadily along the tortuous mountain path which is supposed to lead, in the long distance, to the City of Eternal Truth, I find myself beset by a multitude of demons and perilous places, bearing names like Extensionalism, Nominalism, Positivism, Naturalism, Mechanism, Phenomenalism, Reductionism, Physicalism, Materialism, Empiricism, Scepticism, and Functional-ism.... After a more tolerant (permissive) middle age, I have come to entertain strong opposition to all of them, perhaps partly as a result of the strong connection between a number of them and the philosophical technologies which used to appeal to me a good deal more than they do now.<sup>28</sup>

Though this passage does suggest that he, when writing it, was against some sort of reductionism, it also strongly suggests that earlier in his life he supported it. Indeed, in his posthumous "Retrospective Epilogue" dated a year after the publication of the above

passage, Grice answers in the following way J. Jack who Grice reports as having "reproved" him for attempting reductive analyses of semantic notions:

But what kind of analysis is to be provided? What I think we cannot agree to allow her [J. Jack] to do is to pursue the goal of giving a *lax* reductive analysis of meaning, that is, a reductive analysis which is unhampered by the constraints which characteristically attach to reductive analysis, like the avoidance of circularity.... ((In this connection I should perhaps observe that though my earlier endeavors in the theory of meaning were attempts to provide a reductive analysis, I have never (I think) espoused *reductionism*, which to my mind involves the idea that semantic concepts are unsatisfactory or even unintelligible, unless they can be provided with interpretations in terms of some predetermined, privileged, and favored array of concepts; in this sense of "reductionism" a felt ad hoc need for reductive analysis does not have to rest on a reductionist foundation. Reductive analysis might be called for to get away from unclarity not to get to some predesignated clarifiers.))<sup>29</sup>

This passage explains the senses in which Grice saw himself for and against the *reduction* of semantic notions. Clearly the sense in which he is for reduction shows that he would not be satisfied with an Avramidesean *reciprocal analysis*. Circles are not to be tolerated.

The concern to avoid circularity in the analysis of meaning is found in Grice (1957) when Grice twice remarks on circularities in Stevenson's theory.<sup>30</sup> Thus, Grice in "Meaning" can be understood as being concerned with finding a reductive analysis of semantic notions. He nowhere states or suggests that his motive for trying to find analyses of semantic notions is somehow to legitimate notions that might otherwise be suspect. So, we can take his word for it when he says that he has never supported *reductionism* in the sense of the above quotation. But this will not interfere with our supposing that Grice took himself to be trying to provide reductive analyses of semantic notions.

Of course, perhaps it still puzzling why, if reductive analysis was what Grice was

after in 1957, he shouldn't have more clearly stated so given the clarity of Grice and Strawson (1956). A partial answer to this puzzle might be found in the fact that, actually, "Meaning" was not written after Grice and Strawson (1956), but long before it for a seminar he and Strawson gave in 1948.<sup>31</sup> Grice, it might be supposed, was not that interested in methodological subtlety in his seminar presentation and didn't dwell on being clearer on the points I am discussing in this section.

So far I have only argued for the extremely modest point that Grice, anyway, took himself to be trying to find reductive analyses for semantic notions. The question still remains whether Avramides is correct in thinking that the search for such analyses is misguided somehow. I cannot argue the point here, but I will merely assert that I do not think that Avramides is correct and that it is perfectly reasonable to seek reductive analyses of at least some important notions, *meaning* among these.

### 1.2.3 Speaker-Meaning

The attempt to explicate a notion speaker-meaning plays a central role in Grice's "Meaning". Grice's leading idea was, again, that expression-meaning should be explicated in terms of some notion of speaker-meaning and that speaker-meaning should be explicated in terms of notions of propositional-attitude psychology. In this section I will talk first a bit about Grice's analysis of speaker-meaning and then about the status of this notion within the theory of meaning as a whole.

Roughly<sup>32</sup>, Grice's analysis of speaker meaning has it that a speaker *S* *speaker-means* a proposition *p* by an utterance *u* just in case *S* produces *u* with the three

intentions (a) of getting an audience A to believe p, (b) of having A recognize that S's intention is that A believe p, and (c) of it being part, at least, of A's reason for believing p that A recognized S's intention to get A to believe p. It is common for people first encountering a formulation of Grice's account of speaker-meaning to be baffled by its apparent complexity. But, the complexity is well motivated, and Grice's grasping the motivations here should probably be considered one of the genuine examples of innovation to be found in twentieth-century philosophy.

I will discuss below to what extent the notion of speaker-meaning is related to any ordinary notions. But a good mnemonic is that the ordinary notions of *saying* or *telling* as, for example, in the sentences [a] and [b] come somewhat close to *speaker-meaning*:

[a] Digby said that the game was exciting.

[b] Gerti told me that we would be on time.

The notions of *saying* or *telling* are both notions of *communication*. To *say* or to *tell* something to someone is to *communicate* something to them. And it is helpful to keep the ordinary notion of *communication*, or something like it, in mind in discussions of *speaker-meaning*. In the following informal discussion I will use *saying*, *telling*, *communicating*, *meaning*, and *speaker-meaning* somewhat interchangeably.

The first thing to notice about speaker-meaning is that it requires that the speaker intends to get an audience to have a certain belief<sup>33</sup>. But the second thing to notice is that not just any case of getting a person to have a belief is a case of a speaker *meaning* (*saying* or *telling*) something to somebody. If an action of mine is to be properly called an act of telling my guest that I am out of coffee, I must intend that my guest come to

believe that I am out of coffee. But the action with this intention by itself doesn't constitute telling. For I can perform an action with the intention that my guest comes to believe that we are out of coffee which is clearly not a case of *telling*. For example, I can just show my guest that my coffee canister is empty with the intention of their coming to believe that I am out of coffee. So performing an action intending that an audience come to believe something is necessary but insufficient for *speaker-meaning*.

If I perform an action with the intent that you come to believe something, I am presumably going to present you with some sort of evidence for the belief. Showing you the empty coffee canister is evidence for the belief that I am out of coffee. The thing that Grice seems to be the first to notice about all this is that in cases where we are trying to *communicate*, to *say* or to *tell*, something to some audience, the evidence that we present the audience should not be evidence that would have gotten the audience to have come to have the appropriate belief if it somehow were available to them apart from being shown *by somebody*. If my guest happened to see my empty coffee canister without my showing it, this would have amounted, presumably, to the same evidence for them that I was out of coffee as did my presenting it.

Consider, to use an example drawn from Grice, the difference between showing someone a photograph and presenting them with a drawing. A photograph will generally be evidence that what it depicts occurred, and this will be so independently of whether the photograph was intended to convey something to somebody. But a drawing depicting an occurrence does not - by itself - count as evidence that what it depicts occurred. If you show me a photograph of a situation, I will, in the usual case, come to believe that

that situation occurred, but my conclusion that it occurred will be drawn on the basis of what the photograph depicted.<sup>34</sup> But if you show me a drawing of a situation, since the drawing itself cannot be considered evidence that the situation occurred, if I am to come to the conclusion that the situation depicted, in fact, did occur, I will have to use as evidence for this conclusion not just what the picture depicts, but, presumably, that I took it to be your intention that I should believe what the drawing showed. That is, if I thought that you were showing me the drawing in order to get me to believe that what it depicted was the case, then I may well conclude - supposing I deem you trustworthy - that what it depicted was, in fact, the case. So, the fact that you intend to get me to believe that some state of affairs holds can count as evidence for me that that state of affairs, indeed, does hold.

Grice saw that this sort of evidence on the basis of recognition of intention is at the heart of communication, of speaker-meaning. If a speaker is to tell someone that some state of affairs holds, the speaker must perform an action intending to get an audience to believe that that state of affairs holds on the basis of their recognizing the speaker's intention to get them to so believe. The speaker's intention to get the audience to believe that the state of affairs holds *itself* must be part of the audience's reason for coming to believe that the state of affairs holds. Or, at least, the speaker should intend it to be so.

There are important counterexamples to both the necessity and the sufficiency of Grice's analysis of speaker-meaning<sup>35</sup>, but it is not my purpose here to give a comprehensive report on this aspect of Grice's view of meaning. The details of Grice's



analysis of speaker-meaning won't play a big role in the discussion in subsequent chapters. But it will play some role, and ultimately, in chapter 7, I will suggest an a variation to Grice's analysis. But there's no need to anticipate the details of all that here.

I turn now to the discussion of the role of the notion of *speaker-meaning* in the theory of *expression-meaning*.

It is not altogether obvious that there is a pretheoretically clear notion of speaker-meaning to be found and analyzed. This point has been at least hinted at by a number of writers.<sup>36</sup>

Grice himself in "Meaning" at one point turns our attention away from the schema

*A* meant<sub>NN</sub> by *x* that so-and-so (on a particular occasion)

which he begins with to the notion of *telling*:

What we want to find is the difference between, for example, "deliberately and openly letting someone know" and "telling" and between "getting someone to think" and "telling."<sup>37</sup>

This move seems to indicate some sort of unclarity in just what notion Grice was after. In Grice (1969) Grice speaks of "the notion of saying that *p* (in the favored sense of *say*)".<sup>38</sup> And in Grice (1968) he speaks of "what the speaker has *said* (in a certain favored and maybe in some degree artificial, sense of 'said')".<sup>39</sup>

So, though the basic formula of the Gricean program - something like, "expression-meaning to speaker-meaning and speaker-meaning to speaker attitudes" - may seem attractive enough at first glance, there really is quite a bit that is vague here. And I don't think that the vagueness can or need be finally or completely dispelled in advance of the consideration of proposals for actual theories of meaning. But, still, I will try to

clarify things at least a little on this matter.

What is wanted is a reductive analysis of the notion of a sentence's meaning what it does among a group of people, that is, of the notion of expression-meaning. One thing that this means is that concepts used in the desired analysis must be themselves analyzable without recourse to the notion of expression-meaning: the analysis of expression-meaning shouldn't be circular.

One way of proceeding in this endeavor will be to try to analyze expression-meaning in terms of propositional-attitude notions and one or more pre-theoretically available propositional speech-act notions like *saying*, *telling*, *asking*, *commanding*, etc. (along with perhaps non-semantic and non-psychological ancillary notions that need not concern us here). To succeed in such an endeavor would be quite an achievement, in my view, even without analyses of whatever speech-act notions were employed. But, to complete the entire project, the analysis of the speech-act notions would have to be achieved in terms that didn't require the notion of expression-meaning. I think that this overall strategy is probably the best and it informs much of what I say in the last chapter of this dissertation. But it is not the only strategy.

David Lewis, more-or-less, offers an analysis of expression-meaning without using, or proceeding through the use of, any propositional-speech-act notions. Lewis at one point does try to suggest that, after all, his theory will entail that expression-meaning is a sort of regularity of speaker-meaning among a group of people.<sup>40</sup> But, even if Lewis were wrong about this,<sup>41</sup> I should think that it wouldn't matter to the project of trying to understand the nature of expression-meaning. I see no good reason why an analysis of

expression-meaning should have to say or entail anything about a notion of speaker-meaning to be legitimate. I doubt that a serious theory of expression-meaning can avoid doing so - that is in part why I think the best strategy is to make free use of propositional-speech-act notions in the analysis of expression-meaning -, but I don't pretend to know of any *a priori* reasons why it should not avoid doing so.

It seems to me that through his life, Grice's own strategy for attempting to elucidate expression-meaning was a mixture of the two that I have just discussed. Instead of attempting to stick with antecedently available speech-act notions, he attempted to isolate a speech-act notion that would ultimately allow for the elucidation of expression-meaning. But even if his attempt to isolate such a notion was not an attempt to clarify an antecedently available speech-act notion, neither was it an attempt to construct a wholly artificial notion. Recall the passage quoted above at the beginning of section 2.1 in which Grice complains of causal theories that they fail to help elucidate the notion of a speaker meaning something on a particular occasion. Whatever the notion of speaker-meaning Grice wanted was, however, the ultimate interest of it is absorbed in the question of how successful a theory of expression-meaning that employs it might be.

#### **1.2.4 Natural and Non-Natural Meaning**

"Meaning" begins with the suggestion of a distinction between what Grice calls *natural meaning* and what he calls *non-natural meaning*. As examples of cases of natural meaning Grice gives:

Those spots mean (meant) measles.

The recent budget means that we shall have a hard year.<sup>42</sup>

As examples of cases of non-natural meaning, Grice gives:

Those three rings on the bell (of the bus) mean that the bus is full.

That remark, 'Smith couldn't get on without his trouble and strife,' meant that Smith found his wife indispensable.<sup>43</sup>

The natural/non-natural meaning distinction is indicated by Grice by giving the above examples and noting five differences in the kinds of inferences that can be drawn in the various cases. For example, he notes that it seems odd to say "those spots meant measles, but he hadn't got measles", but okay to say "those three rings on the bell (of the bus) mean that the bus is full, but it isn't in fact full - the conductor has made a mistake".<sup>44</sup> Grice also tells us that he would like cases of speaker-meaning as well as of expression-meaning to be counted as cases of non-natural meaning<sup>45</sup>. And cases in which the word "mean" (or cognates) are used in the sense of *purpose* or *intend* - as in "John meant to do the laundry" - Grice indicates he would like to include as cases of natural meaning. Classifying these various senses of "meaning" as Grice suggests seems right enough given the remarks by Grice which were alluded to just above about how to draw the natural/non-natural meaning distinction. But Grice does not really provide a sharp demarcating criterion between natural and non-natural meaning. In fact, he leaves off entirely the discussion of natural meaning to focus on the analysis of non-natural meaning after the following remarks:

The question which now arises is this: "What more can be said about the distinction between the cases where we should say that the word is applied in a natural sense and the cases where we should say that the word is applied in a nonnatural sense?" Asking this question will not of course prohibit us from trying to give an explanation of "meaning<sub>NN</sub>" in terms of

one or another natural sense of "mean."

The question about the distinction between natural and non-natural meaning is, I think, what people are getting at when they display interest in a distinction between "natural" and "conventional" signs. But I think my formulation is better. For some things which can mean<sub>NN</sub> something are not signs (e.g. words are not), and some are not conventional in any ordinary sense (e.g. certain gestures); while some things which mean naturally are not signs of what they mean (cf. the recent budget example).<sup>46</sup>

Grice is presenting here an aspect of his thought that links it up to a long tradition of thinking about language that begins at least as far back as Plato's *Cratylus* where a distinction between conventional and natural meaning was drawn perhaps for the first time.<sup>47</sup> My discussion right now will focus on the question of what Grice might have meant when he spoke of an "explanation of 'meaning<sub>NN</sub>' in terms of one or another natural sense of 'mean'".

One very general thing that Grice *might have* meant by speaking of an "explanation" of non-natural meaning in terms of some sort of natural meaning is that, ultimately, one should expect that expression-meaning and speaker-meaning will be analyzable in terms of some sorts of non-intentional notions. The idea, presumably, would be that, first, there would be the two-tiered reduction of expression-meaning - expression-meaning would be explained in terms of speaker-meaning and speaker-meaning would be explained in terms of propositional-attitude notions -, but, then, propositional-attitude notions would be explained in a certain way by wholly non-intentional notions. The explanation of propositional-attitudes in non-intentional terms would have to talk about the relations that held between organisms and the world such that organisms could have intentional states about the world. These organism-world relations would amount to some sorts of relations of *natural-meaning*. So, the idea would be, that Grice, when

he speaks about the "explanation" of non-natural meaning in terms of natural meaning, has in mind that ultimately the analysis of public-language semantic notions will have to lead us to some sort of non-intentional explication of propositional-attitude notions and this explication will use some notion or other of *natural-meaning*.<sup>48</sup>

If Grice meant this by his comment, then he was simply indicating that things might be more-or-less like what some of the theories I will discuss in the later chapters of this dissertation make them out to be. And I don't think it is altogether odd to read Grice's comment this way. But in fact, I think he had something else in mind with the comment which I will now discuss a bit.

For Grice, we might say, all language is founded in speech somehow, but speech is at least conceptually possible without language. This is just to summarize the idea that expression-meaning is *reducible* to speaker-meaning (along with some other notions that are not important here): if speaker-meaning somehow turned out to require expression-meaning, then whatever the relation expression-meaning stood in to speaker-meaning, it wouldn't be one of *reduction*. But speech is essentially the production of some behavior or artifact to *communicate* something to somebody. If speech is possible without language, then some behaviors or artifacts will be capable of being used in communication even though they don't have conventional meaning. But since such behaviors or artifacts are capable of being used in communication, that is, capable of being used by one person to get another person to have a belief, we might just be tempted to suppose that these behaviors or artifacts have *meaning* in some sense, even if this sense is not that of *expression-meaning*. I believe that this sort of reasoning was behind the

traditional notion of *natural meaning* and that to some degree it lies behind Grice's interest in trying to clarify such a notion as well as his suggestion that there might be something of an "explanation" of non-natural meaning in terms of natural meaning. If all language is conceptually traceable somehow back to language-less speech and language-less speech requires behaviors or artifacts that naturally mean, then all language has been traced to natural meaning. I think that this is the sense in which Grice might have felt there might be an "explanation" of non-natural meaning in terms of some notion of natural meaning.<sup>49</sup>

I think there is both something right and something wrong in this line of reasoning. There are two parts to what I think is right here. (1) If the Gricean picture of language is correct, we do somehow expect that every bit of language must somehow be traceable - conceptually, not historically - to non-linguistic speech. And (2) a behavior or artifact that is used in non-linguistic speech must have some feature that makes it suitable for its use in communication. What I think is wrong here is that the feature of a behavior or an artifact used in non-linguistic speech that makes it suitable for communication needs to somehow exploit behaviors or items that naturally mean something.

It seems to me that what Grice probably has in mind is a case like the following. Suppose that June and Bingo, two language-less people, have noticed that whenever the big shady tree shakes in a certain way a buffalo is charging nearby. So, we may suppose the shaking in that way of the big shady tree *naturally means* that a buffalo is charging nearby. One day June wants to tell Bingo that a buffalo is charging nearby and tries to

do so by attempting to shake the big shady tree - which we may suppose was not shaking by itself on this occasion - in that certain way. Supposing that such a case might be a case of language-less telling, we can see that there is a sense in which *natural meaning* is exploited to achieve non-natural meaning, in this case, to tell somebody something.

But it isn't hard to imagine cases in which there is no real connection to anything that *naturally means* something in any obvious sense.<sup>50</sup> Imagine another scenario in which June wishes to get Bingo to believe that a buffalo is charging. Suppose that June and Bingo have known each other a long time and that once, some time before the present occasion, June and Bingo saw a charging buffalo and June suddenly began to dance playfully and laugh. Suppose also that June and Bingo have seen many buffalo without June's doing the buffalo dance and that a few times June did the buffalo dance for fun without a charging buffalo about. This last supposition, of course, is to ensure that June's buffalo dance is not like that certain shaking of the big shady tree which could be taken to *naturally mean* that a buffalo was charging nearby, I presume, because its shaking in that way was sufficient in June's and Bingo's experience to indicate that there was a buffalo charging nearby. Suppose further that just a week earlier June and Bingo were in a somewhat similar situation to the present one but in the earlier episode Bingo seemed to June too close to the path of a charging buffalo. On that occasion June did the buffalo dance but with a face that she knew Bingo would take to be incongruous with June's merely playing. June did this *hoping* that it might cause Bingo to suspect that a buffalo was about, look about for it, see the buffalo that was, in fact, charging, and move herself to a safer distance from its path. None of what June hoped for happened however.



Bingo, it turned out, was at a safe enough distance when the buffalo past by. But upon seeing the buffalo pass by Bingo realized what June was hoping to do. And June came to believe that Bingo upon seeing the buffalo pass by realized what June hoped to achieve by doing the buffalo dance with a face she knew Bingo would take to be incongruous with play. So, now June wants to get Bingo to believe again that a buffalo is charging. She remembers all the episodes just mentioned and she believes that Bingo remembers them too. And she believes because of all of this that if she does the buffalo dance with a face that she knows that Bingo is likely to take as incongruous with play, Bingo will recognize that June intends to get her to believe that a buffalo is charging nearby and thereby, in fact, come to believe that a buffalo is charging nearby. And so she does the buffalo dance with an appropriate expression intending Bingo to reason as she has imagined.

June in this case, it would seem, has succeeded in *telling* Bingo that a buffalo is charging. But she has not exploited anything that could easily be understood to *naturally mean* anything. What made it possible for June to succeed in communicating with Bingo in this case might be described as an accidental feature of their shared history that could somehow be exploited to achieve communication. I don't know if there is anything interesting in common between this sort of case and cases like the first one I considered where it is remotely plausible that items that *naturally meant* were used to achieve communication. And I don't know if there are further cases that ought to be considered. My point is simply that exploitation of items that *naturally mean* doesn't seem to be a necessary feature of language-less speech.

Of course, there is the not-so-interesting common element in the two cases that I have already mentioned: there must be *some* feature of an artifact or a behavior used in language-less speech that makes it suitable for communication. One could define *natural meaning* as the having of such a feature, but it is hard to see what would be gained by doing so.

The sorts of issues that are involved here will arise again at the very end of this dissertation. And they are not unimportant to the Gricean. A common objection to Griceanism takes the form of the observation that it seems unlikely that many of the things that we say in our everyday talk could ever really be thought if we didn't have language, let alone expressed in language-less speech.<sup>51</sup> For example, it may seem hard to imagine that language-less creatures could have a thought like that if next Thursday it rains, perhaps it will be better not to try to travel to my cousin's house unless I can get a ride with a friend. And even if you could convince yourself that a language-less creature can have some such thought, still, it is hard to see how anything could be usable for expressing such a thought to another unless there were some conventional devices for doing so. So, the objection has it, the Gricean program is implausible because there seems to be no way that a sentence that has as its meaning a complex thought like the above could derive its meaning from non-linguistic communicative intentions.

Griceans have responded to this sort of complaint by sketching stories or suggesting sketches of stories that have it that things start off somewhat like in the June and Bingo stories above. A little language gets introduced. Then, once that language is introduced, this makes it possible for more complex thoughts to be available. For

example, once June and Bingo have a conventional way of expressing the thought that a buffalo is charging, it is conceivable that they now have the ability to see the activities of other creatures as charging activities because these activities are similar enough to the charging buffalo cases. And perhaps by a clever variation of the conventional device developed for mentioning charging buffalos, bolting rhinos can be talked about as well. Then after repetition, this sort of communication becomes conventional and part of June and Bingo's shared language. And so on. A little thought, some communication, then language; then a little more thought, some creative extensions of already existing language in new communications, and then after repetition, new language; etc. And in this way thought precedes language, but also depends on it and eventually the richness of our own linguistic devices can be arrived at.<sup>52</sup>

The matters here are interesting and, as I say, not unimportant to the Gricean, but I will not be able to discuss them further in this dissertation.

### **1.3 Gricean Theories of Meaning**

I will talk now briefly and in general terms about the goals of the theory of meaning and I will distinguish various sorts of theories that, I think, can all fairly be called Gricean. And I will indicate the sort of theory that the present dissertation will aim at.

In chapter 3 I will try to motivate a little better the view that we distort nothing essential to our purposes of coming up with a general theory of expression-meaning if we begin by considering the abstract case of languages without either indexicals or non-

indicative moods. For now I will just state that that will be what I will do throughout most of this dissertation.

Supposing that abstracting in that way is appropriate, then, we can state a primary goal of theorizing about expression-meaning as, more-or-less<sup>53</sup>, the provision of a *satisfactory completion* of [M]<sup>54</sup>:

[M]  $(\forall P)(\forall x)(\forall y)(x \text{ means among the members of } P \text{ } y \equiv \dots)$

It is extremely difficult if not impossible to say ahead of actual critical work on particular theories what should count as a *satisfactory completion* of [M]. But someone working within a broadly Gricean framework can say a little bit about this anyway.

If we are after a Gricean theory of meaning in a pretty strong sense, then the completion of [M] must not contain any vocabulary that expresses public-language semantic notions or any vocabulary that expresses notions that require for their applicability the applicability of public-language semantic notions. To guarantee that this requirement has been met may be very difficult. For, Gricean theories of expression-meaning, in a slightly broader sense now, are generally aimed to provide glosses of semantic notions in terms of propositional-attitude notions. To guarantee that the condition under discussion has been met will be to guarantee that propositional-attitude notions themselves don't require for their applicability the applicability of any public-language semantic notions. I do believe that there is some reason to think that propositional-attitude notions won't presuppose public-language semantic notions, but I don't know of any conclusive arguments that this is so. But I can't discuss the matter further here.

I will be happy, however, in the discussion to follow to count as a satisfactory completion of [M] any completion that makes free use of propositional-attitude *and* propositional-speech act notions so long as none of the other notions it uses presuppose public-language semantic notions. I will count such a completion as a Gricean theory of meaning. But it may be useful to qualify this: I will call such a theory a *weak* Gricean theory. To have a *strong* Gricean theory of meaning, let's say, is to have the conjunction of a weak Gricean theory and a theory of propositional-attitude and propositional-speech act notions that shows that such notions don't themselves presuppose any public-language semantic notions. One might want to speak of a third sort of Gricean theory as well: we might say a *moderate* Gricean theory was one that glossed public-language expression-meaning in terms only of propositional-attitude notions along with some other non-public-language semantic notions, but that did not avail itself of propositional-speech act notions.<sup>55</sup> Grice, then, himself was apparently only aiming at a moderate Gricean theory in trying to elucidate the notion of speaker-meaning - a propositional-speech act notion - in terms of propositional-attitude notions. That is, he used a propositional-speech act notion as a stepping stone to a theory of expression-meaning that did not, in fact use such notions. Another way of putting this which uses the terminology I suggest here is that Grice used a weak Gricean theory to move on to a moderate Gricean theory.

This is clearly not at all a complete discussion of the constraints on *satisfactory completions* of [M]. But, as was said, it is probably the best that can be provided ahead of critical work on actual theories, at least for someone who is interested in trying out the Gricean program. It always remains an open possibility that some bit of vocabulary turns

out, after critical assessment, to be less clear than it was thought to be.

A satisfactory completion of [M], then, would amount to a theory of meaning for languages with no indexicals or non-indicatives because it would describe in terms of propositional-attitude and propositional-speech act notions the conditions necessary and sufficient for a sentence to mean some specific thing among the members of a population.

#### **1.4 Summary**

In this chapter I introduced some important aspects of the Gricean program in the theory of meaning by discussing various aspects of Grice's article "Meaning" and I discussed in a general way the goals of a theory of meaning. In the next chapter I will give a brief history of the investigation of meaning in the twentieth century. Then in the subsequent chapters I will move on to particular issues involved in providing a theory of expression meaning.

## Notes

1. I am pretending all Finns speak Finnish, but there are Finns who speak only Swedish and perhaps there are other non-Finnish-speaking Finns as well.
2. When I speak of *propositional-attitude notions* I will mean notions like *belief, desire, intention*, etc. and **not** *speech-act notions* like *speaker meaning, telling, saying*, etc. even though these latter can be called *propositional-attitude notions* as well. I will sometimes refer to the latter notions as *propositional-speech-act notions*.
3. See Grice (1957), Grice (1968), Grice (1969), and Grice (1982).
4. See Schiffer (1972), Schiffer (1982), and Schiffer (1988).
5. See Lewis (1969) and Lewis (1975).
6. See Strawson (1969), Fodor (1975), Fodor (1989), Loar (1976), Loar (1981), Bennett (1973), and Bennett (1976). See also Landesman (1972) and Avramides (1989), although Avramides argues that the Gricean program has been largely misunderstood in important respects by many philosophers, including at least some of the above, who have taken themselves to be working within it.
7. I am simplifying somewhat: Grice conceived of the notion of *meaning* as applying to more than just what are ordinarily understood to be "linguistic expressions". Things will get appropriately complicated in due course, but I will stick with the slightly loose talk here.
8. Of course, I will always mean *public-language semantic notions* in this and the following sections. *Private-language semantic notions*, i.e., semantic notions pertaining to a *language of thought* will not be discussed until the last section of chapter 2.
9. Stevenson (1944).
10. Grice (1957), p. 217. Page number references for Grice (1957) are to Grice (1989).
11. Grice (1957), p. 217. The "x"s in the predicates of this quotation are intended to range over expressions; the "A"s, over persons. "Means<sub>NN</sub>" and its cognates are intended by Grice to express the relation of what Grice calls "*nonnatural* meaning". This special notion and what Grice intended to convey with it are unimportant to the present discussion except insofar as "means<sub>NN</sub>" and its cognates can be read simply as "means" or a grammatically appropriate cognate.
12. Grice (1957), p. 220.

13. In Grice (1982): "It seems plausible to suppose that to say that a sentence (word, expression) means something (to say that 'John is a bachelor' means that John is an unmarried male, or whatever it is) is to be somehow understood in terms of what particular users of that sentence (word, expression) mean on particular occasions." (See Grice (1989), p. 298.)
14. See Avramides (1989), pp. 33-38.
15. Avramides (1989), p. 21.
16. Ibid.
17. Avramides (1989), p. 25.
18. Avramides (1989), p. 33.
19. Avramides (1989), p. 174, n. 112. I will quote the passage from Grice below.
20. Ibid.
21. Ibid.
22. Grice (1957), p. 219. The details of Grice's analysis of speaker-meaning are not the concern of this section, but of the next, so I don't complete the quote here.
23. Grice (1957), p. 220. Again, all that is important right now are the connectives, so I leave out the details of the various proposals here.
24. Grice and Strawson (1956), p. 202.
25. Since I talk in the present essay about the analysis of *notions*, I will retain that talk in the current section in spite of the fact that Grice and Strawson talk in terms of the analysis of *expressions*. I don't see that this should engender serious confusion for the careful reader.
26. Ibid.
27. Grice and Strawson (1956), p. 203.
28. Grice (1986), p. 67.
29. Grice (1987), p. 351.
30. Grice (1957), p. 216.



31. See the table of contents of Grice (1989), p. vii, where "Meaning" is dated "1948, 1957". Stephen Schiffer, Richard Grandy, and Richard Warner have all told me in personal correspondence that Grice originally wrote the paper for a seminar that he and Strawson were to give in 1948, but was reluctant to publish it. Strawson had the article typed out and submitted it for publication without Grice's knowledge. Strawson only told Grice after the article was accepted for publication. Stephen Schiffer has told me that Grice himself told him this story and Richard Warner has written to me that he also heard this story from Grice. The only person I have been able to find who has noted the 1948 date is Fogelin in his review of *Studies in the Way of Words*: "'Meaning'...was first published in 1957, and apparently given as a lecture almost ten years earlier in 1948" (Fogelin (1991)). But that "Meaning" was given as a lecture does not seem to be noted anywhere in *Studies in the Way of Words* and Fogelin doesn't mention a source for the information he provides.

32. I am not going to discuss at all the way Grice deals with imperatives or other moods.

33. Again, I am ignoring non-indicative speech acts for simplicity here.

34. Of course, there are all sorts of background beliefs that I need to have here, and in unusual cases these may have to be beliefs that you are not trying to deceive me with a trick photograph of some sort. But I ignore such unusual cases.

35. See Strawson (1964), Grice (1969), and Schiffer (1972) for discussion of the most important counter-examples to Grice's original analysis and suggestions on what to do about them.

36. See Schiffer (1982), pp. 120ff., and Yu (1979), p. 281. See Vlach (1981) for someone who proceeds on the assumption that there is a pretheoretically clear notion of speaker-meaning. See also Shwayder (1972).

37. Grice (1957), p. 218.

38. Grice (1969), p. 87.

39. Grice (1968), p. 118.

40. This is not how Lewis presents his views, but this is how I take his discussion in Lewis (1969), pp. 152-159. See Schiffer (1993) for an argument that one thing that may be wrong with Lewis-style theories of meaning is that they *do not* use any propositional-speech-act notions. I will discuss this criticism in chapter 5 below.

41. Or if I am wrong in my reading of Lewis here.

42. Grice (1957), p. 213.

43. Grice (1957), p. 214.

44. Grice (1957), pp. 213-214.

45. Grice (1957), p. 215.

46. Grice (1957), p. 215.

47. For some of the history of the sort of speculation about the origins of language that I will discuss here, see Kretzmann (1967) and Aarsleff (1976). Aarsleff's discussion ends at the beginning of the 19th century with the rise of the idea that language study should be more rigidly scientific and less speculative than it had been in the past. There is no discussion of the resurrection of speculation about language origins in "respectable" circles that was due chiefly to the advent of evolution theory. See also Ullman (1975) and Wright (1976).

48. Russell seems to have suggested that the organism-world relations that could constitute propositional attitudes could be understood in terms of a notion of *natural meaning*. In one place he says of words and ideas that they "both have meaning" but that "in the case of words, the relation to what is meant is in the nature of a social convention, and is learned by hearing speech, whereas in the case of ideas the relation is 'natural'; i.e. it does not depend upon the behavior of other people, but upon intrinsic similarity and (one might suppose) upon physiological processes existing in all human beings, and to a lesser extent in the higher animals" (Russell (1948), p. 96). Landesman quotes this passage too and notes the dubiousness of the specific take on *natural meaning* as some sort of *intrinsic similarity* (Landesman (1972), p. 3). But if you read for "ideas", "language of thought expressions" and for "intrinsic similarity", "some sort of causal relation" you just about have an idea that survives today in, hopefully, a somewhat more acceptable form in the work of Fodor and a number of others. See, for example, Fodor (1987) and Fodor (1990). Also, Russell's view evolved over time from 1919, when he essentially first began working on the theory of meaning (see Russell (1919), Russell (1921), Lecture X, Russell (1926), and my discussion of the history of the theory of meaning below, chapter 2). In one place, for example, he interestingly claims with respect to words and "images": "...the relationship which constitutes meaning is much the same in both cases. A word, like an image, has the same associations as its meaning has" (Russell (1921), p. 210). Thus, the causal relations a word or "image" has is what matters for the determination of its meaning: if the relevant associations caused by a word or "image" are like the associations caused by some thing, then the word or "image" *means* that thing. This is not an unproblematic view, but it is somewhat more attractive than the resemblance-style view mentioned above. I can't investigate the matter any further here however.

49. That this is the sort of thing that Grice had in mind is born out by his discussion in "Meaning Revisited" (Grice (1982)). At one point he characterizes what he wants to do as follows: "So what I want to do now is look to see if one would represent the cases of nonnatural meaning as being descendants from, in a sense of 'descendant' which would suggest that they were derivative from and analogous to, cases of natural meaning. I shall

also look a little at what kind of principles or assumptions one would have to make if one were trying to set up this position that natural meaning is in some specifiable way the ancestor of nonnatural meaning" (Grice (1982), p. 292).

50. Catherine Joos first helped me understand cases like the following.

51. I believe that Davidson is suggesting such a criticism of the Gricean picture in Davidson (1973), Davidson (1974), and Davidson (1975).

52. Strawson mentions the criticism I am discussing here and offers a sketch of the sort of story I have been indicating as a response to it: "...it is clear that we can, and do, communicate very complicated things by the use of language; and if we are to think of language as, fundamentally, a system of rules for facilitating the achievement of our communication-intentions, and if the analysis is not to be circular, must we not credit ourselves with extremely complicated communication-intentions (or at least desires) independently of having at our disposal the linguistic means of fulfilling those desires? And is that not absurd? I think this is absurd. But the programme of analysis [essentially, the Gricean program] does not require it. All that the analysis requires is that we can explain the notion of conventions of communication at a rather basic level. Given that we can do this, then there is more than one way in which we can start pulling ourselves up by our own linguistic boot-straps" (Strawson (1969), p. 174). Strawson then goes on to talk about how language could evolve from expression of very basic thoughts to more complex thoughts and in turn, their expression.

53. "More-or-less" because of details like subsentential- expression-meaning and non-literal meaning. Such details will be discussed briefly in chapter 3.

54. If you are worried about the variable "y" and the quantifier that binds it, wait till section 3.3 of chapter 3 where I talk about propositions.

55. Loar sets in contrast Grice's way of theorizing about meaning and that due to Ramsey in Ramsey (1927) in which, in Loar's words, "[t]he idea is that you give the meaning of a sentence of z's by saying what z would be *believing* if z accepted that sentence" (Loar (1981), p. 210). I have no complaints about Loar's making the contrast, but from the perspective I am suggesting now, we could talk about Ramsey as suggesting a *moderate Gricean theory*.

## Chapter 2 The Theory of Meaning in the Twentieth Century

### 2.1 Introduction

Grice criticized on a number of grounds the theory of meaning found in C. L. Stevenson's *Ethics and Language*.<sup>1</sup> His final criticism, essentially, was that, though it might be expected that a theory of meaning should both explain what *speaker-meaning* amounts to and explain *expression-meaning* in terms of *speaker-meaning*, a theory like Stevenson's shows no promise of being able to do either of these things. Grice offers this last criticism somewhat more tentatively than his earlier criticisms and says the following about it:

I am sympathetic to this more radical criticism, though I am aware that the point is controversial.<sup>2</sup>

Since the criticism that Grice refers to here is really a statement of the program that his paper champions, it seems that Grice, in saying that he is "aware that the point is controversial" - and not just, for example, that it may *appear to be* controversial -, is implying that the program, or, at least, some important aspect of it, has already been discussed somewhere. But where?

In this chapter I will provide a rough sketch of the progress of the theory of meaning through the twentieth century, up to Grice and a little beyond him. I believe that this sketch will provide some clues as to the sources of Grice's thought and I will suggest an answer to the above question. But I also think such a sketch is interesting in its own right and worth providing since much of this history, for some reason, is overlooked or barely discussed today.

Before beginning with the actual story here I would like to note a certain very rough conception of the history of the theory of meaning through the first half of this century that is pretty common but extremely distorted. A very crude outline of this conception goes something like this. Frege and Russell made important advances in logic and Wittgenstein in his *Tractatus Logico-Philosophicus* used these advances in an attempt to understand the nature of language generally. The logical positivists and others in the 20's and 30's developed some of Wittgenstein's ideas along with some of their own and some that came from the work in formal semantics by Tarski, Church, Carnap and others. Then Wittgenstein's later influence sort of dismantled the ideas central to the conception of language that derived from the seminal logical work of Frege and Russell. That brings us about to 1950 or so.

This conception, though it has a germ of truth in it, is highly distorted. And the account of the history of the theory of meaning that I am about to offer may seem somewhat revisionary with respect to the above conception. I think that this appearance can be somewhat lessened by noting that the expression "theory of meaning" means different things in different mouths. But if lessened, still, I doubt the revisionary appearance of the following history can be wholly eliminated. I will say a little bit more about the usual rough conception of the theory of meaning and its motivations towards the end of this chapter.

## 2.2 Victoria Welby

The history of the theory of meaning in the twentieth century, I believe, should

rightly begin with the work of Victoria Welby (1837-1912). Welby is the first, I think, to see the question, "What is meaning?" as worth careful scrutiny in its own right. In an early essay, "Metaphor and Meaning", published in *The Monist* in 1893, Welby tells us:

...it is rather a hope for the future, that one of the most practically serviceable of subjects, that of Meaning, its conditions and changes - shall be seriously taken up. [Welby (1893), p. 512]

Indeed, one of her most important publications is her book *What is Meaning?* which was published in 1903. This book is praised by C. S. Peirce in a review he wrote of both it and Russell's *The Principles of Mathematics*. The review begins:

Two really important works on logic are these; or, at any rate, they deserve to become so, if readers will only do their part towards it.<sup>3</sup>

Peirce estimates the general importance of the book in the following way:

The greatest service the book can render is that of bringing home the question which forms its title, a very fundamental question of logic, which has commonly received superficial, formalistic replies. [Welby (1977), p. 159]

This comment of Peirce's could apply not just to *What is Meaning?*, but to all of Welby's works. They are of value and historical interest, for the most part, not so much because they systematically pursue this or that line of argument about *meaning* or related notions, but because they persistently call attention to the need for an answer to the question of what meaning is and the lack of any good answer to it in the work of her day. But this is not to say that Welby's works have no interesting strands of thought that are perhaps important to later work. They do, and I will mention some of these and their relations to each other. I will also say something about Welby's influence on later thought.<sup>4</sup>

A good theme to begin a discussion of Welby's work with is that of her scepticism towards the idea that a linguistic expressions has some sort of central, core, or standard, literal meaning - she often refers to this notion as that of *Plain Meaning* - which does not vary from context to context<sup>5</sup>:

The fact is, that we have been postulating an absolute Plain Meaning to be thought of, as it were, in capital letters. We have been virtually assuming that our hearers and readers all share the same mental background and atmosphere. We have practically supposed that they all look through the same inferential eyes, that their attention waxes and wanes at the same points, that their associations, their halos of memory and circumstance, their congenital tendencies to symbolise or picture, are all on one pattern. [Welby (1893), pp. 512-513]

The very phrases which are our only shorthand for the vast oratory of nature and experience betray us in the using. We have taken them as though they were like numerals invariable in meaning, thus supposing them subject to a permanent uniformity....

But the suggestion now made is that this is precisely one of the most dangerous of presuppositions. [Welby (1893), pp. 513-514]

Since no two circumstances in which an utterance is produced can ever be exactly identical with respect to the utterer's psychological states, no two utterances, Welby argues, could ever mean the same thing.

Of course, Welby's scepticism towards the notion of expressions having central meanings was not at all an uncritical one and there is a second argument that Welby deploys in defense of her view of the notion of central meanings. In the following passage we see that she is concerned with the positive motivations, supplied here by quotations from Stout, for the thesis that expressions have central meanings:

It is perhaps not necessary that "there should be an identical element of meaning pervading all the applications of a word. Moreover, this common element, in so far as it does exist, cannot be called a *meaning* of the word in the same sense as the occasional signification." And "the

'usual' or 'general' signification is not in itself one of the significations borne by a word. It is a condition which circumscribes within more or less vague and shifting limits the divergence of occasional meanings." Would not this repay more extended treatment? *E.g.*, if a general and usual signification is not a meaning, is it ever a *sense*? "In order to fix the occasional meaning of words" we need a cue; but permanent change of meaning arises from a gradual shifting of limits. [Welby (1983), pp. 256-257]<sup>6</sup>

Thus, Welby weighs the motivations for supposing that expressions have central meanings against the phenomenon of meaning change. She sees the force of the thought that without *something like* core meanings it is hard to explain how one figures out what another means in producing sounds: sounds as linguistic expressions must somehow have *some* properties which give a listener *some* clue about what a speaker wants to get across in using them. But Welby does not accept that such properties of linguistic expressions allow for an ultimately useful notion of *invariant meanings across contexts*. For Welby there cannot be a useful notion of *invariant meaning across contexts* since meaning change does, in fact, occur, and since such change must be due to the uses made of expressions in particular contexts. Her idea, I believe, is that even though we only notice "permanent" meaning changes after long periods of time, such changes must be taking place in ordinary contexts by way of subtle "shifting of limits", that is, subtle differences in the applications of terms; thus, appearances of stability should be taken with a grain of salt: there really cannot be stable central meanings which expressions carry from one context to another.<sup>7</sup>

Welby uses her scepticism towards standard meanings for expressions as the basis for her criticisms of those who would hope to construct ideal logical languages or international languages like Esperanto or Volapük:



At present we have not even attained to an adequate conception of what an ideal language should be: we think of it, if at all, as...a formalised dialect of culture with its phrases "rendered according to the genuine and natural importance of words," as if this were anything but what their speakers intended by them! Or we try to invent an artificial 'Volapük.' It is surely time that the fetish of a possible Plain Meaning, the same at all times and places and to all, were thoroughly exposed, and students more explicitly warned against anything approaching it, accept on the narrowest basis of technical notation. [Welby (1896), p. 192]<sup>8</sup>

She opposed those who felt that important ends could be met by social institutions surrounding some sort of idealized international language, reasoning that even the most ideal language imaginable still needs to be employed by speakers and that such employment will always serve, and therefore be at the mercy of, the particular intentions of speakers on particular occasions. We have already seen this view of Welby's in the discussion above of her understanding of meaning change. The ends that supporters of movements for ideal or international languages wanted to achieve, she felt, would be better served by general instruction in "significs", her term for the most general study of speech and language. Indeed, Welby had ambitious educational hopes, imagining that significs should become part of the standard education of children from their earliest years.<sup>9</sup> And one of the central goals of this training in significs would be the student's critical sense of what might be called *conventional* elements in speech coupled with a genuine sensitivity to the subtleties of context:

The child must therefore learn to gauge context by context, and to hunt with unerring scent for some else unnoticed peculiarity of apparently chance expression or form of expression which gives a clue to the writer's (or speaker's) real sense, and therefore to the true order of sentences. In this work he must even in the interests of detection ignore grammar. He must learn to become a sense-detective, detecting his own as well as others' more subtle blunders, more hidden flaws in that significance which it is the object of articulate expression to convey. The accomplished

Significian is at least a Sherlock Holmes, and more, a Helmholtz. [Welby (1983), p. 225]<sup>10</sup>

Welby is highly critical of those who would place adherence to convention prior in importance to creative expressiveness in language:

'Words are conventional signs. They mean what they are intended to mean by the speaker and understood to mean by the hearer. There is no other sense in which language can be properly said to signify anything' (p. 220 [of Greenough and Kittredge's book, *Words and their Ways in English Speech* (published in 1902)]). Therefore it is plain that if we chose we could acquire complete control over language which now makes a mock of our helplessness and leaves us dumb where we most need expressiveness. [Welby (1983), pp. 283-284]

And in *Significs and Language*, published just a year before her death, she discusses this theme once again:

...great and wise and beautiful things are conveyed to us, and we rise in response beyond the self of commonplace with which we have no right to be content. Only, that response is unconsciously impoverished and even distorted by quite avoidable drawbacks, which we not only complacently tolerate but teach to our children, thus ensuring their permanence and stifling the instinct of right expression which, though in quaint forms, shows itself clearly in the normal child until successfully suppressed. And though we do now and then recoil from a glaring misuse of term in the 'rising generation,' and lament such a lapse from *our* good ways, we never see that the fatal seed has been sown, the fatal tradition of a far more extensive misuse has been handed on, by us; that in scores and hundreds of instances we have carefully habituated the child, trained it, to say one thing when it means another, or to be content to leave much of language in rags or else cramped by antique armour. [Welby (1911), pp. 62-63]<sup>11</sup>

Just about all of the above quotes exhibit another theme of Welby's work, even more pervasive and fundamental than those already mentioned. This is that of the identification of *meaning* with the speaker's intention to convey something to an audience. Throughout her writings on language this theme rings out constantly. In "Sense, Meaning, and Interpretation" published in 1896, for example, she tells us:

The fact that Meaning includes Intention and End seems to indicate that it is the most general term we have for the value of a sign, symbol, or mark. [Welby (1896), p. 28]

In *What is Meaning?* Welby clearly identifies *meaning* with the speaker's intention and she uses this conception of meaning to once again oppose the idea that expressions have standard meanings apart from their contexts of use:

There is, strictly speaking, no such thing as the Sense of a word, but only the sense in which it is used - the circumstances, state of mind, reference, 'universe of discourse' belonging to it. The Meaning of a word is the intent which it is desired to convey - the intention of the user. [Welby (1983), p. 5]

She repeats something like this formulation in her 1911 article "Significs" for *The Encyclopedia Britannica*:

..."Sense" is not in itself purposive; whereas that is the main character of the word "Meaning," which is properly reserved for the specific sense which it is *intended to convey*.<sup>12</sup>

For Welby, meaning is always a matter of the creativity or resourcefulness of a speaker in a particular context trying to find a reasonable method for conveying thoughts. Almost all of Welby's thought on language stems from a sense that this the case.

I will leave off here my discussion of Welby's thought on language even though there are many other themes and issues that Welby was concerned with in her studies of language.<sup>13</sup> I will now note some of the places where Welby's influence on thought about language can be found.<sup>14</sup>

Welby never had a formal education and she was not associated directly with any institution. Yet she corresponded and was acquainted with many scholars, including some of the greatest thinkers of her day, on a wide variety of subjects. Russell, for example,

corresponded with Welby from 1904 until 1910 and the two met for discussions on a number of occasions.<sup>15</sup> In his 1926 review of Ogden and Richards (1923), *The Meaning of Meaning*, Russell says the following:

When, in youth, I learned what was called "philosophy"..., no one ever mentioned to me the question of "meaning". Later, I became aware of Lady Welby's work on the subject, but failed to take it seriously. I imagined that logic could be pursued by taking it for granted that symbols were always, so to speak, transparent, and in no way distorted the objects they were supposed to "mean". Purely logical problems have gradually led me further and further from this point of view. [Russell (1926), p. 138]<sup>16</sup>

It is not completely clear from this remark what Russell's assessment of Welby's work was when he wrote the review. But it seems, at any rate, he understood her to be a fundamental figure in the history of the subject. And it is interesting to note that in the same short review he presents as "truisms" about language a few theses that are reminiscent of views we have seen to be Welby's:

The natural function of words is to have affects upon hearers which the speaker desires. [Russell (1926), p. 139]

And again, just below this in the review:

*Words are means of producing effects on others.* I once canvassed a retired Colonel in the Liberal interest during an election, and he said: "Get out, or I'll set the dogs upon you." These words had, and were intended to have, the same effect as the dogs would have had. [Russell (1926), p. 139]

Indeed, these are truisms, but not everybody has felt the need to stress them. I will talk about Russell's own contribution to the history I am sketching later. These truisms which are at least thematically traceable to Welby are very important to Russell's own theory of meaning.

Peirce had a long correspondence with Welby which lasted from 1903 until the

end of 1911, just months before Welby's death in 1912. In his letter of March 14, 1909

Peirce comments on the relation of some of his ideas to some of Welby's:

I confess I had not realized before reading it [Welby's *Encyclopedia Britannica* article "Significs"], how fundamental your trichotomy of Sense, Meaning, and Significance really is. ...I now find that my division [of three kinds of Interpretant] nearly corresponds with yours, as it ought to do exactly, if both are correct. I am not in the least conscious of having been at all influenced by your book in settling my trichotomy, as nearly as it is settled; and I don't believe there was any such influence; though of course, there may have been without my being aware of it. In reading your book my mind may, quite well have absorbed the ideas without my remembering it; and when I came to search for a division of the Interpretant, those ideas may have seemed to me to have been struck out by processes of thought that I thought then were presenting themselves to me for the first time, when the fact was that they were due to a bent of my thoughts which the perusal of your book had made. However, as I do not believe this did happen, I feel some exultation in finding that my thought and yours nearly agree, for I think it is because we were both trying to get at the truth; and I should not wonder if you have the same feeling. But as far as the public goes, I can only point out the agreement, and confess to having read your book. [Welby (1977), p. 109]

This passage is particularly interesting since, in spite of Peirce's many disclaimers of influence by Welby, Peirce clearly acknowledges that Welby's work on meaning is earlier than his own. There is no question raised about Peirce's possible influence on Welby.

Peirce goes on to comment on Welby's conception of *meaning*:

Let us see how well we do agree. The greatest discrepancy appears to lie in my Dynamical Interpretant as compared with your "Meaning." If I understand the latter, it consists in the affect upon the mind of the Interpreter that the utterer (whether vocally or by writing) of the sign intends to produce. My Dynamical Interpretant consists in direct effect actually produced by a Sign upon an Interpret-er of it. [Welby (1977), pp. 109-110]

Whatever the story actually is with respect to Welby's influence on Peirce, it is clear that he felt her work significantly like his own to wonder about the question. But perhaps the

most important thing about these passages is that the first of them in part and the second of them wholly were published in a section devoted to Peirce in an appendix of Ogden and Richard (1923), *The Meaning of Meaning*.<sup>17</sup>

*The Meaning of Meaning* figures into the present history in a number of ways and I will mention it a number of times later in this chapter. I mention it now with respect to Welby and her influence because it is one of the few places where references to Welby have been readily available over the past 100 years and because it is such a widely read book even to this day.<sup>18, 19</sup> Besides the references to Welby in the section on Peirce just mentioned, there are three other places in *The Meaning of Meaning* where she is cited. The authors acknowledge Welby in their preface<sup>20</sup>, she is later cited as one of the earliest to raise the question of the nature of meaning<sup>21</sup>, and finally she is mentioned with respect to her view about the identity of meaning and speaker's intention in a section devoted to criticism of just that sort of view.<sup>22</sup>

I will return here and there as I go on to some thoughts regarding Welby and her influence, but I will now leave the part of my discussion devoted to her and move to the next part of my history.<sup>23</sup>

### **2.3 Ferdinand de Saussure**

Although Saussure was not specifically concerned with the theory of meaning in itself, his thought does play an important role in the present history. This is chiefly because of his famous distinction between speech (or speaking) and language<sup>24</sup> which I would like to focus on in this section.

Saussure, in the posthumous *Course in General Linguistics* (Saussure (1916)), takes the subject matter of linguistics to be *language* and tried to clarify this notion and separate it from the notion of *speech*. A language is taken to be a *system of signs*, where a sign is a pair of a psychological conception of a sound (or a "sound-image") and a meaning:

[Language] is a system of signs in which the only essential thing is the union of meanings and sound-images, and in which both parts of the sign are psychological. [p. 15]<sup>25</sup>

A meaning for Saussure is a *concept* or an *idea*:

The linguistic sign unites, not a thing and a name, but a concept and a sound-image. [p. 66]

But for Saussure *concepts* are not entities, either platonic or psychological, that exist prior to language:

Without language, thought is a vague, uncharted nebula. There are no pre-existing ideas, and nothing is distinct before the appearance of language. [p. 112]

And the establishment of a pairing of a sound-image with a concept can only be done holistically, that is, in conjunction with the pairing of many other sound-images and concepts. This holism, as I am calling it, is what motivates Saussure to speak of language as a *system* and not merely a *set* of pairs of signs. Setting up such a system is something that requires a linguistic commun-ity:

...the social fact alone can create a linguistic system. The community is necessary if values that owe their existence solely to usage and general acceptance are to be set up; by himself the individual is incapable of fixing a single value. [p. 113]

Saussure thinks it very important to stress that language has its roots not only in

the speech practices of a community - to this point I will return presently - but also, somehow in the brains of language users:

Language is concrete, no less so than speaking; and this is a help in our study of it. Linguistic signs, though basically psychological, are not abstractions; associations which bear the stamp of collective approval - and which added together constitute language - are realities that have their seat in the brain. [p. 15]

The first important thing to note about Saussure, then, is that he provides a notion in which linguistic expressions are paired in a naturalistic way, somehow, with meanings. The meanings Saussure has expressions paired with are something like the "central" or "core" meanings that we saw Welby above so critical of. But Saussure does not discuss the details of how meanings get paired, in fact, with expressions except, as we just saw, to suggest that the connection is in some limited sense conventional and holistic.<sup>26</sup> And he says nothing at all about how non-literal usage might make use of *meanings* to do its work.<sup>27</sup> Though Saussure has a place in his thinking for meaning, the focus of his discussions of actual linguistic phenomena is much more on phonological and phonetic<sup>28</sup> detail than on meaning.

A second important theme in Saussure is his understanding of the relation of language to speech. Saussure seems to have looked at the relationship between language and speech in two separate ways that don't always seem consistent. Let me present and discuss what he says on this score.

On the one hand, Saussure seems to subordinate language to speech. By this I mean he tends to speak of language as a by-product of speech behavior.<sup>29</sup> He tells us:

Language is a well-defined object in the heterogeneous mass of speech facts. [p. 14]



And more strongly, he says that

speaking is necessary for the establishment of language, and historically its actuality always comes first. [p. 18]

And Saussure's view of language change also seems to indicate that speech is somehow what language rests on. He claims that "speaking is what causes language to evolve" [p. 19],<sup>30</sup> and he insists that the susceptibility of language to change is extremely great:

Language is radically powerless to defend itself against the forces which from one moment to the next are shifting the relationship between the signified [meaning] and the signifier [sound-image]. [p. 75]

Thus, language changes constantly and these changes are brought about by the behavior of language users:

...in language...everyone participates at all times, and that is why it is constantly being influenced by all. [p. 74]

So an important strand of thought to be found in Saussure's work is that language is founded in the speech behavior of language users:

the faculty of articulating words...is exercised only with the help of the instrument created by a collectivity and provided for its use.... [p. 11]

But, on the other hand, Saussure seems also to have held that speech is not possible without language:

...language is necessary if speaking is to be intelligible and produce its effects.... [p. 18]

And he also seems to hold at times that language is not susceptible to the affects of speech, that is, that language change cannot result from what people do in speech:

The signifier [sound-image], though to all appearances freely chosen with respect to the idea that it represents, is fixed, not free, with respect to the

linguistic community that uses it. The masses have no voice in the matter, and the signifier chosen by language could be replaced by no other. ...No individual, even if he willed it, could modify in any way at all the choice that has been made; and what is more, the community itself cannot control so much as a single word; it is bound to the existing language. [p. 71]

Thus, Saussure appears to hold two general conceptions of the relation between speech and language that don't sit so well together.<sup>31</sup> But this appearance is deceptive. At bottom, I believe, Saussure's view here is quite consistent.

Saussure, in holding apparently contradictory views about language change, is concerned to respect a certain fact about the relation between language and other social institutions. For, language is often understood to be a matter of convention, a certain sort of social institution, among language users. And Saussure often avails himself of this sort of conception of language. For example he tells us in one place that "language is a convention...." [p. 10], and in another that it "exists only by virtue of a sort of contract signed by the members of a community" [p. 14]. But he clearly sees this as a limited conception of what is going on with language.<sup>32</sup> For Saussure thinks that while individuals and groups of individuals might effectively make changes, by agreeing among themselves, to what are properly understood as social institutions, this is not really possible with language. The idea is that while, for example, a congress might meet of the American Scrabble Players and by agreement rule that in official games two letter words will not be allowed, with language there really cannot be an analogous such congress and ruling. This is the sort of thing that I think he means when he says, in the quote just above, that the "signifier [sound-image] ... is fixed, not free, with respect to the linguistic community that uses it" and that "the community itself cannot control so much

as a single word; it is bound to the existing language". Read this way, what Saussure means is not so much that a group is bound to the existing language and that it can't change, but rather, the group is bound to the language *and* whatever changes it should undergo.<sup>33</sup>

It might look like there is still something of a contradiction left over here, even when we read Saussure as I have just suggested. For when he says that "...in language...everyone participates at all times, and that is why it is constantly being influenced by all" [p. 74], he really does seem to have it that language changes rest on changes in speech behavior, but on the reading I just suggested he seems to be saying that neither individuals nor communities can willfully change a language. Why couldn't each individual in a group *decide* to speak a certain way agreed on by all and thereby change a language? The answer is that Saussure seems to see that such a *decision* wouldn't be enough, on its own, to institute a language change: the only thing that could actually bring about a change is an *actual* change of behavior of each member. It is always the *actual* practices of the community that determines the language that is being spoken in that community. Of course, if the *decision* of a group is followed by an actual change in practice in accordance with the decision, then in some sense, the group changed their language *by agreement*. I don't think that Saussure really wants to exclude such a scenario as a *logical* possibility. But I think that as he sees things, any such decisions, if they should actually be attempted in actual speech communities would not, in fact, end up resulting in the intended change in language. Linguistic practice for Saussure has too many unconscious influences for conscious decisions about it to ever be effective

ultimately. This is not an implausible view and it seems to resolve this last apparent contradiction in his views about language change. That Saussure has this view is nicely illustrated by the following passage about the possibility of instituting artificial languages:

Whoever creates a language controls it only so long as it is not in circulation; from the moment when it fulfills its mission and becomes the property of everyone, control is lost. Take Esperanto as an example; if it succeeds, will it escape the inexorable law? Once launched, it is quite likely that Esperanto will enter upon a fully semiological life; it will be transmitted according to laws which have nothing in common with those of its logical creation, and there will be no turning backwards. A man proposing a fixed language that posterity would have to accept for what it is would be like a hen hatching a duck's egg: the language created by him would be borne along, willy-nilly, by the current that engulfs all languages. [p. 76]

Thus, language change for Saussure is inevitable and rests on the *practices* of speakers, but it cannot and will not, in fact, ever rest on the *willed decisions* of speakers.

But there is still an unresolved matter. Even if Saussure's views about language change are consistent in the above way, there is still the apparently contradictory claim, quoted in two separate parts above, that

...language is necessary if speaking is to be intelligible and produce its effects; but speaking is necessary for the establishment of language, and historically its actuality always comes first. [p. 18]

If language is necessary for speech to work, then how can speech have come "historically" before language? If speech could somehow come before language, then language-less speech should be possible. But here too, the contradiction is only apparent.

It might look like the issue at stake here is not as much about *language change* as about the origins of language. But Saussure took a dim view towards wondering about the origins of language:

...the question of the origin of speech is not so important as it is generally assumed to be. The question is not even worth asking; the only real object of linguistics is the normal, regular life of an existing idiom. [pp. 71 - 72]

Saussure's idea that speech is prior historically to language, then, cannot be understood in terms of the absolute origin of language. In fact, the apparent contradiction now under discussion, gets resolved only in noting Saussure's views of *language change* again. Most notable here is Saussure's view that

...everything diachronic in language is diachronic only by virtue of speaking. It is in speaking that the germ of all change is found. Each change is launched by a certain number of individuals before it is accepted for general use. ...But not all innovations of speaking have the same success, and so long as they remain individual, they may be ignored, for we are studying language; they do not enter into our field of observation until the community of speakers has adopted them. [p. 98]

Thus, in speech, an innovation is introduced and if adopted by the speech community generally, it becomes part of language. In this sense speech always precedes language. But in this sense, there is no implication that speech is possible without language.

There is, therefore, no contradiction for Saussure in saying that speech is prior historically to language and that language is required for speech. We still might be puzzled about the ultimate origins of speech, but that doesn't require us to read Saussure as contradicting himself. Saussure's interest does not seem to be in making what we might call the metaphysical relations between speech and language completely clear. Ultimately he seems to leave us with the somewhat tense doctrine that speech and language are metaphysically on a par: neither is more fundamental than the other.

Saussure, then, provides a notion of a language as a system of pairs of "sound-images" and meanings which is founded on the speech practices of a community.

Saussure's view stresses the practices of the community as somehow fundamental to understanding language, as did Welby's. But unlike Welby's view, Saussure posits something like "central meanings" for the expressions of language. Saussure's picture of these "central meanings" might be said to be, from the point of view of concern about meaning generally, uncritical and rather unsophisticated: he says next to nothing at all about the sorts of phenomena that so vexed Welby, phenomena such as the creative use of unconventional means of communication, metaphor, and non-literal, unconventional usage of language generally. But Saussure's innovation is important none-the-less as will become clearer as we see it developed by Alan Gardiner.

#### 2.4 Alan Henderson Gardiner

Sir Alan Henderson Gardiner (1879-1963) was an Oxford Egyptologist and linguist.<sup>34</sup> As with Saussure, a distinction between speech and language is of fundamental importance for Gardiner. But where Saussure's stress is on structural aspects of languages, particularly with respect to the sound systems of languages, Gardiner's is primarily on issues of *meaning* and *communication*:

I am, of course, well aware that there are important aspects of speech and language which I have as good as completely ignored. My interest being primarily semasiological, i.e. concerned with the function of speech as an instrument for conveying meaning, I have paid but small attention to either its sounds or its aesthetic bearings. [p. 13]<sup>35</sup>

There are a number of themes that run through Gardiner's general works on language that I would like to pick out as most important for discussion in the present context. My discussion will take the following path: (1) first I will discuss Gardiner's

sense of the importance of notions of *purposiveness* and *intention* for studies of speech and language, and I will talk about the already mentioned distinction between speech and language, the import of which Gardiner develops even further than Saussure; (2) then, I will focus on Gardiner's view of the nature of language; (3) this leads to a consideration of his notions of word-meaning and of sentence-meaning; (4) this, in turn leads to a presentation of Gardiner's understanding of non-literal uses of language; (5) next, I say a bit more about Gardiner's view of the relation between speech and language; (6) I continue with more discussion of the importance for Gardiner of notions of *communication* and *communicative intention* and some important ways in which he anticipated and perhaps influenced the work of Austin and Grice; and, (7) finally, I offer some summarizing comments about Gardiner's historical role.

1. Gardiner's most important work with respect to the issues of present concern is *The Theory of Speech and Language* (Gardiner (1951)) which was originally published in 1932. He suggests the following definition for *speech* in his first chapter:

As a first approximation let us define speech as the use, between man and man, of articulate sound-signs for the communication of their wishes and their views about things. [p. 18]

And he suggests straight away a certain conception of the origins of speech and language:

We are often warned, and wisely, against basing far-reaching conclusions on theories of origin. These are bound to be conjectural in a high degree, and nowhere more so than in the cases of language and speech. Still, the philologist can barely escape from some working hypothesis regarding the genesis of speech, and it may be well here to point out that its origin cannot be conceived of otherwise than as the result of social conditions. True, the ultimate basis must be the involuntary cry of the individual animal. This was, I suppose, at the outset, little more than the audible result of muscular movements due to the incidence of some external stimulus. The squeal of the trapped rabbit provides the type. But such

emotional monologue is very far removed from speech, nor could any amount of variety either in the stimuli or in the reactions ever have given rise to anything resembling a real language. For the development of a language we are bound to assume a purposeful use of articulate utterances in order to influence the conduct of others. [pp. 18-19]

Here he has not yet actually sharply distinguished between speech and language, but these early passages display nicely how his conceptions of speech and language are both founded in notions of *communication* and *purposiveness*. He takes pains to criticize the view that speech can be rightly understood as the expression of thought.<sup>36</sup> For Gardiner, the primary source of speech is the desire to affect the behavior or beliefs of another. We don't *merely* express thought in speech, for Gardiner, but we always, or fundamentally, express thought in order to affect the behavior or attitudes of others. Thus, speech can be seen as an expression of thought only insofar as it is first seen as a species of purposive behavior, as a means of communication of one type or another. The notions of purposive behavior and communication will be discussed more below, but for now, it is helpful in understanding Gardiner's distinction between speech and language to note that he takes the development of language to be the development of tools for use in speech and that he takes speech essentially to be a matter of one person trying to affect the beliefs and/or behaviors of another.

Gardiner tells us the following of the distinction between language and speech:

We shall see that the impulse to speech, at least in its more fundamental forms, arises in the intention of some member of the community to influence one or more of his fellows in reference to some particular thing. **Speech** is thus a universally exerted activity, having at first definitely utilitarian aims. In describing this activity, we shall discover that it consists in the application of a universally possessed science, namely the science which we call **language**. ...These two human attributes, language the science and speech its active application, have too often been confused



with one another or regarded as identical.... [p. 62]

By characterizing language as a "science", of course, Gardiner is saying that language is a certain body of knowledge:

Language is a collective term, and embraces in its compass all those items of knowledge which enable a speaker to make effective use of word-signs. [p. 88]

Gardiner took the speaker's knowledge as the basis of linguistic theory:

It is my conviction that every adult human being is the living repository of a profound knowledge of language. ...Here, then, existent in the consciousness of everyone, is an immense treasure of evidence available for the construction of a solid fabric of linguistic theory. [p. 5]

2. Of course, to characterize language as a body of knowledge is not yet to tell us what language *is* in itself. In fact Gardiner's view of what language is is very close to Saussure's:

...a physical substitute has to be found whenever anything intellectual or emotional is to be imparted. Such physical substitutes are called **signs**, and are subject to the conditions (1) that they should have a pre-arranged 'meaning', or associated mental equivalence, and (2) that they should be handy objects of sense transferable at will. Any material thing which conforms to these two conditions will serve as a 'sign', and any system of signs is a kind of language. [p. 67]<sup>37</sup>

Thus for both Saussure and Gardiner, language is a *system of signs*. If the two differ in their understanding of language it is with respect to the notion of the meanings of *signs*,<sup>38</sup> and to Gardiner's view of meaning I will return presently. It is important to note though that Gardiner augments the above characterization of language elsewhere in his book and tells us that in addition to words, syntactic rules as well must be considered parts of language:

Words, as the most important constituents of language, may fairly well be

regarded as its units, though it must be borne in mind that the rules for combining words (syntactic rules, as they are called), and the specific types of intonation employed in pronouncing words, are constituents of language as well. [p. 88]

For Gardiner words and syntactic rules, along with their meanings, constitute language.

Sentences have a special status. They are composed of words arranged according to syntactic rules, but they are, for Gardiner, more properly regarded as phenomena of speech than of language. He ends the passage just quoted with the following dictum:

*The sentence is the unit of speech, and the word is the unit of language.*  
[p. 88, his emphasis]

This reflects Gardiner's sense of language being at the service of speech. For in speech a speaker tries to communicate something to an audience and uses whatever might best achieve this end:

Sentences are like *ad hoc* constructions run up for a particular ceremony, constructions which are pulled down and their materials dispersed as soon as their particular purpose has been served. [p. 90]<sup>39</sup>

It is here that Gardiner locates the most important aspects of the distinction between language and speech. The first section of the third chapter of his book is called "The antithesis of 'language' and 'speech'" and there we are told:

The attentive reader will by this time have accustomed himself to think of speech as a form of drama needing a minimum of two actors, a scene or situation of its own, a plot or 'thing meant', and as a last element the extemporized words. Such miniature dramas are going on wherever speech is practiced, and it is little short of a miracle that the authors who deal with linguistic theory seem never to have thought of describing one of them. ...[T]here has been a sort of conspiracy not to isolate or analyse in its entirety a single act of speech.... ...[N]o small part of my purpose will be served if later writers recognize the absolute necessity of examining single acts of speech in their total environment, and if the distinction between language and speech is never again suffered to fall into oblivion. [pp. 106-107]

By "extemporized words" Gardiner is alluding to the use of language: from the words and syntactic structures that constitute language sentences are constructed by speakers to meet the needs she or he has in some given situation. Thus, speech is fundamental for Gardiner: the *act of speech*, it can be claimed, is the central notion in Gardiner's thought. I will return to this emphasis on speech and *acts of speech* below.<sup>40</sup>

3. What Gardiner says about the meanings of signs, or words, is interesting and important. Invariably his comments about meanings aim at the role that meanings play in communication. A good example of this is in the following passage:

...the meaning of a word is not identical with an 'idea' in the Platonic sense. ...[W]ord meanings possess nothing of that self-consistency and homogeneity which are characteristic of 'ideas'. Ideas, if attainable at all, are the result of long and toilsome search on the part of philosophers. The metaphysician may ultimately arrive at an adequate concept of 'Truth', and the physicist may define 'Force' in a way which will stand him in good stead. But these notions are not the word-meanings with which speech operates. If we consult the *Oxford English Dictionary* we shall find the meaning of *truth* set forth under three main heads, each with numerous subdivisions. The applications of the word range from personal faithfulness or loyalty to verified facts or realities. It is not as an 'idea' that the meaning of the word *truth* must be conceived, but rather as an area upon which the various potentialities of application are plotted out. [p. 44]<sup>41</sup>

When Gardiner denies that word-meanings are Platonic 'ideas' the issue at stake for him is not the ontological status of word-meanings, but, rather, something more like their epistemological status. We can take him here as pointing roughly to the idea that the notion of *word-meaning* should be understood in terms of the uses which may be made of a word. He states this a bit more clearly in the following passage:

I believe that all the senses in which linguistic theory must employ the term 'meaning' conceal a similar abbreviation, and that the intending, purposing speaker must always be looked for in the background. Thus

when we allude to the 'meaning' of a word, what is signified is the multitude of ways in which a speaker may, if he will, legitimately employ it. [p. 100]

In another place Gardiner is more specific, if still speaking roughly:

My argument having deprived words of some of their importance by denying the self-sufficiency of their meanings, it will naturally be asked what value I do attribute to them. My answer is that they are primarily instrumental, that their function is to force or cajole the listener into looking at certain things. [p. 33]

...the function of words is to make the listener 'see what is meant'. They are, in fact, 'clues'. The thing-meant is itself never shown, but has to be identified by the listener on the basis of the word-meanings submitted to him for that purpose. [p. 34]

With this talk of 'clues' and the remarks at the end of the quote just before this last one, we can look at Gardiner as taking up the suggestion - I repeat it here - we saw Welby considering, and apparently rejecting, concerning the need for something like central meanings for words:

"It is a condition which circumscribes within more or less vague and shifting limits the divergence of occasional meanings." Would not this repay more extended treatment? *E.g.*, if a general and usual signification is not a meaning, is it ever a *sense*? "In order to fix the occasional meaning of words" we need a clue.... [Welby (1983), pp. 256-257]<sup>42</sup>

And we can also note that where Saussure says little about what role meanings were supposed to play in the use of language, for Gardiner, this role is of central importance. So, with respect to the question of expression-meaning, or the central meaning of expressions, we can look at Gardiner as synthesizing the emphases of Welby and Saussure. Welby strongly and consistently resists central meanings because of her concerns about the flexibility of speech. Saussure embraces central meanings for expressions, but says next to nothing about what use is made of these in speech. Gardiner

places the role of meaning in speech at the center of his characterization of meaning itself. This is a powerful and original move.

Gardiner often repeats that words are 'clues' and his understanding of how sentence interpretation on the part of a listener works, needless to say, makes important use of this notion:

...I showed that the essential method of speech consisted in presenting to the listener successive word-signs each possessing a definite area of meaning. Employing these clues, the listener reconstructs the thing-meant by an effort of his intelligence, using the situation as an additional source of inference. [p. 195]

The use of inference in interpretation is often repeated and stressed:

...the listener's interpretation is always a matter of reasoning. [p. 199]

Thus, the words of a sentence provide 'clues' along with features of the context of the utterance of the sentence and, if all goes well, the audience will deduce from all of this what the speaker's communicative intentions are.<sup>43</sup> The contemporary reader may well think that Gardiner is committed to the view that a language will enjoy a *compositional-meaning theory*, a finitely axiomatizable theory which entails for each sentence of the language a theorem that says what the sentence means.<sup>44</sup> Such a theory is generally understood to require that sentence meanings are entities of some sort. And, in fact, Gardiner seems friendly to the idea that sentence meanings are *things* when he notes that in ordinary talk it is common to speak of a sentence's meaning as a sort of thing:

...there is no reason to deny that a thought about something is in itself a thing. For to describe the reference of a complete sentence as a 'thing' is not only in accord with common linguistic usage, but is also vital and fundamental for a satisfactory theory of speech. English permits us to say: *That Pussy is beautiful is a THING which can be expressed in many different ways.* Or again: *Pussy beautiful? I never heard of such a*

THING! *You said something quite different a few minutes ago.* Commands and questions may also be taken as 'things', e.g. *The question 'how much did he spend?' is a THING you are entitled to ask; The order to start work at 6.0 a.m. was a THING unparalleled....* [sic.] With a little contriving it could be shown that the gist of any sentence could be described as a 'thing' without departing from the general usage of our native tongue and thought. [p. 24]

Gardiner, in fact, says very little about the details of how a listener can infer the meaning of an uttered sentence on the basis of his or her knowledge of the language involved. But this is not because he thought the listener's role unimportant. The speaker for Gardiner always is concerned with what way best to succeed in her or his communicative purposes and this requires a clear understanding of the listener's perspective:

...we must guard against the supposition that the part of the listener is wholly passive. He is a recipient rather than an initiator, no doubt, but the act of understanding is one which demands considerable mental effort. ...[I]n the course of actual speech, the words serve mainly as clues. It is upon the listener that devolves the duty of interpreting those clues.... ...Sometimes the part to be played by the listener greatly transcends the mere effort of comprehension. In questions and commands a definite responsive movement is expected of him. This responsive movement lies, it is true, outside the speaker's own linguistic act, but in a sense it belongs to it, questions and commands being otherwise inexplicable. [pp. 64-65]<sup>45</sup>

4. On the topic of Gardiner's understanding of *meaning*, it should be pointed out that he has some interesting things to say about metaphor and non-literal usage generally. For one thing, as did Welby, Gardiner thought it important to note that individual speakers press conventions into their service and are not simply and wholly bound in speech by the forces of convention:

Words do not all resemble one another. They may be likened to the stones in a builder's yard, of different materials and of different shapes. They have been hewn into diverse shapes for special purposes, some meant for

this position in the building and some for that. In themselves they carry a presumption of their future use, but at the last moment the builder may change his mind, and use a particular stone in a way for which it was not intended. In skilled hands, a stone so employed may perhaps be even more effective than another originally destined for the same place. [p. 175]

In the following passage, Gardiner display an attitude strongly reminiscent of Welby's towards too great a respect for the priorities of conventional mechanisms of speech:

...mention must be made of...the fallacy of the *mot juste*. Some eminent authors have been pleased to toy with the illusion that there is only one correct way of saying a thing, and conversely, that each word has only one correct application. To hold such a view is to affirm the rights of language, but to deny those of speech. Individuality in speaker or writer is seemingly forbidden, and it is difficult to imagine, on this presupposition, how new thoughts could come to expression or old ones take on a new aspect. One of the most precious characteristics of language is its elasticity, which permits speech to stretch a word or construction to suit the momentary fancy or need. [p. 174]

Still, Gardiner understood past usage, and the central meanings that past usage gives rise to, to represent constraints upon the speaker. One is not merely free to use a word to mean whatever. He quotes Raleigh in this regard:

The true position is summed up by Walter Raleigh with great profundity: 'The business of letters, howsoever simple it may seem to those who think truth-telling a gift of nature, is in reality twofold, to find words for a meaning, and to find a meaning for words. Now it is the words which refuse to yield, and now the meaning, so that he who attempts to wed them is at the same time altering his words to suit his meaning and modifying and shaping his meaning to satisfy the requirements of his words.' Without the notion of a give and take between speech and language, linguistic theory is an impossibility. [p. 174]

The constraint that the central meaning of an expression places on its use informs Gardiner's understanding of metaphor:

Metaphor, from the Greek *μεταφορά* 'a transference', signifies, in its technical use, any diversion of words from their literal or central meanings.

[p. 165]<sup>46</sup>

And Gardiner, unlike Saussure, clearly identifies such diversions from central meanings as one of the sources of linguistic change:

The new meaning introduced by speech gradually gains the upper hand over the old meaning imposed by language. If at last the old meaning ceases to be felt when the word is used, incongruent word-function becomes congruent, and metaphor dies. A new word-form or a new word-meaning has become established. Speech has become language. [pp. 165-166]

But Gardiner never attempts a full-scale investigation of metaphor and non-literal usage in every day speech. In the section of his book that he devotes to metaphor he mostly sticks with examples taken from literature. Still his inclusion of the topic marks something, again, of a synthesis of Welby's views and Saussure's. For Welby, non-literal usage appears to threaten the very notion of literal meaning, whereas for Saussure, though he has something of a notion of literal meaning, the use of literal meanings to non-literal ends is never really discussed. Gardiner is able to maintain literal meanings as well as to accommodate the existence of non-literal usage.

5. Gardiner understands the relationship between language and speech in a somewhat novel way. Language "enters into speech", for Gardiner, but it is also the "product of speech" [p. 110]. Part of Gardiner's understanding of language's being a product of speech seems definitely informed by Saussure's sense of the sources of linguistic change:

Every change in language, conscious or unconscious, great or small, whether of pronunciation or of meaning, has its origin in some single act of speech, hence passing, if it find favour with the multitude, from mouth to mouth, until at last it becomes common property. [pp. 111-112]



And he ends the section from which the above quote is taken with the words:

There is no need for lengthier insistence on the universally recognized truth that language is, and can only be, the outcome of countless single examples of speech. [p. 112].<sup>47</sup>

It was noted above that although Saussure seems to claim that language rests on speech, he does not take this relationship to be a wholly asymmetric affair. Speech, for Saussure, seems always itself to require language. We also saw that Saussure cannot be pressed on this matter with respect to the question of the origin of the use of language because he has little respect for that sort of question. Gardiner, though he shares with Saussure in much of the above discussed thought, does differ from him in a few crucial respects.

For one thing, as was seen at the very beginning of this section, Gardiner, though cautious about speculations about the origins of speech and language, believes that some view about these origins is necessary in theorizing about speech and language. And in his comments about the origins of speech and language, he seems to allow that it is conceptually necessary for communication to be possible without pre-established conventions of any sort. And this is a second and crucial point of difference between Saussure and Gardiner concerning the relationship between speech and language. Gardiner does seem to recognize, even independently of questions of the origin of speech and language, the possibility of language-less speech and he seems to see this as the foundation of all language:

It is highly important to realize that all linguistic form arouses an expectation of use. The reason is that language is only a name for established habits of speech, built up out of innumerable repeated acts of the same type. [p. 138]

Thus, Gardiner, it seems, somewhat more than Saussure, provides a sort of metaphysical primacy to speech over language. Language is something like the sum-total of speech regularities in the community, and this seems to require for Gardiner the conceptual possibility of language-less speech.<sup>48</sup>

6. Besides the distinction between language and speech, the single most important aspect of Gardiner's work with respect to the present history is his constant, relentless insistence on the primacy of notions of communication and the communicative intentions of speakers in the study of speech and language. This has already been seen throughout the above discussion, but even a cursory glance through *The Theory of Speech and Language* will be enough to convince one of how important he thought the notion of a speaker's intention is to linguistics. Without an act being intentional, it cannot be speech. And crucially, speaker's intentions in speech are first and foremost intentions with respect to an audience in order to affect their behavior or attitudes in some way.<sup>49</sup> Speech is always, thus, audience-directed intentional behavior. The notion of a sentence, which it will be recalled Gardiner takes as the unit of speech, is importantly defined for Gardiner in terms of the intention of a speaker:

...the title of 'sentence' being reserved for those single words or combinations of words which...give satisfaction by shadowing forth the intelligible purpose of a speaker. It is only when, in a given situation, a word or words betray such a purpose, seem fired or galvanized by some reasonable communicative intent, that the dignity of sentence-rank can be conferred upon them. [p. 182]

Gardiner is sensitive to the variety of reasons a speaker might have for producing a sentence. In the following passage we hear of this variety in a way that might call to mind the writing of J. L. Austin:

On a broad survey there might seem hardly any limit to the variety of purposes with which a sentence can be uttered. Sometimes a speaker makes an affirmation with intent to persuade, protest, or even deceive; sometimes he may give a description for his own amusement or for that of his audience; or again, he may speak merely for the sake of speaking. His sentences may be aspirations, prayers, promises, threats, judicial verdicts, sarcasms, witticisms, sneers, teasings, exhortations, complaints, flatteries, and much else. [p. 186]

But Gardiner does not think that it is the job of the linguist or grammarian to give an account of all of these sorts of purposes. The chief aim of the linguist is with those purposes of speech that are reflected in the formal structure of sentences and not all of the purposes of the above list are so reflected [pp 186-187]. Ultimately Gardiner settles on classification of sentences into four sorts: (1) statements, (2) requests (or commands), (3) questions, and (4) exclamations [p 189].

Gardiner's view seems to be that the variety of purposes for which speech can be employed can be achieved by the production of sentences of these four sorts. He thinks questions and requests, for example, are directly tied to the intention on the part of the speaker to elicit from an audience information or action, respectively:

In requests some specifically named action is demanded by the speaker, whereas in questions a relevant verbal response is desired. [p. 303]

And of exclamations he tells us:

The essence of exclamations is that, whether by way of description or only through implication, they emphasize to the listener some mood, attitude, or desire of the speaker, in extreme cases to the exclusion of all else. [p. 315]

But Gardiner is more cautious when it comes to speaking of the purposes to which statements may be put by speakers. He feels that there are just too many sorts of such purposes:

...the purposes with which this sentence-form [the statement] is used are less obvious than in the other classes of sentence. Indeed, those purposes are exceedingly various.... [p. 302]

But still, he does seem to focus, in his discussion of the statement, on its use for the purpose of providing an audience with information of some sort or another, that is, with its use to make assertions:

We now see wherein the peculiarity of statements lies; they predicate something of something. Or to use the term customary in this connexion, statements 'assert'. [p. 295]

He defines *assertion* in the following way:

...'assertion', the definition of which is contained in the following formula:  
*All statements assert, i.e. present their predicate either as true or else as false of the thing denoted by their subject.* [p. 299, emphasis Gardiner's]<sup>50</sup>

Gardiner distinguishes between what he calls the *locutional*- and the *elocutional*-forms of sentences. The *locutional-form* of a sentence is determined largely by the words and syntactic structures that are employed. The *elocutional-form* of a sentence is determined largely by the intonation with which it is spoken or the gestures used when it is uttered [pp. 200ff.]. There can be sentences that have no locutional-form because they use no words, but that do have elocutional-form:

...I...mention...a mode of speech which, paradoxical as it may seem, takes the further step of dispensing with words altogether. Questions may be answered with a nod or a shake of the head, and unpleasant subjects dismissed with a shrug of the shoulders. If these acts are not speech, I do not know where to place them; and it should be observed that the communicative means they employ are good elocutional form. [pp. 203-204]

Elocutional-form, Gardiner tells us, "provides the dominant clue to the special quality of the sentence" [p. 205].<sup>51</sup> Thus, the elocutionary-form of a sentence - the tone of voice,

the gestures accompanying an utterance, etc. - is the main indication provided by a speaker as to what general sort of sentence is being uttered. This aspect of sentences is extremely important for Gardiner since knowing the sort of sentence that is being uttered is crucial to deciphering what the overall intention of the speaker was in uttering the sentence:

...from the speaker's point of view it is desirable that his listener should know precisely what aim or intention he had in mind. In fact, it is not enough for an utterance to have recognizable function as a sentence, as the vehicle of the general purpose to make a communication; it must somehow reveal or hint at the special purpose entertained by the speaker. [p. 186]

Thus, Gardiner sees it as an essential feature of an *act of speech* that the speaker intend her or his communicative intention to be recognized. That it should proceed via a speaker's intention to have her or his communicative intentions recognized is, crucially, what Grice later was to indicate as essential to communication. It is also directly related to Austin's notion of *securing of uptake*, at least as Strawson glossed the latter notion in his classic paper.<sup>52</sup> But the idea seems clearly anticipated here in Gardiner's writing.<sup>53</sup>

Gardiner did not think that elocutional-form alone had to be responsible for indicating what sort of sentence a speaker intended to utter. He drew a distinction between what is *described* in a sentence and what is *implied* by it:

Words and sentences not only have immediate reference, resulting from intentionally directed meaning, but they also have 'form', a method of conveying knowledge by a sort of overtone, less well characterized by the term 'description' than by the term **implication**. Speech achieves its ends partly by describing, partly by implying. [pp. 195-196]<sup>54</sup>

Thus, though the special-sentence quality of an utterance is generally, as has been mentioned, indicated by the elocutional-form of the utterance, it can also be indicated by

*description*. This amounts to what in post-Austinian terminology might be called the use of an *explicit performative*. Gardiner tells us:

...*I ask you, did you give that poor beggar anything?* is at least primarily two sentences, of which the first does not tell us that it states, nor the latter that it asks a question. [p. 191]

The first sentence here is the one that says "I ask you": this states that the speaker is asking something, but it doesn't state that it so states. The second sentence is the question, "did you give the poor beggar anything?" This does not *state* anything about its special sentence-quality at all. Gardiner thinks interesting problems arise in connection with the use of explicit statements within sentences which indicate what the special sentence-quality of the utterance is intended to be. But it is interesting that he discusses this sort of example.<sup>55</sup>

7. Though Gardiner's thought on language was rich and probably widely influential,<sup>56</sup> it is hard to find many references to him in the philosophical literature where this would be expected to be most appropriate. But some references can be found.

Gilbert Ryle opens his paper "Use, Usage and Meaning" with the following passage:

In 1932 Mr. (now Sir) Alan H. Gardiner published *The Theory of Speech and Language* (Clarendon Press). A central theme of his book was what, with some acknowledged verbal artificiality, he labelled the distinction between 'Language' and 'Speech'.<sup>57</sup>

Mats Furberg strongly suggests that Austin was influenced by Gardiner. In the penultimate paragraph of a section devoted to removing the common impression that Austin was either a Wittgensteinian or importantly influenced by Wittgenstein, Furberg makes the following remarks:

...the philologist Alan H. Gardiner gave a first sketch of the distinction between locutionary and illocutionary aspects of a speech-act. (See Part II of *A* [sic.] *Theory of Speech and Language*.) His remarks on 'sentence-qualifacators' (§§ 61 and 68) are clearly relevant to such philosophical puzzles as why it is odd to say 'S is P, but I don't believe it'. [Furberg (1971), p. 52]

Thus, Furberg acknowledges Gardiner as an originator of the important distinction between locutionary and illocutionary speech-acts before Austin. And more importantly, his point in making the acknowledgments in this passage is to show that we need not believe Austin was influenced at all by Wittgenstein, that is, that we do better to understand that Austin was influenced by Gardiner (and others who Furberg mentions, but are irrelevant here). Immediately following the above quoted passage in Furberg is the claim:

There is, then, no need and no reason to suppose that Austin formed his basic ideas under Wittgenstein's influence. [Furberg (1971), p. 52]

I cannot assess Furberg's claims about Wittgenstein's influence on Austin. But his assessment of Gardiner and his influence on Austin is plausible enough. Unfortunately Furberg says little else about this influence. He only cites a single place where Austin refers to Gardiner.<sup>58</sup> This, of course, is nice since it shows that at least Austin definitely was aware of Gardiner. But it would be nice if there was more to go on here.

Edmond Wright makes the following interesting sequence of claims:

Sir Alan Gardiner was perhaps the first to elucidate the sign situation as essentially concerning the take-up of the speaker's intentions by the hearer:

The thing meant by any utterance is whatever the speaker has intended to be understood from it by the listener.

but it had indirectly been glanced at over a century before by the rhetorician George Campbell:

Eloquence is the art whereby discourse is adapted to produce the effect which the speaker intends it should produce in the hearer.

Recently H. P. Grice has been notable in developing this approach theoretically, but without making reference to his predecessors.<sup>59</sup>

But Wright doesn't indicate whether he thinks Grice was actually influenced by Campbell or Gardiner.

John Lyons notes a thematic relation between Gardiner and Grice in the following passage:

...successful communication depends, not only upon the receiver's reception of the signal and his appreciation of the fact that it is intended for him rather than for another, but also upon his recognition of the sender's communicative intention and upon his making an appropriate behavioural or cognitive response to it. This has long been a commonplace of non-philosophical treatments of meaning and communication (e.g., Gardiner, 1932); and it has been forcefully argued more recently, from a philosophical point of view, by such writers as Grice (1957) and Strawson (1964).<sup>60</sup>

I am not sure whether it is correct or not to take as a commonplace what Lyons suggests here: just above I quote Wright claiming that Gardiner was "perhaps the first to elucidate the sign situation as essentially concerning the take-up of the speaker's intentions by the hearer". But, however things stand in that regard, it is interesting to see Lyons noting Gardiner's anticipation of this important aspect of Oxford philosophy of language.

The most important reference to Alan Gardiner however, for the present study, is to be found in Ogden and Richards's *The Meaning of Meaning*. It was already noted above that Ogden and Richards discuss the view in which there is a close conceptual link between notions of *meaning* and notions of *intention*. And we saw that they cite Welby in a footnote to this discussion as a holder of this view. On the same page as the Welby



citation they begin a quotation from an earlier work of Gardiner's which ends with the formulation:

*The meaning of any sentence is what the speaker intends to be understood from it by the listener.*<sup>61</sup>

Ogden and Richards criticize Gardiner and the attitude towards meaning they take him and Welby to stand for. Though they later admit the speaker's intention to affect an audience as one of five functions of language they identify (Ogden and Richards (1923), p. 230), they clearly seem to have a strong opposition to the view:

'Mean' as shorthand for 'intend to refer to,' is, in fact, one of the unluckiest symbolic devices possible.<sup>62</sup>

I will leave off my discussion of Gardiner on this note, but he will reappear when I come to speak of Grice in section 2.6.

## 2.5 Causal Theories of Meaning

Up until this point I have mainly focused on a little noticed, but, I believe, very important strand of thought in the history of the theory of meaning. The main theme of this strand is its stress on the intentions and practices of speakers. We might call this strand the *intention-based* tradition of the history of the theory of meaning. If a history tried to trace concern for meaning back further than the twentieth century it might be that important aspects of the intention-based tradition would be found not only in philosophers who wrote on speech and language, but also in the writings of theorists of rhetoric.<sup>63</sup> Whatever its pre-twentieth century history, however, I would like to locate Welby and Gardiner as two central thinkers in this tradition in the twentieth century.<sup>64</sup>

But there is another strand of thought about meaning that runs alongside the intention-based tradition. This is the *causal* tradition. Causal theories are not necessarily conceptually opposed to intention-based theories.<sup>65</sup> But in general causal theorists have not tried to discuss meaning in terms of the intentions of speakers and intention-based theorists have not tried to discuss meaning in terms of causal notions. I will discuss this more below, but the deeper conceptual relations between the general types of theories associated with these two traditions will have to be left for a more comprehensive study.

Probably the most important causal theories in the twentieth century are due to Peirce<sup>66</sup>, Russell<sup>67</sup>, Ogden and Richards<sup>68</sup>, C. L. Stevenson<sup>69</sup>, and Charles Morris<sup>70, 71</sup>. I cannot discuss each, or even any, of these theories in great detail here. But since causal theories are far better known to English-language philosophy than the pre-Gricean intention-based views that I have thus far been discussing, I won't feel too bad about my brevity and, perhaps, gruffness here.

Causal theories of meaning generally explain the meaning of public-language expressions in terms of two separate types of causal relation. The first is a causal relation between a public-language expression itself and some mental state and the second is a causal relation between the mental state and the *meaning* of the public-language expression.<sup>72</sup> So, for example, a causal theorist might say something like, "Well, the English word 'snow' means snow because 'snow' tends to cause a certain tendency to behavior in English speakers, and this same tendency to behavior is in important respects just the tendency that snow itself would tend to cause in them."<sup>73, 74</sup>

Russell, to take a more concrete example, in his earliest theories<sup>75</sup> argued for a

theory in which public-language expressions caused mental-images in the minds of language users. Roughly, Russell's theory had it that a public-language expression tends to cause a certain mental image in a person, and that this mental image is, in relevant respects anyway, the same as the mental image that the meaning of the public-language expression would cause were the person to perceive the meaning of the public-language expression directly.<sup>76</sup> Thus, "snow" will tend to cause a mental image of snow somehow, and this mental image, it turns out, is a mental image *of snow*, precisely because it is the same image - in certain relevant respects - as snow itself would tend to cause were the snow to be perceived directly.

The gross differences between different causal theories are always about what sort of mental state gets caused by a public-language expression and what relation the adduced mental-state stands in to the meaning of the public-language expression. In Peirce's theory, a public-language expression causes what he called *an interpretant*<sup>77</sup>; in Russell's early theories, a mental image gets caused, as was just noted; for Ogden and Richards, it is what they call an *engram*<sup>78</sup>; for Stevenson, it is *cognitive mental processes* that get produced<sup>79</sup>; and for Morris *tendencies*, or *dispositions*, to behavior are what get caused<sup>80</sup>. But the sorts of problems that philosophers have had with causal theories of meaning have generally had little to do with the details of what sort of mental state gets caused by public-language expressions.<sup>81</sup>

The most important thing about causal theories for the present discussion is that they are, just like intention-based theories, *psychological theories*. Causal theories locate public-language expression meaning in the relation that holds between the expressions and

the psychologies of their users. Indeed, it is more-or-less the case that if it turned out that a speaker's intention to produce a certain cognitive effect in an audience tended to produce that effect, and if public-language expressions had no relevant causal efficacy in the absence of speakers' intentions to produce certain cognitive effects in audiences then, an intention-based theory would be equivalent to a causal theory. And in more recent developments in the theory of meaning, it happens to turn out that the intention-based tradition and the causal tradition dovetail one another.

## 2.6 Grice

Grice's contribution to the theory of meaning has already been glossed, at least roughly, above in chapter 1. But there still remains the question which I raised at the beginning of this chapter as to who Grice might have had in mind when he suggested that his program for the theory of meaning was a matter of controversy. A good method for trying to answer this question would be to examine every place where intention-based theories are discussed, and to give special attention to all of those places where both intention-based theories and causal theories are discussed. It would, needless to say, be hard to know that one had ever successfully looked at every such place. But I have done my best and the only places in the twentieth-century literature I have been able to identify in which intention-based theories are discussed before Grice's work are these: Welby's work, Gardiner's work, and Ogden and Richards's *The Meaning of Meaning*. That's it. Perhaps I haven't looked enough, but I have looked pretty hard. And the only place I have been able to find both intention-based theories and causal theories discussed in the

twentieth-century before Grice's "Meaning" is *The Meaning of Meaning*.

So, here is a hypothesis: Grice read *The Meaning of Meaning* and saw that its authors saw intention-based theories as problematic while themselves offering a causal theory, and that is why he took his program to be controversial.

This is not at all an implausible hypothesis, I think, since *The Meaning of Meaning* has been a widely read book from the time of its publication until today. And if the hypothesis is correct, it gives all the more plausibility to the view that I have been suggesting throughout this chapter that there is an identifiable tradition of *intention-based* thinking about meaning in the twentieth-century that begins with Welby, and runs through Gardiner to Grice. And if I am right about Gardiner's influence on Austin, then this tradition can be said to be responsible for an awful lot of what has been important in twentieth-century theorizing about meaning.

Of course, the idea of there being an intention-based tradition is by no means false if it turns out that Grice never read or was influenced by *The Meaning of Meaning*. But the story that includes Grice as somehow influenced by Welby and Gardiner is a much more interesting one.

If Grice did read *The Meaning of Meaning*, then all the more is there reason to give to Welby a place of importance in twentieth-century philosophy. For she must be identified as the originator in twentieth-century philosophy of the idea of seeing meaning as identifiable with a speaker's intention to affect an audience, the idea that Grice would make the corner-stone of his conception of meaning.<sup>82</sup> And likewise, Gardiner should all the more be acknowledged as the important thinker he clearly was. Of course, in my

view, both Welby and Gardiner should be much more recognized than they are whether or not Grice read *The Meaning of Meaning*. But if Grice plausibly read that work and was influenced in working on intention-based theories in part because of it, then clearly Welby and Gardiner deserve credit as originators of the sort of theory that Grice was later to make such ingenious and influential contributions to.<sup>83, 84</sup>

## **2.7 The Usual Conception Today of the History of "The Theory of Meaning"**

Finally, I will say a few words about the usual rough picture that people have when you say the words "theory of meaning" near them. This is the conception of the history of the theory of meaning that I mentioned at the beginning of this chapter which begins with Frege and runs through Russell and Wittgenstein, to Tarski, Carnap, Church, and onwards. The story that I have just told seems to have little to do with that common conception. For one thing, I didn't say anything about Frege at all in the story so far. Let me say a bit about Frege now and his relation to the theory of meaning.

So far as I know Frege never wrote a word directly about the theory of meaning in the sense of that expression with which this dissertation is concerned and to which the history in the present chapter has been devoted. The theory of meaning, in the sense in which I am interested in it, is a psychological theory that says what features of a population's use of a public-language expression determine the meaning, for them, of the expression. But Frege seems not to have been directly concerned with the theory of meaning in this sense. Frege was a logician. His interest in meaning extended as far as logic allowed it to. That means that for the most part Frege was concerned with the

*logical form* of sentences, that is, with how the truth conditions of sentences can be determined from the denotations of their terms along with conditions on the syntactic structures that make them up. Logicians have traditionally done such work in investigating the logical form of sentences without having or being interested in precise psychological theories that explain why for a group of language users the simple expressions and syntactic structures contribute in just the way that they do to the truth-conditions of the sentences in which they occur. Frege seems not to have been an exception.

On this score, consider the following passage from Carnap's *Aufbau* in which Carnap is happy to understand the legitimacy of investigating the purely compositional aspects of logical form, but in which he also fails to see the sense of asking for the psychological conditions of meaning:

Let us consider the *designation relation* as it holds between written words and their meanings. Since natural languages do not have general rules which allow us to deduce the meaning of a word from its form, there is no way of indicating the extension of this relation except by enumeration of all its member pairs. If a basic language is already known, then this is done through a dictionary; otherwise, the answer takes on the form, for example, of a botanical garden, that is, a collection of objects, each of which has its name written on it. If the meanings of the *words* are known, then the answer to the correlation problem of the designation relation for *sentences* can be solved through a general function, which however, is usually very complicated. It is the syntax of the language in question cast in the form of a meaning rule. A meaning rule may (in an elementary case) have the following form: if a sentence consists of three words, a noun in the nominative case; a verb in the third person singular, present tense, active mood; and a noun in the accusative case, then it designates the state of affairs that the object of which the first word is the sign stands to the object of which the third word is the sign in the relation of which the verb is the sign.

From the correlation problem, we distinguish the *essence problem*. Here we do not simply ask between what objects the relation obtains, but

what it is between the correlated objects, by virtue of which they are connected. The question does not ask for the constitution of the related object, but asks for the *essence* of the relation itself. Later on,...we shall indicate the difference between science and metaphysics..., and we shall see that the essence problems belong to *metaphysics*....<sup>85</sup>

This is a striking example of a logically astute philosopher understanding the nature of the question of the logical form of a sentence - for the description that he gives of how to solve the so-called *correlation problem* is really a sketch of how to describe the logical forms of sentences (see just below on *logical form* and *compositional-semantics*) -, but failing to understand the nature of the psychological grounds for the correlations between sentences and their meanings. I think this shows that the project of doing logic for natural language *has* been taken as independent of the project of providing a theory of meaning (in the sense of this dissertation).

It can be argued that Frege still indirectly had an interest in the theory of meaning. For, the argument goes, Frege in being interested in discovering the logical forms of all sentences of natural language implicitly took *knowledge* of logical forms by language users to be the basis of the meaning facts that hold among them. Thus, "la neige est blanche" means among the French that snow is white because the French know something about the logical form of this sentence, that is, they know how to determine the truth conditions of this sentence from knowledge that they have of the denotations of its terms and of conditions on its syntactic structures. Thus, the theory of meaning implicit in Frege, according to this argument is something like the following: a sentence means what it does among the members of a population just in case each member of the population knows the logical form of the sentence. To put this in somewhat more contemporary



terms, the theory is, roughly, that a sentence means what it does among a group of people because each member of the group stands in a specific relation to a *compositional-truth theory* for a language that contains that sentence.<sup>86</sup>

This is indeed, at least in outline, a theory of meaning in the sense this chapter has been concerned with. And perhaps Frege did implicitly hold such a theory. But I have not been able to locate any place in Frege's writings where he explicitly defends such a claim, nor have I been able to locate a place in which he even directly raises issues that would motivate such a view.<sup>87</sup> Frege's ideas, with important additions from Tarski, however, certainly were influential to theorists of meaning from Davidson on who do explicitly argue for the sort of theory of meaning in question here. We might identify, in fact, a third tradition in the history of twentieth-century theorizing about meaning as the *compositional-semantics tradition* which, arguably, really explicitly begins with Davidson in 1967.<sup>88</sup> And Frege should be given credit for originating central aspects of this tradition. For that reason Frege surely deserves to be mentioned in this history, even if he didn't directly concern himself with attempts to give an account of the actual psychological grounds of meaning.<sup>89</sup>

I think that much of the interest in semantics and logic in the twentieth century has not been due to interest in the theory of meaning. Indeed, I don't think the two interests explicitly met until the 1960's with Davidson. So it is appropriate that I place this discussion of Frege and the tradition in the theory of meaning that he had most to do with here, after already having discussed the *intention-based* and *causal* traditions in the theory of meaning which clearly did explicitly float around before the 1960's. So this

accounts for my neglect of Frege and the tradition of formal semantics up till now in this chapter.

I still have not mentioned Wittgenstein.

Wittgenstein is an odd figure for this history. It is common to take Wittgenstein as having been an adherent to a certain sort of *theory of meaning* in the *Tractatus*, but then to have refuted that theory, and perhaps every other possible theory of meaning in his later work. I think this is a somewhat distorted understanding of what Wittgenstein actually did. I will very briefly comment on both aspects of this view.

Wittgenstein during his early period did seem to have interest to some extent in something like the idea that languages have compositional-semantic theories. This is shown, in part, by his interest in philosophical analysis which he took to result, ideally anyway, in the discovery of the logical forms of propositions.<sup>90</sup> But it is also shown by the following passage:

To understand a proposition means to know what is the case if it is true.

(One can understand it, therefore, without knowing whether it is true.)

It is understood by anyone who understands its constituents.  
[4.024]<sup>91</sup>

These considerations might lead one to assimilate Wittgenstein to the view that I above noted that some seem to believe Frege to have implicitly held, namely, the view that a sentence means what it does, roughly, because speakers who use it are related in a certain way to a compositional-semantic theory of a language to which the sentence belongs.<sup>92</sup> So, the early Wittgenstein might be assimilated to the *compositional-semantics* tradition somewhat as Frege was. But as with Frege, Wittgenstein never directly raises the

question of the theory of meaning nor does he in any obvious manner raise the issues that would motivate such a theory. He does at one place in the *Tractatus* suggest that everyday language is based on conventions, but he nowhere really brings up the question of *how* the meanings of expressions are related to the conventional practices and attitudes of the people who use the expressions.<sup>93</sup>

Also, Russell, it seems, understood Wittgenstein in the *Tractatus* as engaged in a project different from that of providing a theory of the psychological grounds of expression-meaning. This is evidenced in Russell's introduction to the *Tractatus*:

There are various problems as regards language. First, there is the problem what actually occurs in our minds when we use language with the intention of meaning something by it; this problem belongs to psychology. Secondly, there is the problem as to what is the relation subsisting between thoughts, words, or sentences, and that which they refer to or mean; this problem belongs to epistemology. Thirdly, there is the problem of using sentences so as to convey truth rather than falsehood; this belongs to the special sciences dealing with the subject-matter of the sentences in question. Fourthly, there is the question: what relation must one fact (such as a sentence) have to another in order to be *capable* of being a symbol for that other? This is a logical question, and is the one with which Mr Wittgenstein is concerned. [Russell (1922), p. ix]<sup>94</sup>

Russell when he wrote the "Introduction" had already come to understand the notion *meaning* in psychological terms. So his looking at Wittgenstein's views here as *logical* and not *psychological* amounts, it would seem, to his seeing Wittgenstein as *not* engaged in the theory of meaning, in the sense of this term as used in this dissertation at least.<sup>95</sup> So, there is some reason, at least, to *not* assimilate early Wittgenstein to the *compositional-semantic* tradition that I identified above.

This leaves the question open how to take Wittgenstein's picture theory, of course. But this cannot be investigated any further here.

So, either the early Wittgenstein should be taken, like Frege, as implicitly involved in the compositional-semantics tradition, or he should be taken as involved in a project other than the discovery of a theory of meaning, in the sense in which I use this term.<sup>96</sup>

Of course, the complicated question now arises about the relation of the work of the later Wittgenstein to the theory of meaning. There are too many aspects of this question to deal with here. How did *he* really understand his earlier work? Had his view of what he was doing in his earlier work changed? And what was he really doing in the later work? I can't here begin to investigate these questions. So I will just make a few remarks about what Wittgenstein seems to have done in his later work.<sup>97</sup>

One does get the impression that Wittgenstein felt that theories of meaning are generally not going to be possible. It is hard to assess his reasoning on this score, but two things stick out.

First, Wittgenstein seems to have felt that the idea of there being a reductive analysis of meaning-concepts was problematic:

The mistake is to say that there is anything that meaning something consists in.<sup>98</sup>

This suspicion of analysis probably resulted from a sense that there was something wrong with analysis generally<sup>99</sup> as well as from a deep anti-psychologism of sorts that runs throughout Wittgenstein's later works.

This latter point leads to the second thing that sticks out concerning Wittgenstein's reasoning about theories of meaning. Wittgenstein speaks at length against at least two sorts of psychological views of meaning, one in which meanings are mental images<sup>100</sup>, and the other in which a person meaning something is a sort of mental *activity*.<sup>101</sup>

So, the general feeling that one gets from Wittgenstein is that he felt that we do something wrong if we look for a psychological explication of the notion of meaning, that is, if we look for a theory of meaning. And it seems that this view has been pretty influential since the 1950's.

## 2.8 Summary

I suggested that Grice gave reason to suspect that he took himself as engaging in a debate between causal theorists and intention-based theorists of meaning. But Grice didn't provide any clear suggestion about who the parties to this debate might have been. I identified Victoria Welby as the originator of intention-based theorizing in the twentieth-century. I noted a number of important aspects of her work including suspicion of a notion of what I called *central meaning*. Ferdinand de Saussure, I suggested, embraced a notion of central meaning in his work and used this notion to define a notion of *language* which he importantly distinguished from the notion of *speech*. With the speech/language distinction and with central meanings, the way is paved for asking the question, How do facts about central meanings - which constitute *language* -, rest on facts about the *speech* practices of the members of a group of people? This question essentially asks for a theory of meaning, in my favorite sense of that term. So, Saussure prepared the way conceptually for asking the central question of the theory of meaning. But Saussure's views generally about the relation of speech to language seemed underdeveloped since he seemed to have so little to say about issues like metaphor and non-literal usage. Alan Gardiner, I suggested, synthesized important issues in Welby and

Saussure - whether he did so consciously or not - by developing further the speech/language distinction with a special emphasis on the non-linguistic intentions of speakers in speech. Gardiner developed a number of important themes which were widely influential later, though he was little noted, including something like the notion of the illocutionary force of an utterance and its relation to speaker's intentions, as well as the notion that a necessary condition for an *act of speech* - as he called it - is that the speaker should intend her or his audience to recognize her or his communicative intention in making the utterance made. This latter notion, I noted, is importantly related both to Austin's notion of *securing of uptake* and to Grice's analysis of the concept of *speaker-meaning*. I pointed out that Welby and Gardiner could be seen as taking part in a certain tradition in twentieth-century theorizing about meaning which I called the *intention-based tradition*. I then discussed a second tradition which I called the *causal tradition*. I noted the chief theorists in this tradition and I gave something of a gloss of it. I next returned to the question of where Grice might have come to learn of a debate between intention-based theorists and causal theorists. I suggested that it was through Ogden and Richards 1923 book *The Meaning of Meaning* which is the only place before Grice that I have been able to find a discussion of theories of both traditions. Finally, I discussed a few philosophers who are commonly associated with the expression "theory of meaning", most notably Frege and Wittgenstein. I identified a third tradition in the history of the theory of meaning which I called the *compositional-semantics tradition*. This tradition, I believe, really begins with Davidson in 1967. If Frege and Wittgenstein (and Carnap and Tarski and others) belong somehow to this tradition, I argued, it is not because they explicitly

raised the question of the theory of meaning and tried to solve it by reference to what are known as *compositional-semantic theories*, but rather because they *implicitly* worked from such a perspective. I didn't spend time assessing the historical plausibility of the view, but I noted it as a view that had at least some *prima facie* merit. I also discussed some of what appear to be the later Wittgenstein's ideas on the notion of a theory of meaning.

I have not really discussed, however, work since the 1950's in the intention-based tradition. Such a discussion would include mention of the works of David Lewis, Stephen Schiffer, and Jonathan Bennett among a number of others (see my first chapter above). Since this dissertation is really a response to this work, I won't discuss it here, but merely mention it.<sup>102</sup>

### Notes

1. I will talk about the sort of theory that Stevenson offered later in this chapter, in section 2.5.
2. Grice (1957), p. 217.
3. Peirce's review appeared in *The Nation*, October 15, 1903, pp. 308-309. It is reprinted in Welby (1977), pp. 157-159.
4. For a more complete discussion of Welby's life, works, and influence, probably the best source available today is Schmitz (1985) which is printed as the (267 page!) introduction to Welby (1985). A very good short summary of Welby's life, work, and influence can be found in Myers (1995). See Thayer (1968) for a short review of Welby's work and influence. Schmitz says of Thayer (1968): "...the actual rediscovery of Lady Welby was induced by the research on the history of pragmatism in general and on Peirce in particular. The painstaking investigation by H. S. Thayer (1968) certainly is largely responsible for this fact." (Schmitz (1985), pp. xiv-xv) And see Nerlich (1992) for more discussion of Welby's work and especially her influence. See also Eschbach (1982) and Mannoury (1969).
5. I will use expressions like "central meaning", "core meaning", "literal meaning" and sometimes combinations of these when talking throughout this chapter about the sort of notion that Welby seems concerned about here. The notion is more-or-less the notion of *expression-meaning* that I am concerned about in the dissertation generally, but I prefer to use the above terms in the present setting because they have a more conversational and less technical sound to them.
6. The quoted material in this passage is from Stout (1891), "Thought and Language", p. 194. (Welby apparently makes a mistake in her citing the Stout article as coming from *Mind*, Volume 62.) It should be noted also that Stout, in the passage that Welby is quoting from, is himself discussing the work of the German linguist Hermann Paul, *Prinzipien der Sprachgeschichte*, translated *Principles of the History of Language*. The terms "occasional" and "usual" with respect to "signification" come from Paul. Stout quotes Paul's glosses of these: "occasional signification" is that "of a word each time that it is employed", and the "usual signification" is "that which by usage attaches to it considered in itself" (Stout (1891), p. 194). But Stout insists immediately upon mentioning the usefulness of Paul's distinction that it "must be noticed, however, that the usual signification is, in a certain sense, a fiction" (Ibid.). The view of meaning change that Welby suggests in her passage is also clearly in the Stout article a few pages later: "Permanent change of meaning arises from the gradual shifting of the limits circumscribing the general significations. This shifting is due to the frequent repetition of the same kind of occasional application" (Stout (1891), p. 196). Stout's article is an important one in the history of the theory of meaning and a fuller treatment of this history



would discuss it and its sources in more detail. Nerlich discusses some of Stout's central ideas in Nerlich (1992) pp. 236-241. See also the Aristotelian Society symposium "The Relation between Language and Thought" (which consists in the three articles Jones (1893), Mann (1893), and Stout (1893)) in which some of the issues from Stout (1891) are picked up on. The contribution by E. E. Constance Jones which leads off the symposium has independent interest here because in it Jones anticipates an important aspect of Welby's thought: "If Language is the instrument of Thought, worked out by Thought for its own purposes, it must happen sometimes that Thought presses quite beyond the limits of current language. Current speech cannot go beyond the average level of Thought, and the thinker who transcends this must also transcend ordinary language. The artist and inventor must often do it too" (Jones (1893), p. 112). For more on Jones, an important but today little remembered woman in 19th and early 20th century philosophy see Waithe and Cicero (1995). I thank Gary Ostertag for first bringing Jones's work to my attention.

7. Myers makes the claim that for Welby "metaphor is unavoidable in the adequate expression of meaning, but the use of any metaphor must be closely connected to the literal import of its words..." (Myers (1995), p. 5). It is hard to assess this claim. Myers doesn't provide any specific support for it from Welby's writings. Nor does he explain how this view is to jibe with Welby's disparagement of central meanings. My sense is that Myers might be reading into Welby's views a view that relieves a certain amount of tension, but that this is not the sort of thing Welby would have easily assented to.

8. See also Welby (1983), p. 211: "Many proposals or suggestions have been made for the acquisition of an universal language; and even now the adoption of new-Latin as a common language for philosophical as well as scientific purposes is being urged as meeting a crying need. But I venture to suggest that, except in a limited sense or as a temporary expedient, that would be beginning at the wrong end." And see Welby (1983), p. 54.

9. See Welby (1983), chapters XXVIII and XXIX, for a feeling for Welby's views on signification and education. She even includes a moderately detailed description of a series of lessons that she gave to her eight-year-old grandson in a rather long note to chapter XXIX; see pp. 306-313.

10. The emphasis is mine: I am taking her notion of *grammar* here to be broadly a notion of what may be called "conventional" in language.

11. And see Welby (1901), p. 188: "I have myself ventured to protest in many forms against the absurd assumption that from our birth we are inevitably delivered over to an abstract entity called language, occupying a throne of irrational despotism, and that we remain till death its hopeless slaves."

12. The *Encyclopedia Britannica* article is reprinted in Welby (1977), pp. 167-175. See p. 169 for the present quote.

13. The most notable omission in my discussion is probably the ethical aspects of Welby's conception of language and along with this her special notion of the *significance* of speech. This is too broad a matter to discuss in such a short summary of her work. But it connects her with American pragmatism in important respects and *any* more thorough study would have to deal with this aspect of Welby's work. See Schmitz (1985) and especially Thayer (1968).

14. Once again, see Schmitz (1985) for the most comprehensive discussion I have found of Welby's influence. Schmitz discusses Welby's relation to many thinkers, among these G. F. Stout, F. C. S. Schiller, and J. Cook Wilson, all Oxford philosophers and all relevant, therefore, to the study of Grice's influences.

15. This is according to Schmitz; see Schmitz (1985), pp. clx and clx.

16. Oddly, Schmitz doesn't mention this citation of Welby.

17. Welby became acquainted with C. K. Ogden in 1910 when he was a student at Cambridge. She discussed her ideas with him and apparently allowed him to make copies of important material in her correspondence with C. S. Peirce and perhaps others. See Schmitz (1985), p. clxxxii. See also Welby's letter to Peirce dated May 2, 1911 (Welby (1977), pp. 138-139).

18. We saw already that Russell read it. He actually reviewed it twice. See Russell (1923) and the already cited Russell (1926). Ramsey also read it and reviewed it for *Mind* (Ramsey (1924)). Wittgenstein also read at least parts of it and gave an opinion about it to Ogden in the correspondence the two of them had, the extant part of which has been published (Wittgenstein (1973)).

19. I can't assess here the following claim by Schmitz about Ogden with respect to Welby, but I mention it: "In *The Meaning of Meaning*,...Ogden went to great pains in his rare references to Lady Welby ... to obscure his earlier collaboration with her and to distance himself from significs, which he had previously greeted with joy." Schmitz says next to nothing, however, to substantiate this claim.

20. Ogden and Richards (1923), p. xvii.

21. Ogden and Richards (1923), p. 160. The only thinker earlier than Welby cited as raising the question of meaning is "one Edward Johnson". The quote Ogden and Richards provide on that page is from Johnson's *Nuces Philosophicæ* (published in 1842). Welby also quotes from Johnson without mentioning his name in Welby (1896), p. 190. The quote Welby used she reports to have come from "a forgotten book of somewhat quaint dialogues called *The Philosophy of Things*". The Ogden and Richards quotation from Johnson and the Welby quotation from Johnson are not identical but they just about overlap with the sentences: "B. 'What then do you mean by the word *meaning*?' A. 'Be patient. You can only learn the meaning of the word *meaning* from the consideration of

the *nature of ideas*, and their connexion with *things*."

22. Ogden and Richards (1923), p. 192.

23. I cannot discuss but would like to at note at least in passing the existence of the Dutch Significs movement which thrived in Holland from the 20's to the 50's, and saw Welby as the source of many of their most important themes. This movement, which included the mathematician L. E. J. Brouwer, was closely associated with the early years of the journal *Synthese* and one can learn much about it by looking through the early volumes of that journal. Volumes IX and X (1955 and 1956), in particular contain the papers of the sixth through ninth "International Significal Summer Conferences". Philipp Frank read papers at the sixth and eighth conferences, and Bar-Hillel read a paper at the ninth conference.

One of the leading members of this movement was Gerrit Mannoury who died in 1956. That year the "Dutch Members of the General Editorial Committee" of *Synthese* published an obituary (*Synthese*, vol. Xa) which noted that important views of Mannoury had "their origin in Lady Welby's 'significs'" (p. 111). In the same volume a D. van Dantzig had a piece describing aspects of Mannoury's work: "Already in 'Mathesis en Mystiek' (1924)...Mannoury stated that instead of investigating single words one should study 'acts of discourse'..., i.e. actions by which a subject (usually a human being, or a group of these) tries to exert some influence on an other [sic.] one [footnote: 'Already V. Welby stressed the importance of considering the psychological importance of words.']. The first subject is called the 'speaker', in an extended sense, including also the case where he is e.g. a 'writer', and the second one similarly the 'listener'.... He also pointed out that it is not sufficient to consider 'the' meaning of a word, or even of an act of discourse, but that it is necessary to distinguish its significance in many respects, in particular for the speaker and the listener separately. He denoted these by the terms 'speaker's meaning' and 'hearer's meaning'". (Dantzig (1956), p. 425) In a writing of Mannoury's own: "From the outset signific writings have laid stress on the *instrumental* use of language, on the aims of influencing the listener, or reader, or, more generally speaking, the 'hearer' (in the signific sense of the word), for certain purposes intended by the 'speaker'. The speaker has the *intention* of making the listener believes what he says." (Mannoury and Vuysje (1953), p. 148) I cannot assess the full historical importance of Mannoury or the Dutch Significs movement, but it should be clear to anybody familiar somewhat with the works of Austin and Grice that these passages contain themes that are deeply related to the central issues associated with the two Oxonians. Further research, I believe, could hardly prove uninteresting.

24. I will ignore the subtle difference between "language" and "parole" in Saussure. Baskin Wade translates the former with "speech" and the latter with "speaking". See Saussure (1916), pp. 9 and 13. In my discussion, I will continue just to speak colloquially of *speech*, but I usually will be talking about Saussure's "parole".

25. All page references in this section are to Saussure (1916).

26. See pp. 10 and 14 which I quote below, but also throughout much of the *Course*, where this talk of language as conventional is maintained. But see below for why he takes the sense in which language is conventional to be limited.

27. One might have expected that some of the sources of linguistic change and evolution should have been found in the use of metaphor or other non-literal usage. But in his discussion of linguistic change (in Part Three of the *Course*), Saussure doesn't even mention these. See next footnote.

28. *Phonology* for Saussure is about the "physiology of sounds" while *phonetics* is "the study of the evolution of sounds" [p. 33]. "Phonetics" Saussure tells us, "- and all of phonetics - is the prime object of diachronic linguistics" [p. 140]. He does suggest that there are meaning phenomena in diachronic linguistics as well: "...sounds are not the only things that change with time. Words change their signification...." [p. 141]. But his considerations of issues concerning meaning in his discussion of diachronic linguistics (Part Three of the *Course*) are minimal.

29. Saussure attributes something like this view to "neogrammarian" German linguists of the last quarter of the 19th century (who included Hermann Paul): "The new school...fought the terminology of the comparative school, and especially the illogical metaphors that it used. One no longer dared to say, 'Language does this or that,' or 'life of language,' etc. since language is not an entity and exists only within speakers" [p. 5, footnote].

30. See pp. 98-100 for some general comments on how Saussure thinks of language change, and also the third part of the book called "Diachronic Linguistics" in which he discusses what he takes to be the mechanisms of language change.

31. It is interesting to compare Saussure with Welby here. For Welby, as we saw, also respected greatly the apparent mutability of language and its source in the individual speech behaviors of the linguistic community. But, since she had such a strong aversion to the notion of *central meanings*, a notion like that of Saussure's *language* could never have been at issue for her: a Saussurean *language* essentially pairs sound-images with meanings, that is, central meanings. But Saussure must struggle with a language that is stable enough to study empirically, and yet founded somehow on the constant flow of everyday speech.

32. We might look at his talk of convention as a limited metaphor and note that Saussure also defends the use of metaphor in linguistic studies right after the comment quoted in footnote 33 above apparently praising the neogrammarian condemnation of certain metaphors: "One must not go too far, however, and a compromise is in order. Certain metaphors are indispensable. To require that only words that correspond to the facts of speech be used is to pretend that these facts no longer perplex us. This is by no means true, and in some instances I shall not hesitate to use one of the expressions condemned at that time" [p. 5, footnote].

33. See Saussure's discussion of and arguments about the relation between language and other sorts of social institutions, Saussure (1916), pp. 72-74.

34. See the treatment given to Gardiner and his influence by Nerlich in Nerlich (1990) and Nerlich (1992). But with respect to Gardiner's influence on Oxford philosophy Nerlich says nothing, her interest being primarily in the history of the discipline of linguistics and not the history of ideas more broadly.

35. Unless otherwise indicated, all page number references in this section are to Gardiner (1951).

36. As with most of the themes in *The Theory of Speech and Language*, this point can be found in earlier publications of Gardiner. See Gardiner (1919), p. 5: "Language...is a mechanism for the communication of thought, not a duplicate, or kind of soul in it."

37. Gardiner acknowledges Saussure's influence to a limited extent in *The Theory of Speech and Language*, but the extent of the influence seems much greater in many respects than what Gardiner acknowledges there. And in later discussions he gives more credit to Saussure. Gardiner (1933) begins: "To Ferdinand de Saussure belongs the merit of having drawn attention to the distinction between « speech » and « language », a distinction so far-reaching in its consequences that in my opinion it can hardly fail, sooner or later, to become the indispensable basis of all scientific treatment of grammar." In Gardiner (1937), Gardiner gives the following account of Saussure's influence on him: "I read de Saussure's now famous *Cours de linguistique* shortly after its first appearance, and at that time found it extraordinarily obscure. It was not until my own volume was out that I studied de Saussure again consecutively, and only then did I discover how much, though quite subconsciously, I really owed to him. It is a matter of justice to a great thinker that I should now openly admit my indebtedness, and express the regret that I did not earlier become aware of it." Gardiner throughout his work is generally not at all ungenerous in citing influences, especially with respect to the linguist Philipp Wegener (to whom *The Theory of Speech and Language* is dedicated), and so I find this account of Gardiner's wholly believable. It would be wrong to suppose, in addition, that Gardiner did not do a lot of creative and wholly independent work based on the distinction between speech and language.

38. It may look from this passage as though Gardiner disagrees with Saussure in taking a *sign* to be a sound which has a meaning rather than a pairing of a "sound-image" and a meaning. But the difference is only apparent. In §25 of *The Theory of Speech and Language* Gardiner argues that words cannot be understood to be sounds and claims that they are instead "psychical entities" (Gardiner (1951), pp. 69-71). And at one point he uses the term "sound-image": "The articulate sounds appear to be physical, audible, copies of one aspect of their psychical originals. It is only the sound-image connected with words which can be reproduced in a physical copy" (Gardiner (1951), p. 70). I am chiefly concerned with issues of meaning however, and I will generally ignore these sorts of questions, however interesting.

39. This idea is anticipated by Stout: "Let any one pick out at random a sentence from a book and then let him look for another exactly like it; it is a hundred to one that he will succeed in finding one, even by a long and diligent search. Sentences, indeed, are, for the most part, composed of mere elementary conjunctions of words which have become familiar through custom. But the *point* of a sentence which interests and attracts attention usually finds expression in the relative novelty which belongs to it as a whole" (Stout (1893), p. 123). Something like this idea is picked up on later by Ryle when he argues in Ryle (1953) against the idea that each meaningful sentence must have a unique *role* (or use). (See Landesman (1972), pp. 19 ff. on Ryle on this point.) And, of course, Chomsky makes much of the novelty of most sentences in various of his works.

40. Gardiner uses the expression "act of speech" frequently throughout his book in ways that seem to anticipate the use of the term "speech act" associated with Austin. I will discuss Gardiner's influence on Austin below.

41. Gardiner, in a footnote to this passage, cites Erdmann as the source of this distinction between word-meanings and 'ideas'.

42. I do not know whether Gardiner knew anything of Welby's works directly. He does refer to *significs* (Gardiner (1951), p. 85) and he equates it with "semasiology" and "semantics". Given that, as we saw, Gardiner sees his primary concern as "semasiological" (Gardiner (1951), p. 13), there is at least this much evidence, then, that he saw himself as doing just the work that Welby called for. Other than this, I have no evidence of any influence of Welby on Gardiner.

Gardiner's talk of 'clues' also recalls Welby's talk of the "Significian" as a "sense-detective".

43. I will discuss Gardiner's understanding of what today we call *speech-acts* below, but it is probably good to note here that Gardiner saw speech-act notions as central to his theory, so "communicative intention" here includes for him not just intentions to say things, but to command things, request things, etc. as well. In the present context, however, I am focussing not on the speech-act that needs to be discovered by a listener, but the content of the speech-act.

44. See my chapter 4 below for a discussion of compositional-meaning theories.

45. The last sentences of this passage in connecting specific *acts of speech* (questioning and commanding) to responses from the listener might call to mind Austin's notion of *securing of uptake* being essential to *speech acts* (see, of course, Austin (1962) and Strawson (1964)). I will mention this connection again below.

46. Gardiner cites Hermann Paul in his section on metaphor. He also mentions at one point Paul's distinction between occasional and usual meaning which it was noted above (note 6) Stout cited and Welby quoted: "...[Paul] distinguishes between *usuelle Bedeutung*, the generally accepted meaning of a word, and *okkasionelle Bedeutung*, the

meaning which a speaker attaches to a word at the moment of utterance..." [p. 58].

47. Interestingly, Gardiner does not cite Saussure at all in this section of his book, but he does cite the linguist Hermann Paul. Paul also, it will be recalled, had some influence on Welby and on Saussure. See above, footnotes 6 and 29.

48. It should be noted that Gardiner often enough says things that seem to contradict this claim. For example, he says at one point quite plainly that "every act of speech is the purposeful performance of a 'speaker' employing 'words' in order to draw the attention of a 'listener' to some 'thing'" [p. 187]. But I think that Gardiner's view is such that he would allow non-conventional signs to be words sometimes. He speaks notoriously little in terms of *conventions* of language even though he stresses the necessity of more than one person in order for there to be language. Because of this, his notion of language sometimes gets applied to pre-conventional speech situations. But I think that his overall intention was to hold that language is, as in the quote in the text above, the result of repetition of what were originally language-less speech episodes. I won't try to iron out this matter more nicely here. Gardiner's interests were not primarily with working out the details of the metaphysics here and so, as with Saussure, it can be expected that he will have some metaphysically puzzling threads hanging out at the edges of his thinking.

49. Gardiner makes it clear throughout his work that his attitude on this score owes much to the linguist Philipp Wegener. He provides the following quote from Wegener: "the purpose of our speech is always to influence the will or the perception of someone in a way which the speaker considers to be of importance" [p. 237]. He also discusses a number of other earlier authors on this score; see p. 182 and pp. 237-239.

50. Austin's gloss of the truth of a statement in his famous paper "Truth" is reminiscent of the present definition of a statement by Gardiner: "A statement is said to be true when the historic state of affairs to which it is correlated by the demonstrative conventions (the one to which it 'refers') is of a type with which the sentence in making it is correlated by the descriptive conventions" (Austin (1950), p. 122). Strawson in his famous criticism of Austin (1950) notes that the thesis "that all statements involve both demonstration and description is, roughly, the thesis that all statements are, or involve, subject-predicate statements..." (Strawson (1950), p. 193) and goes on to find fault with Austin for holding this thesis (Strawson (1950), p. 211). Strawson's characterization of Austin's view nicely emphasizes the features of the latter that are like Gardiner's definition of a statement: Austin's characterization of truth merely repeats Gardiner's characterization of a statement.

There is one other feature, it is interesting to note, of Austin's paper "Truth" that has a surprising resemblance to a doctrine of Gardiner's. Austin claims: "If there is to be communication of the sort that we achieve by language at all, there must be a stock of symbols of some kind which a communicator ('the speaker') can produce 'at will' and which a communicatee ('the audience') can observe: these may be called the 'words', though, of course, they need not be anything very like what we should normally call words - they might be signal flags, &c. There must also be something other than the words, which the words are to be used to communicate about: this may be called the

'world'" (Austin (1950), p. 121). This passage lists exactly the four elements that Gardiner lists as definitive of speech situations: a speaker, a listener, words, and things that the words are about: "Thus the speaker, the listener, and the things spoken about are three essential factors of normal speech. To these must now be added the actual words themselves" [p. 28]; "Not a factor of speech, but the setting in which speech can alone become effective is what is here termed 'the situation'. All four factors must be in the same situation, that is to say accessible to one another in either a material or a spiritual sense" [p. 49].

The following passage in Gardiner's book is also interesting with regard to the above, bringing together both an allusion to Gardiner's understanding of subject-predicate statements and an allusion to his understanding of the four necessary elements of a speech situation: "What, then, is the source of the almost universally held conviction that every genuine sentence must consist of subject and predicate? In my belief, this conviction springs from a dim consciousness possessed by every user of language that the act of speech involves the two factors, apart from speaker and listener, of a thing spoken about and of something said about it. In my own technical phraseology, speech involves both (1) words having a meaning and (2) a thing-meant. Or again, speech consists in using words to put meanings upon things standing outside speech. Now when speech is quite explicit, it presents to the listener something corresponding to each of the two factors in question. The subject-word places before the listener a thing to which he is to direct his attention, and the predicate-word tells him what he is to perceive or think about it" [p. 216].

I cannot argue the case fully here, but I do think that the thematic and textual correlations here strongly suggest Gardiner's influence on Austin. Other aspects of this influence will be mentioned below. Furberg, in arguing that Wittgenstein's influence on Austin was not very profound, as it is often thought to have been, indicates that Gardiner influenced Austin. I will give a quote from Furberg below to this effect. Furberg mentions a tape recorded lecture of Austin's called "What I do as a philosopher" in which Austin refers to Gardiner. See Furberg (1971), pp. viii and 57.

51. "Special sentence-quality" is a technical term for Gardiner: "...the utterance itself is called a request, question, statement, or exclamation, and these descriptions constitute its **special sentence-quality**" [p. 197].

52. On Grice on recognition of intention, see my first chapter above. On Strawson on recognizing intention and *securing of uptake*, see Strawson (1964).

53. There are a number of passages in Gardiner's book that illustrate this anticipation of the notions of *intention of recognition of intention* and of *securing of uptake*. Another is the following: "Now the speaker may have had the best of intentions, but the listener may nevertheless fail to understand what was meant. In that event the sentence does not function, and its special quality has been in vain. The act of speech desiderates an intelligent act of understanding as its counterpart, and this, however much mechanized, is always a deduction from both the words and the situation" [p. 198].



54. This distinction does have things in common with Grice's distinction between what is *said* by an utterance and what is *implicated* by it, though I cannot pursue the matter here. But see pp. 202 ff., 213, 223, 299, 306, and 330 where this distinction is discussed or illustrated. See especially pp. 230 ff. for an interesting example by Gardiner.

55. Urmson in his paper "Performative Utterances" (Urmson (1979)) discusses a little bit of how and when Austin developed his thoughts on performative utterances, but he mentions no historical influences. Still, as mentioned above, there is evidence that Austin knew of Gardiner's work and there might have been some influence here. I cannot investigate the matter more fully at this time unfortunately. See below in the text when I discuss Furberg's observations concerning the influence of Gardiner on Austin.

56. His influence in Egyptology is still widely felt today, I understand, and it has been argued that he has had an important influence on the discipline of linguistics. See Nerlich (1990) and Nerlich (1992).

57. Ryle (1961), in Parkinson (1968), p. 109.

58. Furberg (1971), p. 57 and above, note 48.

59. Wright (1976), pp. 513-514. The work by George Campbell that Wright cites here, *The Philosophy of Rhetoric*, was originally published in 1776, so it was over 150 years prior to Gardiner's work (Wright cites an 1823 edition; but see Campbell (1988)). The suggestion that hooks Gardiner and thereby Grice to the tradition of rhetorical studies is perhaps a historically interesting one. Gardiner himself speaks of the "rhetorical intentions" of the speaker a number of times in his book. In a very early paper of his, "Some Thoughts on the Subject of Language", he says the following: "Besides the content of the statement, command, question, wish, negation, maxim, epigram, or curse that is expressed, there is always involved an unexpressed relation to a listener, which by a curious paradox, is in practice the decisive factor in determining what words are actually used and the order in which they are arranged". He then asks in a footnote, "May it not, then, be said that what is now most required for the progress of linguistic science is the study of comparative Rhetoric?" (Gardiner (1919), p. 6). The connection between the tradition of rhetorical studies, which, of course, goes back to Aristotle and earlier, and the tradition that I see as flowing through Welby and Gardiner to Grice, unfortunately cannot be further examined here.

60. Lyons (1977), p. 34. "Gardiner, 1932" is, of course, *The Theory of Speech and Language* (Gardiner (1951)).

61. The emphasis is in Gardiner's original (Gardiner (1922), p. 360) and as it is quoted in Ogden and Richards (1923), p. 193. It is interesting to note that in the full quotation that Ogden and Richards use, Gardiner anticipates one of Searle's (Searle (1965)) and Ziff's (Ziff (1967)) famous, if misguided, criticisms of Grice's theory: "Is the meaning of a sentence that which is in the mind of the speaker at the moment of utterance...?"

...[N]ot that which is in the mind of the speaker, for he may intentionally veil in his utterance the thoughts which are in his brain, and this, of course, he could not do if the meaning of the utterance were precisely that which he held in his brain" (Gardiner (1922), p. 360). Gardiner thinks the formulation he presents - the one quoted in Ogden and Richards - is not subject to this criticism. See the footnotes on pp. 83 and 101 of Gardiner (1958) for responses by Gardiner to the criticisms of Ogden and Richards.

62. Ogden and Richards (1923), p. 195. It is to this sentence that Gardiner responds in his footnote on p. 101 of Gardiner (1951). It is also interesting to note that Ogden and Richards included a supplemental essay by Bronislaw Malinowski in their book in which Malinowski makes the following claim: "Dr A. H. Gardiner, one of the greatest experts in hieroglyphic script and Egyptian grammar ... has published some remarkable articles on Meaning, where he approaches the same problems as those discussed by Mr Ogden and Mr Richards, and solved by them in such an interesting manner, and their respective results do not seem to me to be incompatible" (Malinowski (1923), p. 298; in a footnote Malinowski indicates that the "remarkable articles" he has in mind are Gardiner (1919) and Gardiner (1922)). But, whatever the compatibilities between Gardiner's and their own, Ogden and Richards do seem to present the intention-based view of meaning as something rather misbegotten.

63. See note 59 above.

64. I don't at all include Saussure in the intention-based tradition - even though he is clearly important to it - because Saussure was not concerned directly with notions of speakers' intentions, no less with the identification of *meaning* with such notions. Saussure's importance cuts across the various traditions that I discuss in this chapter.

65. See note 62 above where Malinowski is noted as having remarked that he takes Gardiner's approach to be compatible with that of Ogden and Richards.

66. See Peirce (1955a), and Welby (1977) (the Welby/Peirce correspondence). For an overview of Peirce's theory of signs, see Rorty (1961). Peirce's theory is extremely complicated and his writing is replete with all sorts of unfamiliar technical jargon. Langer put it nicely: "Charles Peirce, who was probably the first person to concern himself seriously with semantics, began by making an inventory of all 'symbol-situations,' in the hope that when all possible meanings of 'meaning' were herded together, they would show empirical differentia whereby one could divide the sheep from the goats. But the obstreperous flock, instead of falling neatly into a few classes, each according to its kind, divided and subdivided into the most terrifying order of icons, qualisigns, legisigns, semes, phemes, and delomes, and there is but cold comfort in his assurance that his original 59,049 types can really be boiled down to a mere sixty-six" (Langer (1942), pp. 55-56). (Langer's source here is the portion of the Welby/Peirce correspondence that was published in Ogden and Richards (1923).) If there is a really tendentious aspect of the present history it is surely in my not giving Peirce a more central place. But my reason for not giving him a more central place is simply that I have not been able to convince

myself that his influence over English-language philosophy in the twentieth century has been as great as Welby's and Gardiner's. Though Welby and Gardiner are little known today, they actually did have quite an influence, I believe, in their time as I tried to show to some extent in the main text above. But Peirce's contribution to the theory of meaning was not as widely read and known as either Welby's or Gardiner's. Recall also that Peirce implicitly acknowledged Welby's work on meaning to be earlier than his own; see his letter to Welby, discussed above, of March 14, 1909 (Welby (1977), pp. 109-110).

Recall that Russell, in his second review of Ogden and Richards (1923), says that he first learned of the theory of meaning from Welby (Russell (1926), p. 138), but towards the beginning of the first paper of his in which he took up with the theory of meaning he remarks: "Logicians, so far as I know, have done very little towards explaining the nature of the relation called 'meaning'..." (Russell (1919), p. 7). Russell was aware of Peirce as a first-rate logician - see the numerous references Russell makes to Peirce in Russell (1903). So unless one takes Russell to be frivolous in his remark in Russell (1919), one can only assume that he was unaware of Peirce's contribution to the theory of meaning.

Then again, F. C. S. Schiller, at the beginning of the article of his which opened the *Mind* symposium, "The Meaning of 'Meaning'", approvingly quotes Russell's remark from Russell (1919) (Schiller (1920), p. 385). Schiller certainly knew of Peirce, for the two had corresponded (see "Peirce and Schiller and Their Correspondence", Scott (1973)). But apparently Peirce wrote a poor review of a piece by Schiller which created bad blood between the two. See Peirce's letter to Welby of May 14, 1905 (Welby (1977), pp. 54-57) for Peirce's side of the story at least. And Hardwick, the editor of Welby (1977), quotes a rather negative remark about Peirce by Schiller (Welby (1977), p. 56). See also Schmitz (1985), pp. cxlviii-clviii, for a little more on the relationship between Peirce and Schiller.

Peirce did, of course, influence Ogden and Richards as well as Morris, but Peirce's influence in Europe, I believe, was much greater than it was in either England or America, largely because of the importance that semiotic studies have been given there. But the extent of Peirce's influence in the English speaking world cannot be assessed wholly here. It is my general view, however, that for the history that culminates with Grice's works, Peirce is of less importance than Welby or Gardiner, which is why I gave these two such a prominent place in my story.

67. Most important for the present discussion are Russell (1919), Russell (1921), Lecture X, Russell (1923), and Russell (1926).

68. Ogden and Richards (1923), chapter 3. See Max Black's discussions of Ogden and Richards theory, which are actually helpful for understanding causal theories generally as well; Black (1949c), Black (1949d), and Black (1968)

69. Stevenson (1944), chapter 3.

70. Morris (1946). See Max Black's article on Morris, which, again, is helpful in understanding causal theories generally; Black (1949b).

71. I would like to mention however that Ferdinand Tönnies also provides something of a causal theory of meaning in his paper "Philosophical Terminology" (Tönnies (1899)). It is not unreasonable to wonder whether Tönnies might have been influenced in his causal theory by Hobbes since Tönnies was the editor of Hobbes's *Behemoth* and *The Elements of Law* as well as the author of a book on Hobbes's life and work (Tönnies (1925)). For a little about Hobbes's view on language, see the section on Hobbes in Kretzmann (1967) and see Landesman (1972), especially the quote on p. 2.

72. Ogden and Richards put this nicely; see Ogden and Richards (1923), pp. 10-11, especially the diagram on p. 11.

73. This isn't quite Stevenson's theory though it might sound a little like it here. As I said, though, I can't get into the subtleties here. See Stevenson (1944), chapter 3.

74. Of course, theorists who spoke in terms of "tendencies to behavior" might not be so happy with my referring to such things as *mental states*. But causal theories have been around longer than behaviorists and it seems that the behaviorists merely took the older theories and replaced the allusions to mental states with allusions to things like tendencies or dispositions to behavior.

75. See Russell (1919) and Russell (1921), Lecture X.

76. In Russell (1921), Russell seems already less committed to images than he was in Russell (1919) and towards the end of the former he says of the meanings of public-language expressions and mental images that "the relation which constitutes meaning is much the same in both cases. A word, like an image, has the same associations as its meaning has" (Russell (1921), p. 210). This sort of claim is significant because it shows that though Russell was using images in his theory, he wasn't depending on their being images to do semantic work: it is not because images resemble things that they *mean*, but because they cause the same associations as their meanings. One can wonder why Russell bothered to use images if their semantics is determined causally, but that is a matter for another time. Note, though, that in Russell's second review of Ogden and Richards (1923) he says, "These authors urge - rightly, as I now think - that 'images' should not be introduced in explaining 'meaning'" (Russell (1926), p.140). But see Russell (1948), p. 96 and my discussion in chapter 1 above, note 49, for indications that Russell seems later to have not only embraced images again, but to have let them *mean* in virtue of their resemblance to what they are images of. Go figure! Unfortunately, I can't discuss this any further here either.

77. "A sign, or *representamen*, is something which stands to somebody for something in some respect or capacity. It addresses somebody, that is, creates in the mind of that person an equivalent sign, or perhaps a more developed sign. That sign which it creates I call the *interpretant* of the first sign" (Peirce (1955a), p. 99).

78. "A sign is always a stimulus similar to some part of an original stimulus and sufficient to call up the engram formed by that stimulus" (Ogden and Richards (1923), p. 53). "An engram," they tell us, "is the residual trace of an adaptation made by the organism to a stimulus" (ibid.).

79. "...[A] sign's descriptive meaning is its disposition to produce *cognitive* mental processes, where 'cognitive' is to be taken as a general term designating such specific kinds of mental activity as believing, thinking, supposing, presuming, and so on" (Stevenson (1944), p. 62). True, Stevenson does speak of *tendencies* to behavior also. But he seems not to want to be taken as a full-blown behaviorist: "In stressing dispositions to *action*, the present account is not presenting an uncompromising defense of behavioristic psychology" (op. cit., p. 66).

80. "If anything, *A*, is a preparatory-stimulus which in the absence of stimulus-objects initiating response-sequences of a certain behavior-family causes a disposition in some organism to respond under certain conditions by response-sequences of this behavior-family, then *A* is a sign" (Morris (1946), p. 10, emphasis Morris's). See note G on p. 250 of Morris (1946) where Morris cites Tarski as the one who gave him the idea to try to state only a sufficient condition for something's being a sign, rather than a necessary and sufficient condition.

81. See, for example, Grice's criticisms of Stevenson's theory in Grice (1957), Black's criticisms of Ogden and Richards's theory in Black (1949c), and Black's criticisms of Morris's theory in Black (1949b).

82. It is perhaps important to note that various philosophers who I have mentioned in this history have at least vaguely broached the subject of *meaning* in the sense in which a speaker is said to *mean* something by an utterance. What is important here is that though such philosophers addressed this notion, it seems not at all to have occurred to them that perhaps a theory of meaning could be based on this notion as it did to Welby. Langer is a good example of such a philosopher. She speaks of "the ambiguous verb 'to mean'" and says of it that "sometimes it is proper to say '*it* means,' and sometimes '*I* mean.'" She continues, "[o]bviously, a word - say, 'London' - does not 'mean' a city in just the same sense that a person employing the word 'means' the place" (Langer (1942), p. 55). But though Langer notes the notion of *speaker meaning*, it is only to put it aside: "In the further analyses that follow, 'meaning' will be taken in the objective sense, unless some other is specified; that is to say, I shall speak of terms (such as words) as 'meaning' something, not of people as 'meaning' this or that" (Langer (1942), pp. 57-58). The fact that Langer didn't make use of a notion of speaker-meaning doesn't make her work any less interesting, by the way. But Langer represents a clear case of a philosopher who identified something like a notion of speaker-meaning only to let it go. Welby did not do this.

83. I would also like to say that I would not be horribly shocked if it should somehow be revealed that Grice read Gardiner. There is good evidence that both Ryle and Austin had read at least something of Gardiner since they both make reference to him (see above in my section on Gardiner). But, in fact, I have found no direct evidence that Grice read Gardiner.

84. Interestingly, Stenius seems not at all to have been influenced by the *intention-based* tradition in his "Mood and Language-Game", but rather, he was explicitly influenced by Ogden and Richards and Russell and their causal theories (Stenius (1967), p. 259). Yet Stenius's paper has had a pretty big affect on the Gricean tradition largely through its influence on David Lewis (see Lewis (1969) and Lewis (1975)). This is just one place that the causal and intention-based traditions dovetail in more recent work.

85. Carnap (1928), pp. 34-35.

86. Or, slightly less ambiguously, the theory is that there is a relation such that a sentence means what it does among a group because each member of that group stands in the relation to a compositional-truth theory for the language to which that sentence belongs. See the next note for more on this theory.

87. The theory that the argument now under discussion attributes to Frege is very close to that which is associated with Davidson in his various writings since Davidson (1967) (or maybe to some extent since Davidson (1965)). For, to know the logical form of a sentence is to know how that sentence is accommodated in what is sometimes called a *compositional-truth theory*. Of course, one aspect of Davidson's thought on language is a rather disparaging view of the idea of giving an analysis of the concept of meaning. But putting this aspect of his thought aside, we can say that insofar as there is something like a theory of meaning suggested in Davidson's work, it is something like this: for a sentence to mean something among a group of people is for the sentence to be part of a language that has a compositional-truth theory and for the members of the group to stand in a certain relation to that truth-theory. (See also Peacocke (1976) for something like this gloss of Davidson's views.) I will discuss the idea that a theory of meaning will require that language users *know* a compositional-truth theory below in chapter 4.

Perhaps the following famous passage from a draft of a letter Frege was to write to Philip Jourdain suggests a view importantly related to that suggested in the text: "...for a proposition must have a sense if it is to be useful. But a proposition consists of parts which must somehow contribute to the expression of the sense of the proposition, so they themselves must somehow have a sense. Take the proposition 'Etna is higher than Vesuvius'. This contains the name 'Etna', which occurs also in other propositions, e.g., in the proposition 'Etna is in Sicily'. The possibility of our understanding propositions which we have never heard before rests evidently on this, that we construct the sense of a proposition out of parts that correspond to the words. If we find the same word in two propositions, e.g., 'Etna', then we also recognize something common to the corresponding thoughts, something corresponding to this word. Without this, language in the proper sense would be impossible. We could indeed adopt the convention that certain signs were

to express certain thoughts, like railway signals ('The track is clear'); but in this way we would always be restricted to a very narrow area, and we could not form a completely new proposition, one which would be understood by another person even though no special convention had been adopted beforehand for this case" (Frege (1980), p. 79). The view here is not that meaning depends on a relation speakers have to compositional-truth theories, but that it depends on a relation that speakers have to what are called *compositional-meaning theories*. Chomsky in his *Rules and Representations* (Chomsky (1979)) seems to have held such a view. I will discuss this sort of view in general also below in chapter 4.

Dummett, at least, seems to have suggested that it might be right to attribute views something like the above to Frege. See Dummett (1981), chapters 3 and 5.

88. If I don't discuss this tradition here it is because it will be dealt with, but conceptually, not historically, in chapter 4 below. See Davidson (1963), Davidson (1967), and Davidson (1973). See also Evans and McDowell (1976).

I believe that credit has to be given to Katz and Fodor for their important early work attempting to deal with the compositional features of natural languages. See Katz and Fodor (1963). But I don't know whether before Davidson the idea had ever been explicitly suggested that *meaning* might be explained - or explained away - by stating a relation of some sort between speakers and compositional-semantic theories. The fact is, I am not sure if Davidson really explicitly suggested this before his paper *Radical Interpretation* (Davidson (1973)). But these fine points of the history of the theory of meaning will have to be discussed on another occasion.

It should also be pointed out that a *compositional-semantic meaning theory* need not be incompatible with either or both of an intention-based theory or a causal theory. See Jerry Fodor for an example of someone who finds a way of combining all three approaches. See Fodor (1987), p. 98 and Fodor (1989), p. 178.

It is because Welby explicitly called for a theory of meaning in terms of speakers' intentions that we can say that *intention-based tradition* begins way back at the beginning of this century. Likewise, Peirce clearly had in mind the hopes of explicating what *meaning* generally was. Langer, for example, clearly took him in this way as is evidenced in the quote given in note 66 above. But nobody explicitly claimed, so far as I know, that meaning was to be explained in terms of relations to compositional-semantic theories until Davidson.

89. To speak of the *psychological grounds of meaning* here is not to speak of the psychological grounds of *meanings*. Looking for the psychological grounds of meaning, in the sense in which this is intended here, is not inconsistent with Frege's platonism, that is, his anti-psychologism, with respect to *meanings*.

90. Wittgenstein used the term *logical form* in a somewhat special sense (see 2.18, 4.12 and 4.121, for example). I am using the term here as I did above in the discussion of Frege. Wittgenstein used the term "complete analysis", more-or-less, like I use the term "logical form". Thus, he says: "A proposition has one and only one complete analysis"

(3.25). (Quotations from the *Tractatus* are all taken from Wittgenstein (1961) and I will cite their locations, as I do here, by just giving the number from the *Tractatus* of the passage.)

91. Note that the term "proposition" in translations of Wittgenstein usually refers to sentences, not to the supposed abstract objects that are taken to be the referents of "that"-clauses as it does in Moore and many more contemporary philosophers. (See Moore (1953), a collection of lectures from 1910-1911 given by Moore; see especially chapter 3 - "Propositions" -, and chapter 14 - "Beliefs and Propositions".)

92. Wittgenstein in the *Tractatus* does believe that natural languages are fully compositional since he believes that all languages are wholly extensional, nay, *truth-functional*. See 5.101 and 5.54-5.422. Also, see Smart (1986) where Smart tries to make something of a case that what Wittgenstein was trying to do in the *Tractatus* is akin in motivation and execution to some of what Davidson was interested in.

93. "Man possesses the ability to construct languages capable of expressing every sense, without having any idea how each word has meaning or what its meaning is - just as people speak without knowing how the individual sounds are produced. [¶] Everyday language is a part of the human organism and is no less complicated than it. [¶] It is not humanly possible to gather immediately from it what the logic of language is. [¶] Language disguises thought. So much so, that from the outward form of the clothing it is impossible to infer the form of thought beneath it, because the outward form of the clothing is not designed to reveal the form of the body, but for entirely different purposes. [¶] The tacit conventions on which the understanding of everyday language depends are enormously complicated." [4.002] But, this passage doesn't suggest an actual theory of how the meanings of the expressions of everyday language actually have their meaning due to *tacit conventions*.

Also, there is a somewhat plausible reading of the *Tractatus* view of language in which Wittgenstein is taken as holding that the intentions of a speaker and other contextual features of an utterance must be grasped by an audience in their interpretation of utterances; see Dale (1988), chapter 4, "The Forms of Elementary Propositions". See the remark at 5.5423 for some of the motivation for this reading.

94. Russell apparently wrote his introduction to the *Tractatus* sometime between December 1919 and April 1920. Wittgenstein and Russell met in The Hague in December 1919. In a letter to Paul Engelmann dated December 15, 1919, Wittgenstein wrote: "Russell wants to print my treatise, possibly in both German and English (he will translate it himself and write an introduction, which suits me)" (von Wright (1982a), p. 87; this was translated from German by L. Furtmüller). So, it seems, Russell hadn't written the piece as of that time. But, the piece was definitely finished by the beginning of April 1920 (op. cit., p. 93). This is significant because it means that Russell had already published Russell (1919) when he wrote the "Introduction". Russell, that means, was clear about the psychological nature of the theory of meaning: "Logicians, so far as I know, have done very little towards explaining the nature of the relation called



'meaning,' nor are they to blame for this, since the problem is essentially one for psychology" (Russell (1919), p. 7; I already quoted the beginning of this passage above in note 66). And in "The Philosophy of Logical Atomism", apparently before Russell had actually seriously attempted something of a theory of meaning: "I think that the notion of meaning is always more or less psychological, and that it is not possible to get a pure logical theory of meaning, nor therefore of symbolism. I think that it is of the very essence of the explanation of what you mean by a symbol to take account of such things as knowing, of cognitive relations, and probably also of association. At any rate I am pretty clear that the theory of symbolism and the use of symbolism is not a thing that can be explained in pure logic without taking account of the various cognitive relations that you may have to things" (Russell (1918), p. 45). So Russell clearly distinguished between logical and psychological sorts of issues with respect to meaning.

95. The story about Russell's views here, however, is probably more complicated. C. K. Ogden after reading the *Tractatus* for the first time, wrote the following in a letter which he sent to Russell in 1921: "I know that you are frightfully busy at present, but I should very much like to know why all this account of signs and symbols cannot best be understood in relation to a thoroughgoing causal theory. I mean the sort of thing in the enclosed: - 'Sign Situations' [chapter 3 of the published version of *The Meaning of Meaning*].... The whole book which the publishers want to call *The Meaning of Meaning* is now passing through the press; and before it is too late we should like to have discussed it with someone who has seriously considered Watson. Folk here still don't think there is a problem of *Meaning* at all, and though your *Analysis of Mind* has disturbed them, everything still remains rather astrological" (quoted in von Wright's introduction to Wittgenstein (1973), p. 3). In Russell's letter in response to Ogden, Russell says the following: "...I think probably the causal treatment of meaning does give the solution. It was because I thought so that I started working on 'Analysis of Mind', which grew out of the problem of meaning" (op. cit., p. 4). This is odd because Ogden seems to have been asking whether Wittgenstein's approach was misguided in a general way and whether the causal-theoretic approach was the right one. Russell appears to be assenting to Ogden. But if he separated the psychological task of providing a theory of meaning in the "Introduction" to the *Tractatus* from the logical task that he ascribed to Wittgenstein - whatever that was! -, then why would he assent to Ogden's question? I cannot sort through this now, but the question is there to complicate things.

96. In the latter case, whatever there is of Wittgenstein's interest in compositionality can be taken as I suggested above we take Carnap's interest in this.

97. I am indebted to Eleanor Williamson for her correspondence on the following matters and for much help with the research here. Though, of course, any mistakes about the later Wittgenstein are all my own fault.

98. Wittgenstein (1967), §16.

99. "Philosophers constantly see the method of science before their eyes, and are irresistibly tempted to ask and answer questions in the way science does. This tendency is the real source of metaphysics, and leads the philosopher into complete darkness. I want to say here that it can never be our job to reduce anything to anything, or to explain anything" (Wittgenstein (1958a), p. 18). This is by no means an argument against reductive analysis and it is not entirely clear how to take this. But it seems to illustrate Wittgenstein's negative view of reduction.

100. Against the mental-image picture, see, for example, the famous passage in Wittgenstein (1958a), p. 3, in which someone tries to fetch a red flower with a mental image. This passage isn't directly concerned with *meaning*, but is a general attack on the idea of mental-images playing useful explanatory roles in our psychology.

101. Here are some passages that indicate this dislike of the idea of *meaning* as a mental activity: "But - can't I say 'By "abracadabra" I mean toothache?' Of course I can; but this is a definition; not a description of what goes on in me when I utter the word" (Wittgenstein (1958b), § 665).

"What does this act of meaning (the pain, or the piano-tuning) consist in? No answer comes - for the answers which at first sight suggest themselves are of no use. - 'And yet at the time I *meant* the one thing and not the other.' Yes, - now you have only repeated with emphasis something which no one has contradicted anyway" (op. cit., §678).

"And nothing is more wrong-headed than calling meaning a mental activity" (op. cit., §693; here the word "meaning" is actually used in the sense of *intention*).

See also op. cit., §§ 358, 507, and 592.

102. Likewise, I have not mentioned more recent work in causal-theoretic semantics. If I were giving a richer discussion, I would mention Stampe, Dretske, and Fodor, most notably, as well as a number of other philosophers. But such discussion is not possible here.

I will discuss compositional-semantic theories, as I have already indicated, in chapter 4 below.

### **Chapter 3**

#### **The Theory of Meaning: Framework for the Present Discussion**

##### **3.1 Introduction**

In my first chapter I discussed the general motivation for and emphasis of a "Gricean" theory of meaning. But in the course of this dissertation I would like to discuss the mechanics of a few theories of meaning and to propose one of my own. All of that requires a more detailed introduction. The present chapter is meant to provide this.

Essentially what I will do in this chapter is present a certain manner of conceiving of - or of talking about, at any rate - the problem of providing a general theory of public-language expression meaning. I will discuss a number of issues concerning public-language meaning - ambiguity, indexicality, non-indicatives, non-literal speech, and sub-sentential expression meaning - and show, either in broad terms or by mere illustration, how the conception I will present here and work with throughout this dissertation is expected to be able to handle those issues. Details about how a specific theory of meaning will deal with the various issues at play will have to await the details of the specific theory, however. I am also going to introduce the notion of a *language of thought* at the end of this chapter since that notion will be important in the discussions that follow throughout the remainder of the dissertation.

##### **3.2 The Actual-Language Relation**

Just about every sentence you can think of will have some indexical element in it. That means that the meaning of just about every utterance will not be determinable independently of features of the context in which the utterance was made. "I think that

there is a greatest prime" contains the pronoun "I" and therefore when uttered by different people, *means* - in a perfectly legitimate ordinary sense of "means" - different things. "Grass is green" evidently means something different depending on when it is said. It is roughly true today, but climactic changes, perhaps, could occur such that in 135 years it is no longer true. So, uttered today it expresses a truth, and uttered in 135 years, it, perhaps, expresses a falsehood. It is, apparently, the present-tense of the verb that is responsible for this shifting semantic value. We might, therefore, identify tense as an indexical element in many sentences in which it occurs. "It is raining" is false if I, sitting in Brooklyn on a clear day, utter it in response to a query about the present weather in Brooklyn, but is true if I, still sitting in Brooklyn on the same clear day, utter it in response to the question "What is the weather like in Prague?" provided, of course, that it is raining in Prague. There is a more subtle indexical element at work here than in the other examples, but, arguably, it is an indexical element all the same: somehow reference to a place is understood in utterances of "it is raining". Perhaps statements of arithmetic such as " $2+2=4$ " don't contain any indexical elements. Perhaps "Grass is green on September 4, 1995 at 7:30 pm" is wholly non-indexical. I am not sure whether it is completely.

But it looks like most ordinary sentences contain some sort of indexicality. This is annoying, but it is true.

Nonetheless, the framework that I would like to work within in this dissertation will have us imagining a class of sentences that have no indexical features. Even if it is hard to come up with definite examples of such sentences from ordinary language, still,

it is pretty easy to imagine what they *would be* like. They would have something like a complete and definite meaning independently of contexts of utterance. "Grass is green on September 4, 1995 at 7:30 pm" perhaps approaches what is imagined.

With or without clear examples from ordinary language, the assumption of non-indexicality is *simply a convenience*. This will be discussed further below.

Imagine, then, that we are concerned only with languages that have no indexical devices. Imagine also, that we are concerned with languages that only contain indicative sentences. What would a theory of meaning be like for such languages? It would be, essentially, the provision of a *satisfactory completion* of [M]<sup>1</sup>:

[M]  $(\forall P)(\forall x)(\forall y)(x \text{ means among the members of } P \ y \equiv \dots)$

In chapter 1 I discussed a bit what would count as a satisfactory completion of [M] for various styles of Gricean theorists. Here I will talk a bit about another way of talking about providing such a theory of meaning.

A satisfactory completion of [M] will be a three-place predicate that is necessarily<sup>2</sup> coextensive with the three-place predicate [\*] and that uses only propositional-attitude and propositional-speech-act notions along with, perhaps, other notions so long as these other notions do not require for their satisfaction the satisfaction of any public-language semantic notions:

[\*]  $x \text{ means among the members of } P \ y$

For a given population P, the extension of the predicate "x means among the members of P y" will be a set of ordered pairs of non-indexical, indicative sentences and their meanings. Let's temporarily call *any* set of pairs of non-indexical, indicative

sentences a *language*'. It is harmless to restate the central task of the theory of meaning as the task of finding a *satisfactory completion* for [U]:

[U]  $(\forall P)(\forall L)(P \text{ uses}^* L \text{ just in case } \dots)$

where P ranges over populations, L over *languages*', and where *uses*' is defined as follows<sup>3</sup>:

$(\forall P)(\forall L)(P \text{ uses}^* L \text{ just in case for every sentence } \sigma \text{ of } L \text{ and sentence meaning } \pi \text{ such that } \sigma \text{ is paired with } \pi \text{ in } L, \sigma \text{ means among the members of } P \text{ } \pi)$

Nothing in all of this is very interesting. All that is being done is the introduction of a way of speaking about providing a completion for [M] and thereby about providing a theory of meaning. It should be clear that the notions of *use*' and of a *language*' have been set up so that it turns out that providing a completion for [U] is equivalent *with no remainder* to providing a completion for [M] (and vice versa).

Some *languages*' will be functions in the set-theoretic sense. These will be the *languages*' in which there is no sentence paired with two distinct meanings, that is, in which there are no ambiguous sentences. We can, following David Lewis, call these *languages*. But, the notion of a *language* here, just like the notion of a *language*' is a special technical notion and not some ordinary notion of a language.

Notice that every *language*' will either be a *language* or the union of two or more *languages*. So, if we reconstrue the "L" of [U] as ranging over Lewisian *languages* rather than *languages*', nothing changes essentially with respect to the theory of meaning. We can still say that the central task in providing a theory of meaning is providing a *satisfactory completion* of [U] where L ranges over *languages* in the Lewisian sense.

The *uses*\* relation which holds between populations and *languages* is what Lewis has called the *actual-language relation*. To complete [U] is to provide a theory of what David Lewis has called the *actual-language relation*. The main task in providing a theory of meaning, then, can be said to be the task of providing a theory of the *actual-language relation* (ALR).

I introduce the notion of the ALR here mainly because literature that I will discuss in subsequent chapters is couched within this way of *talking about* the theory of meaning. But I would like to stress that nothing in such talk adds anything at all of substance to talking about providing a satisfactory completion for [M]. In the introduction to this chapter I spoke of a certain way of *conceptualizing* the theory of meaning that I would be using throughout this dissertation. What I had in mind was actually this way of *talking about* the theory of meaning by way of talking about the ALR. Perhaps merely introducing a way of talking shouldn't be called a way of *conceptualizing* things. But, it should be clear what I am doing here.

### 3.3 Propositions

The objects of our propositional attitudes and the meanings of sentences are, I will take it, propositions. I need this assumption to make sense of the extension of the meaning relation as I have, that is, as a set of ordered pairs of sentences and things that are their meanings. And there seems to be something of *prima facie* support for accepting propositions when we consider certain data from natural language. For example, the inference from [a] to [b] here seems a valid one:

[a] Oscar believes that dogs bite.

[b] Therefore, there is something that Oscar believes.<sup>4</sup>

It is hard to account for the apparent validity of this inference without taking the conclusion to involve objectual quantification over things that Oscar can believe.

Of course, there are famous problems associated with propositions. Most notably, nobody seems to have a clue about how we, presumably bits of the natural order, could be related to them, presumably abstract, non-natural objects. Different philosophers have suggested different ways of availing themselves of propositions with a clear metaphysical conscience. I'll remark on a few of these and then suggest my own.

One attitude that can be held here is that there simply are good reasons for accepting propositions - and accepting them as abstract, non-natural objects -, and that the epistemological worries of some philosophers are tied to a naturalistic bias in their epistemology which can be shed by accepting a non-naturalistic epistemology of some sort.<sup>5</sup> But since I find the notion of a non-naturalistic epistemology extremely unclear, I cannot adopt such a view as this.

Fodor has suggested that were there no worries extending from naturalistic epistemological scruples, then we might as well ask, "if physicists have numbers to play with, why shouldn't psychologists have propositions".<sup>6</sup> But I wouldn't be happy with this attitude either since - even apart from its also asking me to put aside worries that extend from a sense that our epistemology should be accounted for naturalistically - it is not clear what the physicists use of numbers amounts too. You can't quell the epistemological concerns by pleading indispensability since one wonders *because* of the epistemological



concerns whether abstract objects indeed are ultimately indispensable.

A moderate approach to propositions has been to identify them with set-theoretic objects of some sort and then to hope, I think the idea is, that somehow set-theoretic notions will escape the sorts of problems that philosophers have had with propositions. For example, David Lewis<sup>7</sup> and others<sup>8</sup> have identified propositions with sets of possible worlds, and Scott Soames<sup>9</sup> and others<sup>10</sup> have suggested understanding propositions as structured entities that might be identifiable in set-theoretic terms as certain sorts of ordered-pairs. Of course, possible-worlds semantics has ontological and epistemological problems of its own. But still, one might feel - or hope - that acceptance of possible worlds is clearer than acceptance of unreduced propositions. So we can imagine, anyway, that someone might try to forestall the epistemological problems with raw propositions by shifting the issue to the epistemology of possible worlds and set theory.<sup>11</sup> The structured-proposition theorist doesn't have the special ontological and epistemological issues to face as the possible-worlds semanticist, and so might have all the more reason to welcome the identification of propositions with set-theoretic stuff, given the independent view that somehow the epistemology of set-theory is not at all as problematic as the epistemology of raw propositions.<sup>12</sup>

The motivations for set-theoretic identifications of propositions largely, if not completely, stem from concerns with the compositionality of natural language semantics. I will discuss such issues in my next chapter. But for now I will simply say that I am reluctant to accept these set-theoretic identifications of propositions even on the assumption that they could help with the epistemological worries about propositions - an

assumption that I am not sure should be granted either - since I am not convinced that propositions so understood can help to give an acceptable account of our propositional-attitude ascriptions. If they can't, then any promise they might have offered to help with understanding how we could stand in epistemological relations to such things is beside the point: we might as well have picked trees to be propositions - they surely can be known about and they don't help with belief-ascription either. The issues here are bound up with those of compositionality which I will discuss further in the next chapter.

Stephen Schiffer has argued for what he calls a *pleonastic conception of propositions*.<sup>13</sup> On this conception, propositions are something like fictional objects: we can truly speak of their existence and of their having certain properties, but this existence and the properties that they have are determined by our linguistic practices alone and not by *them themselves*. I cannot discuss all the details and merits of Schiffer's discussion of pleonastic propositions here. But, the view is interesting and attractive enough to merit a few comments, even if in the end I will express dissatisfaction with it.

Schiffer tries to make sense of the notion of a proposition by way of making sense of a general group of suspect objects that includes actions, events, and properties<sup>14</sup>. He is impressed by the fact that there is a systematic syntactic relation between the singular-terms that apparently refer to these suspect objects, and certain other linguistic expressions. So, for example, the sentence "snow is white" is not, it would seem, a referential singular-term, and yet, if you stick a "that" in front of it, you get "that snow is white" which arguably is a referential singular-term. And the same is, more-or-less, true for all English indicative sentences (ignoring indexical features): stick "that" in front

of an English indicative and you get something that seems to behave like a referential singular-term. Schiffer thinks it is puzzling that we can somehow get referential singular-terms in this way: "From a true sentence containing no singular term that refers to an entity of the kind in question, we get a singular term that does refer to an entity of the kind in question" (Schiffer (1994b), p. 304). Whether or not this is really puzzling, I am not sure. But let's suppose it is.<sup>15</sup>

Schiffer is interested in examining a position whereby such singular-terms are taken somewhat seriously referentially - they really are *referential* singular-terms -, but the things that they refer to are treated "in a suitably deflationary, or minimalist, manner" (p. 305):

States, events, properties, and propositions exist all right, but in acknowledging this we are merely playing along with the language games that introduce these notions, and there is nothing more to the natures of these things than these little language games determine. [Ibid.]

No doubt, this is a seductive suggestion. It sounds - even if you ignore the allusion to *language games* - a bit like what Wittgenstein might have believed to be true about metaphysically problematic entities.

The story that Schiffer suggests to account for the suspect entities that he considers has it, more-or-less, that their existence is a matter of the existence of referential singular-terms that are constructed in a systematic and purely syntactic way from ontologically unsuspecting language.<sup>16</sup> The best way to understand this account is by looking at an example of the construction of a pleonastic object of an entirely new sort. This should be possible if Schiffer's theory is true.<sup>17</sup>

Consider the following syntactic transformation rule, call it the Q-rule: construct

the noun-phrase "the 'Q'-quantification of F" from an expression of the form "Q F" where 'Q' is a quantifier and 'F' is a common noun.<sup>18</sup> This rule would allow you to get from "every dog" to the noun-phrase "the 'every'-quantification of dog". And we can apply the rule to noun-phrases that occur in sentences. "The 'some'-quantification of dog is fun" will be true just in case "some dogs are fun". We can also allow quantification into the subject position here. We just need to adopt a rule - call it the QE-rule - that allows this, that is, so that it follows from "the 'some'-quantification of dog is fun" that "there is something that is fun".

So it looks like a few simple syntactic innovations have allowed the appearance of commitment to a new class of entities, *quantifications*, that do their work, not by somehow *being out there in the world* constraining thought by their existence, but by having their "existence" wholly determined by linguistic practices that have been adopted parasitic on already existing linguistic practices. But perhaps there is something more complicated is going on than merely paraphrasing familiar sentences with new syntactic arrangements.

Consider the sentence "no dog is fun". Applying the Q-rule to the subject we get, "the 'no'-quantification of dog is fun". Applying the QE-rule here we get "there is something that is fun". This sentence will be true if no dogs are fun, that is, "there is something that is fun" follows from "there is nothing that is fun". The introduction of the Q-rule and the QE-rule seems to have introduced an ambiguity into our talk that wasn't there before. It seems that "fun" in "June is fun" is the ordinary fun that applies to people and things just when they are fun, but not to our new entities, *quantifications*. The "fun"

of "the 'some'-quantification of dog is fun" is a different "fun" that only applies to quantifications and not to people and things. Thus, introducing quantifications also introduced a new sort of verb, one that expresses properties of the new entities. For each verb in English that can have a noun-phrase subject of the form "Q F", there will be another verb that corresponds to it and has in its extension *quantifications*.

It is interesting to look at another sort of example that brings out this feature of the introduction of pleonastic entities by way of Schiffer's recipe.

Consider the A-rule: Construct "the Adj Adv" from sentence of the form "Q Adj F V Adv" where 'Q' is a quantifier, 'Adj' is an adjective, 'F' is a common noun, 'Adv' is an adverb, and 'V' is a verb. Thus from "Every boring lecturer eats quickly" you get "the boring quickly". We can introduce a rule - call it the AS-rule - that allows us to use such noun-phrases in sentences: For any noun-phrase x constructed from a sentence S by the A-rule, there is a sentence "x is the Q F V" which is true just in case S is true. Thus, "the boring quickly is every lecturer eats" is a sentence in an English augmented with the AS-rule and it is true just in case every boring lecturer eats quickly. Nothing Schiffer says rules out this sort of formation. And I am not sure that it should. Such rules can be adopted and used with a little work, and we could probably communicate with each other very well with their results.

In the case of the A-rule and the SA-rule, the new "predicates" we get are clearly not like the predicates of English prior to the adoption of these rules. The *ambiguity* we saw in the "fun" example above is not present here. But, in a sense, we see now that it is not the ambiguity that was really interesting in the "fun" example, but the fact that

entirely new predicates have to be admitted into the language to accept reference to pleonastic entities, whether these predicates are syntactically identical to old predicates, thereby introducing ambiguity, or not.

It seems to me that this all should be allowable given what Schiffer states in his gloss of pleonastic propositions. But I am not sure that this is unproblematic, though I cannot investigate the matter any further here.

But there is a somewhat more picky problem I have with Schiffer's conception of pleonastic propositions. Schiffer tries to trace our apparent acceptance of certain abstract objects into our ontology to our acceptance of syntactic procedures that allow for apparent talk of such objects. But, if this is the case, there will be limits set on our ability to legitimately accept abstract objects into our ontology due to whatever limits there are on our abilities to deal with syntactic objects. If our syntactic competence is limited in the construction of natural-language sentences so that we could only, even under ideal conditions that ignore performance constraints, ever construct countably many sentences, then we should only be able to legitimately conceive of countably many propositions. It would seem that we will only have as many propositions as there are sentences in the language for which the syntactic-transformation rules that Schiffer's view requires are stated. If human languages - natural or artificial - only contain countably many sentences, it will follow from Schiffer's gloss of propositions that there are less propositions than there are mathematical truths, at least on a certain vague but intuitive conception of the notion of mathematical truth. But propositions, classically understood, are supposed to be the things that *are* mathematical truths. Thus, for each real number  $r$  there is a truth

" $r$  is not a member of the empty-set". Each one of these truths will not be expressible in a natural language if natural languages can have only countably many sentences.<sup>19</sup> And classically, propositions have been the sort of thing that can be identified with these truths even when we run out of sentences. So if we accept Schiffer's strategy for understanding propositions, then either we have to accept that mathematical truths are not propositions, or that they are propositions, but there aren't as many as we thought, or that they are propositions, *and* there are more than countably many of them, *but* that the human languages that we know have the resources to allow for more than countably many sentences<sup>20</sup>. It feels odd to me that an account of propositions should force me to make such choices.

Though I don't know that these observations about Schiffer's conception of propositions might lead to objections that are fatal to it, they strike me as making the conception problematic enough I would rather not avail myself of it. So I have the following suggestion about how to feel okay about my talking about propositions throughout the remainder of this dissertation.

To speak of *the belief that snow is white*, is to speak of a property of a certain sort, namely, a specific belief-property. Properties are problematic entities, just like propositions, but if belief-reporting sentences one day enjoy explication, I believe, talk of belief-properties will be explainable as well. But it may or may not be that this future explication of belief-reporting sentences will employ a notion of a *proposition* usefully - for all we know right now, belief-reporting sentences will one day enjoy explications that do not require any talk of propositions. That would be a nice thing. But whether or not

there ever will be such economical explications of belief-reporting sentences, it seems that the possibility of them makes talk of belief properties neutral about the existence of propositions. And since the problematic aspects of speaking of belief-*properties* will dissolve with an explication of belief-reporting sentences, it seems that it is completely legitimate ontologically for an intentional realist to speak as freely as possible about belief-properties.

But it turns out that *every* sentence that reports a propositional-attitude in English can be pleonastically restated with a sentence that talks about belief-properties. For example, [e], [f], and [g] can be restated as [e'], [f'], and [g']:

[e] June desires that she drinks some water.

[f] June expects that she will drink some water.

[g] June intends that she will drink some water.

[e'] June desires that the belief that she drinks some water be true.

[f'] June expects that the belief that she drinks some water will be true.

[g'] June intends that the believe that she drinks some water will be true.

Even belief-reporting sentences themselves can be harmlessly restated in terms of belief-properties, even if these have a sort of redundancy in them:

[h] Bingo believes that June is thirsty.

[h'] Bingo believes that the belief that June is thirsty is true.

The inference from [i] to [j] can be restated as an inference from [i'] to [j']:

[i] Bingo believes that June is dancing.

[j] Therefore, there is something that Bingo believes.



[i'] Bingo believes that the belief that June is dancing is true.

[j'] Therefore, there is something that Bingo believes.

Cases with quantification into "that"-clauses are also capable of being restated with expressions reporting belief-properties. Thus, [k] and [k'] are equivalent:

[k] There is something that June believes is charging.

[k'] There is something such that June believes that the belief that it is charging is true.

It is my view that every sentence with a propositional-attitude verb in it can be restated in terms of belief-properties in some way similar to the above examples. And that seems to indicate that all talk of propositions can be replaced by talk of belief-properties. Thus, all talk of propositions is just as safe as talk of belief-properties. And for the intentional realist that will mean, all talk of propositions is *completely* safe ontologically.

Perhaps the view I suggest here is not wholly unproblematic. There will be issues about the semantics of the restatements, I believe, that I cannot investigate now. But I think the view ultimately will prove a happy one. So I accept it here. All this acceptance will mean for the current dissertation is that I will talk freely in terms of propositions.

### 3.4 Ambiguity

In section 3.2 above I discussed how the goals of a theory of meaning could be expressed in terms of Lewis's notion of the *actual-language relation*. But I had to abstract away the fact that groups of speakers often have sentences with more than one

meaning for them. So, it might seem at first glance that we should have to augment the specification of the actual-language relation in some way to account for ambiguity. But this is not going to be true. For an ambiguous sentence can be treated as two sentences of two separate languages and nothing stands in the way of a population using two separate languages. So, the specification of the actual-language relation already treats of ambiguous sentences without augmentation.

Let me illustrate this point. Suppose the only sentence that was ambiguous for English speakers was the sentence "Sinatra really swings" which means for them both that Sinatra really swings and that Paris is a cool city.<sup>21</sup> We can think of English speakers as speaking two languages here. One of these languages contains all the non-ambiguous sentences paired with their meanings plus the sentence "Sinatra really swings" paired with the proposition that Sinatra really swings. The other of these languages contains just the one sentence "Sinatra really swings" paired with the proposition that Paris is a cool city. English speakers will stand in the actual-language relation to both of these languages, and thus the sentence "Sinatra really swings" will be ambiguous for them.<sup>22</sup>

Thus, a general definition of ambiguity can be given in terms of the actual-language relation:

A sentence  $\sigma$  is *ambiguous for a population P* just in case there are two languages L and L' such that (1) both L and L' contain  $\sigma$  but are such that  $L(\sigma) \neq L'(\sigma)$ , and (2) P uses both L and L'.<sup>23</sup>

David Lewis gives a slightly different account of ambiguity in Lewis (1975).<sup>24</sup>

There he complicates the notion of a language so that it is not just a function from sentences to meanings, but a function from sentences to sets of meanings. That account

is fine, it seems to me, but it is unnecessary. Everything needed to explain ambiguity is already available with the simpler notion of a language and the actual-language relation.

### 3.5 Indexicality

When indexical features of sentences are addressed, it seems best not to take sentence meanings as propositions, but rather as functions from contexts of utterance to propositions. It is common to call such a function a *character* after Kaplan's use of that term.<sup>25</sup> So, indexicals, presumably can be treated by redefining a *language* as a function from sentences to characters and then redefining the *actual-language relation* in the following way:

A sentence  $\sigma$  means among a population P a character  $\chi$  just in case P uses L and  $\chi = L(\sigma)$ .

The question is how to get from an account of the actual-language relation in the original sense in which indexicality is ignored to an account of the actual-language relation in this new sense. I don't know any general way of stating how to do this, but I will give the following example.

Suppose that someone comes up with the following theory of the actual-language relation in the original sense of that notion which excluded consideration of indexicals:

P uses L just in case for each L-sentence  $\sigma$ , members of P utter  $\sigma$  just in case they are trying to say  $L(\sigma)$ .

We could get a theory of the actual-language relation in the new sense glossed just above in the following way:

P uses L just in case for each L-sentence  $\sigma$ , and for each context of utterance c, members of P utter  $\sigma$  in c just in case they are trying to say

$L(\sigma)(c)$ .

I can think of no reason why we should expect that some particular theory of the actual-language relation - in the original sense - will be such that this sort of extension will be unavailable. So, supposing that the notion of a *character* can be appropriately understood, it seems that if we have provided a theory of the actual-language relation - again, in the original sense -, then we will have provided enough to state a theory of meaning that also dealt with the indexical features of languages.

### 3.6 Non-Indicatives

Non-indicative sentences are meaningful too and a theory of meaning should explain wherein lies their meaning. For the present discussion I will restrict my comments to questions and commands, but what I say, hopefully, will apply to other non-indicative moods as well with a little adjustment.

It is arguable that [l],

[l] "'snow is white' means among English speakers that snow is white"

is an ordinary-language sentence that reports the meaning of the sentence "snow is white".

But there doesn't seem to be equally unproblematic sentences of ordinary-language that report the meanings of questions and commands, not to mention other moods. Both [m] and [n] are really strange:

[m] "'Is snow white?' means among English speakers that is snow white.

[n] "'Close the door' means among English speakers that close the door.

I think [o] and [p] are moderately good colloquial English:

[o] "Is snow white?" means among English speakers the question whether snow is white.

[p] "'Close the door' means among English speakers the command to close the door.

And I will suppose that these are paradigmatic meaning-reporting sentences for questions and commands. I will take it, that is, that these are the sorts of sentences that a theory of meaning should aim to clarify to account for the meaning of questions and commands.<sup>26</sup>

Since "the question whether snow is white" and "the command to close the door" are descriptions, it looks like [o] and [p] are committed to new sorts of things, questions and commands, that can be the meanings of sentences. So it looks like we have three sorts of things that can be the meanings of sentences: propositions, questions, and commands. But these three sorts of things can be dealt with pretty economically in the following way.<sup>27</sup> Let each of these types of meanings be an ordered-pair of some arbitrary code and a proposition (in the original sense). Thus, the meaning of "snow is white" can be identified with <1, that snow is white>; the meaning of "is snow white?" can be identified with <2, that snow is white>; and the meaning of "close the door" can be identified with <3, that the door is closed>. Thus, the code "1" is associated with indicatives, "2" is associated with questions, and "3" is associated with commands. If there were further moods, hopefully, we could analyze these by way of pairs of mood-codes and a propositional contents as well. For ease, we can introduce the functors " $m_1$ " and " $m_2$ " to denote the functions from sentences to their mood-code and from sentences

to their propositional content respectively. So,  $m_1(\text{"is snow white?"})=2$  and  $m_2(\text{"is snow white?"})=\text{the proposition that snow is white.}$

With such identifications in hand a language can be redefined to include the non-indicatives as well: a language is now a function from sentences to their meanings in the new sense.<sup>28</sup> The actual-language relation will have to be adjusted to accommodate the new sorts of meanings. So, suppose again that someone came up with the following theory of the actual-language relation for a language without indexicals and non-indicatives:

P uses L just in case for each L-sentence  $\sigma$ , members of P utter  $\sigma$  just in case they are trying to say  $L(\sigma)$ .

To handle a language in the new sense just suggested, this account can be extended in the following way:

P uses L just in case for each L-sentence  $\sigma$ , members of P utter  $\sigma$  just in case either (1)  $m_1(\sigma)=1$  and they are trying to say  $m_2(\sigma)$ , (2)  $m_1(\sigma)=2$  and they are trying to ask whether  $m_2(\sigma)$  is true or (3)  $m_1(\sigma)=3$  and they are trying to command  $m_2(\sigma)$  be made true.

This is perhaps a bit clunky sounding, but I take it that this sort of approach is more-or-less right and will be able to be used to handle all non-indicatives.<sup>29</sup>

Thus, it should be somewhat clear that providing the actual-language relation for the artificial case of a language with no indexicals or non-indicatives makes impressive headway into the problem of supplying a general theory of sentence-meaning.<sup>30</sup>

### 3.7 Sub-Sentential Expression Meaning

So far nothing has been said about word-meaning or the meanings of other sub-

sentential expressions. But an account of these is expected from a general theory of meaning as well. And if this dissertation will focus for the most part on issues concerning sentence-meaning it is because I take it that sub-sentential expressions have as their meanings whatever it is that they contribute to sentence meanings. So, with respect to sub-sentential expression meaning, little can be said in advance of a theory of sentence-meaning.

### **3.8 Non-Literal Meaning**

There are two main issues with respect to non-literal meaning. One concerns how meaning can be possible without the use of any conventional devices at all, that is, how what I have called language-less speech might be possible. The other concerns how conventional devices can be used in non-conventional ways.

The first of these issues will be addressed by a theory of propositional-speech acts, presuming that such a theory will not need to employ notions of conventional meaning, that is, notions of literal meaning. Grice worked on such a theory in trying to analyze the concept of speaker-meaning. But the present dissertation is concerned with understanding the notion of literal meaning itself and so, the project of trying to come up with a theory of propositional-speech acts is not necessary here.

The second of the issues mentioned above is addressed by a theory that says how we create and interpret non-literal uses of conventional expressions. Grice concerned himself with this issue in part in his famous work on conversational-implicature. Others have dealt with how the use of metaphor, metonymy, and other non-literal devices works.

A theory of literal meaning would be pointless if there was no way to have it interface with a theory of non-literal use of conventional expressions. The literal meaning of an expression is, presumably, what we start off with when we interpret an utterance of the expression: if it turned out that we had a theory of literal meaning that didn't help in our understanding of how non-literal usage worked, then, in fact, we had an incorrect theory of literal meaning. In the final chapter of this dissertation I will make a proposal that is directly informed by concerns with the accommodation of non-literal usage.

Still, I will not be able to discuss in this dissertation the details of any theory of non-literal use of conventional devices. I mention this issue only to acknowledge its importance to a complete theory of meaning.

### 3.9 The Language of Thought Hypothesis

The *language of thought* (LOT) hypothesis figures into the discussion of the following chapters in a number of different ways. In this section I would like to describe this hypothesis and then show how it follows from another hypothesis which is extremely plausible and widely accepted.

The LOT hypothesis says that we think in a LOT. For simplicity, in this dissertation, I will assume that each person thinks using the same LOT, and I will call this LOT, following custom, *Mentalese*, or just *M*. Mentalese is a language whose sentence tokens are neural events or states of some sort. Roughly, the idea of the LOT hypothesis is that propositional-attitude states like believing, desiring, expecting, etc. are realized in us by the tokening of Mentalese sentences in certain ways. There are three main theses



of the LOT hypothesis that I will quickly present. I will call these the *language-thesis*, *attitude-thesis* and the *general-content-thesis*. Let me give a brief description of these in turn.

The *language-thesis* has it that Mentalese consists of things that can be called *sentences*. M-sentences, however, are not phonological, orthographic, or gestural types as are the sentences of most public languages, but rather they are types the tokens of which are states of or events within our nervous system, or within the physical stuff that makes up some other creature capable of having propositional attitudes.<sup>31</sup> All the sentences of Mentalese, according to the language-thesis, are *constructed*, in some sense of this term, from a finite stock of items which we may call the *simple lexical items* or *words* of Mentalese. The M-words are arranged in M-sentences in accordance with a finite set of rules for combining M-words to make M-sentences. These rules are the *syntactic rules* of Mentalese. When all the sentences of a language are constructed from a finite set of simple lexical items in accordance with a finite set of syntactic rules it is common to say that that language has a *finitely statable syntax*. Thus, the language-thesis of the LOT hypothesis also has it that Mentalese is conceived of as having a finitely statable syntax.

The *attitude-thesis* has it that for each propositional-attitude type there is a unique physicalistic property that an M-sentence token may have the having of which is metaphysically sufficient for the token to realize a propositional-attitude of that type. Thus, according to the LOT hypothesis, there is a specific physicalistic property, call it  $\Phi_B$ , such that for any person A, a metaphysically sufficient condition for A's having a

belief is that both a token  $s$  of an M-sentence occurs in A's head and  $s$  has the property  $\Phi_B$ . It is common among philosophers today to simplify talk by referring to the property  $\Phi_B$  as the property of *being in the belief-box*.<sup>32</sup> Thus we can say that a metaphysically sufficient condition for A's having a belief is that a token of an M-sentence occurs in A's belief-box. What I have just said about belief will apply to the other propositional-attitudes as well, and thus we end up talking about the *desire-box*, the *expectation-box*, etc. For ease of presentation I will throughout only speak of believing and the belief-box, but what I say about these should be understood as applying, *mutatis mutandis*, to the other propositional-attitudes and *boxes* as well.

Of course, when people have beliefs, they believe some particular thing, and the LOT-hypothesis seeks to explain what determines the particular thing believed. That's what the *general-content-thesis* is about. The *general-content-thesis* has it that for any proposition  $\pi$  that a person can believe, there is an M-sentence  $\sigma$  and a physicalistic property  $\Phi$  such that  $\sigma$  has  $\Phi$  and having  $\Phi$  and being tokened in the belief-box is metaphysically sufficient for believing  $\pi$ .<sup>33</sup> Thus each M-sentence has its own unique property, let's call this the M-sentence's *content-determining property*, which determines a unique proposition which is, roughly, the proposition that would be believed if the sentence were to be tokened in the belief-box.<sup>34</sup> Often we speak of the proposition thus correlated with a specific M-sentence as the *meaning* of the M-sentence. But it is best to keep in mind that saying that an M-sentence  $\sigma$  *means* some proposition  $\pi$  is just a short-hand way of saying that there is some physicalistic property that  $\sigma$  has which is such that having that property and being tokened in the belief-box is metaphysically

sufficient for believing  $\pi$ . Notice that I have said nothing about how, given one of these *content-determining* properties of an M-sentence we might compute what proposition would be believed were the sentence tokened in the belief-box. The *general-content-thesis* doesn't try to say anything about this - that's why I picked the term *general-content-thesis* to name this part of the LOT hypothesis. It tells you that each M-sentence has a property that determines the content of any propositional-attitude realized by a tokening of the M-sentence, but it doesn't tell you how that property in fact determines that content. What you need to explain how these content-determining properties in fact determine the contents of propositional-attitudes is what is often called a *theory of content*. It may be that the attitude-thesis and the general-content-thesis are correct, but that no theory of content can be given. The truth and falsity of the LOT hypothesis does not rest on the existence or non-existence of a theory of content.

I believe that the LOT hypothesis as just stated follows from a widely held and extremely plausible principle, namely, [S]:

[S] If two worlds are physically indistinguishable, then there is no propositional-attitude property that is satisfied in one but not the other of the worlds.

This principle, which I will sometimes refer to as *the supervenience principle*, has it that proposition-attitude properties *supervene* on physical properties. Let me now say why I think that the LOT hypothesis follows from this supervenience principle.

What I will do is show that if [S] is true, then each of the theses of the LOT hypothesis is true.

Consider first the attitude-thesis. This thesis, again, has it that there is a physical

property such that it is metaphysically sufficient for having a belief that an M-sentence has that property and gets tokened in the belief-box. But this will be trivially true if [S] is true. Notice first that there may be a number of different physical properties that are each metaphysically sufficient for having a belief. Their disjunction will itself be a physical property. Thus, that there is some physical property the having of which is metaphysically sufficient for having a belief is guaranteed by the supervenience principle.<sup>35</sup> Call this property  $\Phi_B$ . All we need to do is to show that this property amounts to the property of having a sentence in the belief-box. All that needs to be done here is to say so.  $\Phi_B$  is the property of having a sentence in the belief-box.<sup>36</sup>

This might seem strange because when you first hear the LOT hypothesis you expect that its truth will depend on all sorts of complicated detailed facts about neurons and functional states and what-have-you. But none of that is really the case. This will become more and more apparent, I hope, as I go on. I will discuss this sense of the LOT hypothesis more below. For now I will just ask that you keep in mind that the LOT hypothesis is pitched at such a high level of abstraction that it is open to all sorts of interpretations. I am just providing one on which it will be true if the supervenience principle is true.

Let's consider the general-content-thesis next. This says, roughly, that for any proposition that can be believed there will be a physical property that some M-sentence has such that having that property and being in the belief-box is metaphysically sufficient for producing a belief that that proposition holds. But this also will be trivially true if [S] is. For, for any proposition p, [S] guarantees the existence of a physical property that is

metaphysically sufficient for believing  $p$ , albeit, most likely, a highly disjunctive one. An M-sentence can be understood as any configuration whatsoever of the nervous system or the physical stuff that makes up a believer - nothing in the LOT hypothesis constrains this. Suppose that  $\Phi_{69}$  is the physical property that is metaphysically sufficient for believing that dancing is fun. Suppose that for June to have this property is for her nervous system to be in either state  $S_1$ , or  $S_2$ , or.... There will have to be some such disjunction if June has a nervous system at all. I will call this disjunction  $S_D$  and I will identify it with the M-sentence that means that dancing is fun. For each one of the states in this disjunction will have to be physically related to the whole rest of the world in such a way that  $\Phi_{69}$  holds. And that is just to say that each one of those states can be said to have a property such that  $\Phi_{69}$  holds. But since the general-content-thesis required that for each believable proposition there should be some such property, and since what I have said for the proposition that dancing is fun will just as well hold for any proposition, I have shown that the general-content-thesis is true if the supervenience principle is true.

All that I need do now is show that the *language-thesis* also follows from [S] and I will have thereby shown that the LOT hypothesis follows from [S]. But since I have taken M-sentences to be disjunctions of states of nervous systems or other physical stuff that some non-nervous believer - as we might call such - might be made up from, and since any such system will be constructed of only finitely many elements, it will follow that all M-sentences are made up from finitely many parts arranged in finitely many relations. But this being true is sufficient for Mentalese having a finitely-statable syntax. So it looks like the language-thesis is true if [S] is as well.

It might be objected here that I have misconstrued the point of the LOT hypothesis wanting a finitely-statable syntax for M. For the point is supposed to be that the finitely many parts contribute to the meaning of the whole sentence somehow and I have not shown that this is the case in just pointing out that any nervous system, or other sort of system of a finite non-nervous believer, will be constructed of finitely many physical parts arranged in finitely many physical relationships. This objection is not difficult to address.

Suppose that we identify the simple lexical items and syntactic structures of M with disjunctions of states of nervous systems in the following way. There will be some disjunction of states such that one of the disjuncts holds just in case a person is having a belief about cats. Let this disjunction be identified with the M-word for cat. Likewise, there will be some disjunction of states such that one of the disjuncts holds just in case a person is having a simple two-place-relational belief where the relation is the *is on*-relation. Etc. For each sort of thought that a person can have, the disjunction of states that are metaphysically sufficient for having a thought of that sort can be identified with the M-expression or bit of syntax. Clearly, any belief-making state of the central nervous system will be made up of words and syntactic structures so construed. These words and syntactic structures trivially have properties that make them contribute to the meanings of the states which they form parts of. So it seems that Mentalese does have a finitely-statable syntax in the sense required by the objection after all.

This all may seem outlandish at first sight. But I think it is a harmless bit of interpretation that demonstrates the vacuity of the LOT hypothesis baldly stated as I did above. It is an altogether different question whether the LOT hypothesis shown here to

be trivially true if the supervenience principle is true can serve the purposes which folks who have been interested in the LOT hypothesis have wanted it to serve.<sup>37</sup> Most notably, it might be wondered whether the LOT hypothesis as construed here can serve for the purpose of making sense of psychological explanation. I think the answer is no. A much harder LOT hypothesis will be necessary for such purposes. But, it turns out, the weak LOT hypothesis argued for here actually can do some work for us as I hope will be seen in what follows.

### 3.10 Summary

In this chapter I have laid out what I will take a comprehensive theory of meaning to be like. I have also discussed the LOT hypothesis and I hope that I have successfully made the case that a very weak version of this hypothesis follows from the supervenience principle [S] which holds that propositional-attitude properties supervene on physical properties. I have not mentioned in setting down the framework that I will work within the notion of a compositional-semantic theory at all and this might be puzzling to some who have believed that compositional-semantic theories must play an essential role in a theory of meaning. So in the next chapter I will explain my omitting talk of compositional semantics here.

## Notes

1. I will talk about talk of propositions in the next section. A complete theory of expression-meaning would have to talk about sub-sentential expressions too and should say something about the role of literal meaning in non-literal usage. I will briefly mention these issues later in this chapter.

2. *Metaphysical necessity* is meant here.

3. So, the notion of a population *using*\* a *language*\* is also a technical notion and not any ordinary notion.

4. The example is from Horwich (1990), p. 89. The line of reasoning that I am suggesting here is discussed there as well. See also Schiffer (1994b). And see Moore (1953), chapters 3 and 14, for an early (1910-1911) and extremely lucid discussion of the motivations for taking seriously propositions as objects. See also Frege (1918).

5. Perhaps Jerrold Katz has a view like this. Katz does seem to think, for example, that the epistemology of abstract objects should be explained in non-naturalistic terms. See Katz (1990), pp. 298-307. See also Katz (1981).

6. Fodor (1985), p. 12. The view of this quote, however, is not Fodor's, though he wrote it. He was merely expressing a possible view.

7. See Lewis (1969) and Lewis (1975).

8. For example Robert Stalnaker; see Stalnaker (1972).

9. See Soames (1987).

10. See, for example, the paper by Mark Richard in Salmon and Soames (1988).

11. I couldn't say whether anybody has ever really thought this.

12. Again, I really don't know whether anybody has ever actually directly argued this way. It's just a possible position.

13. See Schiffer (1990a) and Schiffer (1994b).

14. I am only going to consider here what Schiffer says in Schiffer (1994b), section V.

15. There is at least one other issue that Schiffer suggests motivates his conception, but I am simplifying for now and just presenting some of what might make Schiffer's conception attractive.



16. There are other aspects to Schiffer's account, but I don't see these as relevant here, so I won't mention them.
17. The following example is my own. Schiffer gives no examples, but only speaks of properties, propositions, events, and states.
18. I am not being tedious about the use of quotation-marks here. But things should be clear enough.
19. But see Langendoen and Postal (1984) and Langendoen and Postal (1985) for the rather esoteric view that natural languages have vastly more than countably many sentences.
20. Perhaps, for example, Langendoen and Postal are right (see previous note).
21. Keep in mind that I am pretending that there are no indexicals and no non-indicatives - I will discuss these below.
22. Notice that it doesn't matter how the *factoring* - as we might call it - of the sentences goes. It could just as well have been said that English speakers speak three languages, one that doesn't contain "Sinatra really swings" at all, then one that contains only that sentence paired with the proposition that Sinatra really swings, and then one that contains that sentence paired with the proposition that Paris is a cool city.
23. This definition captures the notion of *ambiguity in a population* perfectly, it seems to me. If a sentence is three-ways ambiguous, this definition entails that it is ambiguous. It should be clear how to come up with definitions for fancier notions, like *5-ways ambiguous in a population*, etc.
24. Loar seems to inherit Lewis's method of dealing with ambiguity in Loar (1976) and this just complicates his discussion there.
25. I will assume throughout that an account of characters can be given, but I doubt that this is wholly unproblematic.
26. I will mention wh-questions in a note below, but for now I will stick with yes-no questions.
27. This sort of proposal is due to David Lewis. See Lewis (1969) and Lewis (1975).
28. It should be clear that the propositions that are paired with the code words in meanings on the present proposal could be replaced by characters to thereby provide a fully general account of sentence meaning.

29. Nothing has been said so far, really, about wh-questions. There doesn't seem to be a propositional content to wh-questions so it is hard to see how these can be accommodated by the present strategy. I tend to think that this is, in part at least, motivation for looking at questions as a species of command. Then, the question "who was at the party?" can be taken as the command "Tell me which people were at the party". I don't believe that this is unproblematic, but I cannot discuss this further here.

30. I haven't mentioned anything at all about other sorts of speech-act notions which a full theory should treat. See chapter 4 of Schiffer (1972) for a discussion of such notions.

31. I guess with angels things might be different, though.

32. I understand that such talk of *belief-boxes*, *et. al.*, is an innovation of Schiffer's from a few years ago.

33. When I speak of an M-sentence being tokened in the belief-box, I will always mean an *unembedded* tokening. An M-sentence  $\sigma$  can occur as part of another M-sentence, say  $\sigma'$ . Then if  $\sigma'$  is tokened in the belief-box, so is  $\sigma$ , but this does not mean that what  $\sigma$  means is believed.  $\sigma$  could mean, for example, that snow is highly nourishing, and  $\sigma'$  could mean that Steve believes that snow is highly nourishing. Nell may have  $\sigma'$  tokened in her belief-box without believing that snow is highly nourishing.

34. I say "roughly" in this sentence because I am not sure about my use of "would" here, especially with respect to very large M-sentences.

35. You also need to have a principle that says that disjunctions of physical properties are also physical properties and you need not to be queasy at the sight of possibly indefinitely large disjunctions of properties. I don't see any problem with either of these.

36. And the property of having a sentence in the desire-box can be identified with an analogously understood physical property  $\Phi_D$ , etc.

37. I thank Ching-Ching Lin for help with this point.

## Chapter 4 Compositional Semantics and Meaning

### 4.1 Introduction

A compositional-semantic theory (CST) may be defined as a finite set of axioms such that for each member of some set of expressions, there is a semantic property such that the set of axioms *logically* entails a theorem that says of the expression that it has the semantic property.

Here's a boring example of a CST: the set [C] of axioms (a) and (b):

- [C] (a) "John" refers to John.  
(b) "Grass is green" means in English that grass is green.

This set meets the conditions I just stated for something to be a CST. But it is hard to see what could be interesting about [C], what it might be useful for.

Here's a more interesting example: a CST that entails for each Arabic-decimal numeral, a theorem that says which number is conventionally expressed by that numeral:

- [D] (1)  $f_I(x) =_{df}$  0 if  $x="0"$ ,  
1 if  $x="1"$ ,  
2 if  $x="2"$ ,  
3 if  $x="3"$ ,  
4 if  $x="4"$ ,  
5 if  $x="5"$ ,  
6 if  $x="6"$ ,  
7 if  $x="7"$ ,  
8 if  $x="8"$ ,  
9 if  $x="9"$ ,

or

$$\sum_{i=1}^{\lambda(x)} f_I(g(x, i)) \times 10^{i-1}$$

if  $\lambda(x) > 1$ .

(2) For any Arabic-decimal numeral  $x$ ,  $x$  conventionally expresses  $f_1(x)$ .

The  $\Sigma$ -notation is a conventional way in algebra for abbreviating an expression for the sum of a number of similar terms. Let " $A_i$ " stand for an algebraic expression of arbitrary complexity in which the symbol " $i$ " occurs one or more times, and let " $A_n$ " denote the expression obtained from " $A_i$ " by substituting a numeral  $n$  for *every* occurrence of " $i$ " in " $A_i$ ". Then the abbreviation that the  $\Sigma$ -notation represents may be defined as follows:

$$\sum_{i=1}^n A_i =_{df} A_1 + A_2 + \dots + A_n$$

Here are definitions for the two functions,  $\lambda(x)$  and  $g(x,i)$ , used in the theory [D]:

$\lambda(x) =_{df}$  the number of characters in the string  $x$ .

$g(x,i) =_{df}$  the  $i^{\text{th}}$  character from the right of string  $x$ .

For each of the infinitely many Arabic-decimal numerals, [D] entails a theorem that says which number is conventionally expressed by that numeral. Take the numeral "3652" for example. [D] tells us that the number conventionally expressed by "3652" is  $f_1("3652")$ . Since  $\lambda("3652")=4$ , and  $4>1$ , the definition of  $f_1$  tells us that  $f_1("3652")$  is

$$\sum_{i=1}^4 f_1(g("3652", i)) \times 10^{i-1}$$

Unpacking the  $\Sigma$ -notation we get that  $f_1("3652")$  is

$$f_1(g("3652",1)) \times 10^{1-1} + f_1(g("3652",2)) \times 10^{2-1} \\ + f_1(g("3652",3)) \times 10^{3-1} + f_1(g("3652",4)) \times 10^{4-1}$$

Noting that  $g("3652",1)="2"$ ,  $g("3652",2)="5"$ ,  $g("3652",3)="6"$ , and  $g("3652",4)="3"$ , and doing the subtractions in the exponents, we get that  $f_1("3652")$  is

$$f_1("2") \times 10^0 + f_1("5") \times 10^1 + f_1("6") \times 10^2 + f_1("3") \times 10^3$$

And since according to the definition of  $f_i$  given in [D],  $f_i("2")=2$ ,  $f_i("5")=5$ ,  $f_i("6")=6$ , and  $f_i("3")=3$ , we get that  $f_i("3652")$  is

$$2 \times 1 + 5 \times 10 + 6 \times 100 + 3 \times 1000$$

which is 3652. So, [D] logically entails, assuming arithmetic, that the number conventionally expressed by the Arabic-decimal numeral "3652" is 3652.

An interesting feature of [D], besides its being finite and yet entailing for any of infinitely many Arabic-decimal numerals a theorem that says which number that numeral conventionally expresses, is its apparent ability to shed light on just how the number that an Arabic-decimal numeral conventionally expresses is determined from the parts and syntactic structure of the numeral. That is, [D] seems to show how certain semantic features of members of a set of complex linguistic expressions are determined by facts about the parts and syntactic structures of those complex expressions. Looking at [D] we might conclude that the *simple* Arabic-decimal numerals each contribute, whenever they occur within an Arabic-decimal numeral, a unique number: "0" contributes the number zero, "1" contributes the number one, "2" contributes the number two, and so on through "9" which contributes the number nine. [D] also shows how the syntactic structure of an Arabic-decimal numeral can be seen to contribute to the determination of what number the numeral conventionally expresses. The position from the right within an Arabic-decimal numeral which a simple Arabic-decimal numeral occupies is, of course, a syntactic feature of the numeral, and this feature determines what we can call the *position-weight* of the simple Arabic-decimal numeral. If a simple Arabic-decimal numeral occurs in the  $n^{\text{th}}$  position from the right of an Arabic-decimal numeral, the *position-weight* of the

simple numeral will be  $10^{n-1}$ . We can define the *full-weight* of a simple Arabic-decimal numeral  $k$  that occurs within an Arabic-decimal numeral as the product of the contribution of  $k$  itself and  $k$ 's position-weight. Then, the number conventionally expressed by any Arabic-decimal numeral  $N$  is the sum of the full-weights of each of the simple Arabic-decimal numerals in  $N$ . [D] shows the substance of all of this quite elegantly. Thus, in examining [D], we might come to feel that we have learned something about how each of the simple symbols and certain syntactic features of an Arabic-decimal numeral contributes to the determination of the number expressed. And learning this sort of thing might be interesting to us for one reason or another.

More generally, a CST can be interesting when it is able to show how semantic features of complex expressions might be determined from features of the parts of those expressions and the syntactic arrangement of those parts. And CSTs that philosophers and linguists have been interested in have generally worked by saying something about each of the simple parts of the members of a class of complex expressions and about the basic syntactic structures of those complex expressions.

Notice that [D] does not tell us what the notion of an Arabic-decimal numeral *conventionally expressing* a number amounts to, that is, [D] doesn't provide an *analysis* of the relation that holds between an Arabic-decimal numeral and a number when the former conventionally expresses the latter. Of course, [D] does entail the following:

[E] For any Arabic-decimal numeral  $x$  and any object  $y$ ,  $x$  conventionally expresses  $y$  if and only if  $y=f_i(x)$ .

But since [E], even if it is true, is not metaphysically necessary - if things had been different, "3652" might have conventionally expressed the number 310 -, it cannot be

taken as an analysis of the relation in question. And it seems clear that nothing else that [D] entails will be an analysis of that relation.

I would like to say that the definition I have provided of a CST precludes any CST from being able to entail an analysis of the semantic notions its theorems ascribe to expressions. But I can't say that for an annoying reason. Suppose, to take a single example, that there was a finite theory  $\Delta^*$  which entailed an analysis of the *conventionally-expresses* relation employed in [D]. Then, the conjunction of [D] and  $\Delta^*$  will be a CST by my definition and it also entails an analysis of the semantic notion with which it is chiefly concerned. Of course, all the "CST work", so to speak, of this CST is done by [D].  $\Delta^*$  is just tacked on to [D].

Besides this, I have no argument against the possibility of a CST that worked only if it also provided an analysis of the semantic notions with which it is chiefly concerned. But this much is true: it does not follow from the definition of a CST that a CST provides an analysis of the semantic notions with which it is chiefly concerned. A CST, at least in principle, may employ semantic notions that it does not define.

And that being true it cannot be said that providing a CST in itself is providing an explication of the semantic notions it talks about.

I will make this same point in another way.

Does it follow from a CST's entailing a theorem that says that a linguistic expression has a certain semantic property that the linguistic expression actually has that property? No. Consider the theory [D'] which is identical to [D] except that the clause in the definition of  $f_1$  which applies to numerals of length greater than one is now

$$\sum_{i=1}^{\lambda(x)} f_i(g(x, i)) \times 4^{i-1}$$

The theory [D'] is a CST that logically entails, assuming standard arithmetic, that the Arabic-decimal numeral "3652" conventionally expresses the number 310. That's false. And when a theory entails falsehoods, we say that the theory is false. Accordingly, we say that [D'] is a false theory and that [D] is a true one. Plainly, there are infinitely many false CSTs each of which purports to say for each Arabic-decimal numeral the number it conventionally expresses.<sup>1</sup>

What makes a CST true or false, of course, is whether or not the expressions the CST is concerned with have the semantic properties that the CST's theorems say that they have. And notice that if we would like to evaluate a CST for truth or falsity, we will at the very least need to know something about the semantic notions the CST's theorems ascribe to the expressions with which the CST was concerned. Let me elaborate.

[D] was concerned with Arabic-decimal numerals and for each of these entailed a theorem that, for a specific number, said that it - the numeral - had the semantic property of conventionally expressing that number. So to correctly evaluate [D] for truth or falsity, it is necessary to know, for each relevant particular number, under what conditions an Arabic-decimal numeral has the property of conventionally expressing that number. And we know, I take it, that the idea that a thing conventionally expresses some other thing is the idea that *among some group of people* the first thing conventionally expresses the second. When we say that an Arabic-decimal numeral, say "3652", conventionally expresses the number 3652, what we have in mind is, I believe, that



among a certain group of people, viz., more-or-less, us, that numeral conventionally expresses that number. It was understanding, at least somewhat, the convention that holds among us with respect to the Arabic-decimal numerals that allowed me to judge that [D] is true and that [D'] is false.

We need to have *some* sort of knowledge of the semantic properties that a CST assigns to an expression in order to be able to judge the CST to be either true or false. That knowledge could be either theoretical knowledge of the semantic properties in question, though it rarely, if ever, is, or, some sort of pre-theoretic - or, as is sometimes said, *intuitive* - knowledge of those properties. If we don't know what certain property terms in a theory are about, we cannot judge the theory for truth or falsity, and, *a fortiori*, if we don't have an understanding of the semantic notions employed in a CST independently of the CST itself, then it will be impossible to evaluate the CST with respect to its truth or falsity.

It follows from this last point that, as I am conceiving things, we can hardly take a CST by itself as *defining*, in the sense of providing an analysis of, the semantic notions its theorems employ. Either the semantic notions employed in a CST are taken as primitive notions with respect to the CST or it is impossible to judge the CST for truth or falsity.<sup>2</sup> But since I have defined a CST as something that entails theorems that ascribe substantial features to linguistic expressions, that is, as something that can be evaluated for truth or falsity, the semantic notions a CST employs must be primitive with respect to the CST. But if the semantic notions employed by a CST are primitive with respect to the CST, then the CST will hardly serve as a definition, that is, an analysis, of those

semantic notions.

Of course, one can choose to shun making true/false judgements with respect to [D] and [D'] - why one might do this I'll leave open. I can pretend - or even stipulate - that I don't know what the expression "conventionally expresses" means in either [D] or [D'], that is, I can leave that phrase as an uninterpreted expression of each of them. Though I might still call [D] and [D'] *compositional "semantic theories"*, this would be, according to the way I have set things up, at best merely a *façon de parler*, and at worst, a mistake. The definition I give for the notion of a CST in the first paragraph of this chapter will have it that neither [D] nor [D'] is a CST any more: neither of them entails theorems that say something about the semantics of members of some class of expressions, or about anything else for that matter, since there occurs in each of them an uninterpreted expression. More specifically, since neither [D] nor [D'] entails theorems that say anything about the semantics of expressions, neither of them is, I should think, a *semantic theory*, and, since neither of them entails anything that can be said to be true or false, neither of them is even a *theory* in the usual sense of that term.

It is possible, of course, to take [D] and [D'] as two possible *definitions by stipulation* of the expression "conventionally expresses". In that case, neither [D] nor [D'] enlightens us as to what any prior, perhaps ordinary, notion of *conventionally expresses* amounts to. Quite the contrary. If we take either [D] or [D'] as a definition *by stipulation* of "conventionally expresses", we are choosing to ignore whatever might have been our previous understanding of that expression, and to use either [D] or [D'], as the case may be, as the sole supplier of content for it. But since, as was just noted, neither

[D] nor [D'] can be considered a CST, strictly speaking, we have not found a method whereby a CST is used to define a semantic notion. We defined by stipulation the orthographic type "conventionally expresses", but we have not discovered a definition, in the sense of an analysis of some sort, of some notion of *conventional expression*.<sup>3</sup>

A CST by itself cannot be used as a definition, in the sense of an analysis, of the semantic properties its theorems ascribe to the linguistic expressions with which it is concerned.

So much for introductory comments concerning CSTs. Now I will talk about how CSTs might help with a theory of meaning.

#### 4.2 Compositional Meaning Theories and Compositional Truth Theories

There are two kinds of CSTs that have been of particular interest to philosophers concerned with meaning. These are compositional meaning theories (CMTs) and compositional truth theories (CTTs).

A CMT for a language  $L$  is a finite set of axioms  $\mu$  such that for every sentence  $\sigma$  of  $L$  and every proposition  $\pi$ ,  $\sigma$  means  $\pi$  in  $L$  if and only if  $\mu$  *logically* entails a theorem that says that  $\sigma$  means  $\pi$  in  $L$ .

A CTT for a language  $L$  is a finite set of axioms  $\tau$  such that for every sentence  $\sigma$  of  $L$ ,  $\tau$  *logically* entails a theorem of the form " $\xi$  is true in  $L$  if and only if  $s$ " where in place of " $\xi$ " there will be a structural description<sup>4</sup> in the metalanguage of the sentence  $\sigma$ , and in place of " $s$ " there will be a metalanguage translation of  $\sigma$ .<sup>5</sup>

There seem to be lots of examples of CTTs. It is moderately easy to provide a

CTT for an interpreted first-order language as well as many other formal languages, and for large fragments of natural languages. But providing a CTT for an entire natural language has proved a difficult matter. A central problem for this task has been the accommodation of propositional-attitude sentences within a CTT. No one knows how to do this in a way that is wholly satisfying. I will return to this matter in a moment. Let me say something about CMTs first.

There are no *interesting* examples of CMTs. Not even one. Not even for the simplest language.<sup>6</sup> The reason for this is pretty straightforward: no one knows what the logical form is of sentences like "'Snow is white' means in English that snow is white". Without knowing the logical form of such a sentence, it cannot be known how to derive the sentence via the laws of logic from some set of axioms. And another way to put this is that we don't know what are the referents of the "that"-clauses of meaning-report-sentences. Of course, as I have set things up above in chapter 3, we are entitled to say that these things are propositions. But that doesn't get us far. What we would need to know is how propositions are compositionally determinable, that is, we would need to know operations that can be performed on word meanings given their syntactic arrangements in sentences, and these operations would have to allow us to compute the propositions that are the meanings of sentences in which they occur. What we need is what we had in the case of [D] above which computed the numbers conventionally expressed by Arabic-decimal numerals: we knew operations on certain small numbers that allowed us to compute the bigger numbers that complex Arabic-decimal numerals express. But what we need for a CMT is exactly what we don't have. We don't know

what propositions are and we don't know what should be assigned to the basic vocabulary of a language, so *a fortiori* we don't know what operations should be performed on such things to allow us to compute the things that propositions are supposed to be. In short, it seems that we don't know *any* of the things that we ought to know to know that there are CMTs. But perhaps that is not right. There are arguments some philosophers have offered why, even if we don't know what propositions are and how they might be computed from word-sized meanings, we still need to believe there are CMTs. I will discuss these arguments a bit later in the chapter. But I should think that given the antecedent lack of knowledge of *any* of the essential materials for constructing a CMT, belief in CMTs should be eyed with a healthy amount of scepticism.

There is a connection that it is good to keep in mind between CMTs and CTTs. If propositional-attitude sentences turn out to be accommodated in CTTs as relational, but not as relations to sentences, then apparently the "that"-clauses complements of propositional-attitude verbs will be referential singular terms that refer to propositions. But if this is true, then presumably such propositions will be determinable on the basis of the assignments by CTTs of referents to the terms that occur in the "that"-clauses. So whatever gets assigned by a CTT as the referent of a term in a "that"-clause will be the sort of thing that can help determine the proposition that is the referent of the "that"-clause. But if the referents provided by CTTs of terms in "that"-clauses can be used to compute these propositions, then these same things can be used by CMTs to compute propositions as well. So it looks like the existence of a CTT will entail the existence of a CMT if propositional-attitudes are accommodated by CTTs as relations to propositions.

Notice also that any CMT for a language will entail a CTT for a language. This is true since the following inference schema is valid:

"s" means in L that s  
 $\therefore$  "s" is true in L if and only if s

Thus, if propositional attitudes are in fact construable as relations to propositions, then there will be a CMT for a language if there is a CTT for that language, and there will be a CTT for that language if there is a CMT for it. Or, in other words, if propositional attitudes are relations to propositions, then there will be a CMT for a language just in case there is a CTT for it.

What this means, in part anyway, is that if a theorist wants to shun CMTs with their propositions and other abstracta, that same theorist will have to shun the relational view of propositional attitudes unless she or he takes the view that propositional attitudes are relations to sentences or perhaps some other sort of thing. Davidson and Davidsonians take up a course like that. They deny CMTs but insist on CSTs and yet, since they take propositional attitudes to be relational, are forced to take them as relations to public-language sentences. That is the fast version of that story at any rate. I point all this out now because I think it is good to keep in mind that either CTTs and CMTs stand and fall together or you are not a relational theorist about propositional attitudes or you construe them as relations to sentences.

With all this said, I would now like to look at how CSTs are supposed to help in a theory of meaning.

### 4.3 Meaning and Naturalizing CSTs

It's a real popular idea that somehow it is because we stand in some sort of cognitive relation to a CST that our sentences mean what they do. The idea that is of interest to me is summed up by the question of whether [E] can be the correct form of a theory of the actual-language relation:

[E]     $P$  uses  $L$  just in case  $R_M(P, \Theta_L)$

where " $\Theta_L$ " is a compositional-semantic theory of some sort and " $R_M$ " is a relation that holds between a population and a CST just when the population uses the language that the CST is a CST for.

There are two sorts of problems which philosophers generally confront when they hope for a theory of the actual-language relation with the form [E]. First, they need to show that the sort of CST they require is available for every usable language. And second, they have to say precisely what  $R_M$  is. Nobody has ever done either one of these things. I will discuss the second sort of problem in a bit. Right now I will just mention a few famous attempts to do something like provide a theory of the actual-language relation with the form [E].

#### 4.3.1 Davidson and Peacocke and Intentional Relations to CTTs

Davidson is probably the first to suggest that the notion of *meaning* is best understood in terms of some sort of a relation a person or population stands in to a CST.<sup>7</sup> More specifically, he thinks that there are not going to be CMTs even if every usable language has a CTT and that CTTs play crucial roles in all the relevant meaning

phenomena. He has done quite a bit of work towards trying to show that CTTs exist for natural languages, including suggesting a widely influential theory of propositional attitudes which casts them as relations to public-language sentences.<sup>8</sup> I am doubtful of the success of Davidson's treatment of propositional attitudes, but I won't discuss this matter here.<sup>9</sup> Davidson has never directly discussed his program in terms of the provision of an actual-language relation, but Peacocke shows how to speak about Davidson's project in those terms.<sup>10</sup> I will not discuss the details of either Davidson's or Peacocke's suggestions about meaning and truth-theories. Rather I will just make some brief comments about why I believe that a Davidsonian-style theory will not be able to work as a theory of meaning as I have set things up at any rate.

For Davidson usable languages enjoy CTTs without enjoying CMTs. But this will mean that any relation a group might stand in to a language via a CTT for that language, it will stand in to indefinitely many languages.

Consider a CTT for English. It will entail both [a] and [b]:

[a] "Snow is white" is true in English just in case snow is white.

[b] "Snow is white" is true in English just in case snow is white and  $2+2=4$ .

Both [a] and [b] are true about the English sentence. But imagine a language called English\* which is just the same as English except that "snow is white" means in it that snow is white and  $2+2=4$ . The very CTT which was adequate for English and entailed [a] and [b] above will be perfectly adequate for English\* as well. And that means that any relation that we stood in to English via that CTT, we also stand in to English\* via that CTT. And it should be obvious that there are indefinitely many languages such that



the very same CTT alluded to above for English will also be a CTT for them. But the actual-language relation must be more discriminating than this. We don't speak English\*. We speak English. Therefore, it seems that no relation to a CTT can be the actual-language relation. CTTs don't determine the meanings of sentences.

Well, that is not quite right really. For if it turned out that there were CMTs for natural languages and that the actual-language relation could be glossed in terms of bearing a certain relation to a CMT, then you could talk about a relation to a CTT as well since a CMT will entail a CTT. Of course, in that case, it be pointless to talk about the relation to the CTT since it was the relation to the CMT that was doing all the work. Anyway, the Davidsonian program eschews CMTs, so this wouldn't be an option for a Davidsonian.

Davidsonians are unruffled by this sort of talk, of course. On the one hand, Davidsonians seem to believe that there will be certain sorts of CTTs that will be able to be used in the interpreting of sentences by language users and that if this is the case, whatever the case is with the notion of *meaning*, everything that needed to be explained will have been explained by the provision of a CTT and a statement of how it is used in processing. And on the other hand, the Davidsonian eschews the project of giving necessary and sufficient conditions for *meaning*, that is, for analyzing the concept of meaning as a Gricean is interested in doing. But in any event, the Davidsonian program seems to be quite inadequate for helping out the Gricean.

### 4.3.2 Field, Fodor, and Naturalizing Truth

Hartry Field in his famous paper on Tarski's theory of truth<sup>11</sup> suggested a method for giving a naturalistic gloss of the truth predicate in which the notions of primitive denotation employed by Tarski-style truth theory are explicated in naturalistic terms. Field's project was not to provide a theory of meaning in that paper, but Jerry Fodor has recently suggested that the sort of thing that Field wanted to do with the truth-predicate would help us understand meaning:

Given a truth definition, the content of mental representations is determined by the interpretation of their primitive nonlogical vocabulary. So it's the interpretation of the primitive nonlogical vocabulary of Mentalese that's at the bottom of the pile according to the present view. Correspondingly, we would have largely solved the naturalization problem for a propositional-attitude psychology if we were able to say, in nonintentional and nonsemantic idiom, what it is for a primitive symbol of Mentalese to have a certain interpretation in a certain context.<sup>12</sup>

Note that if there is a finitely-statable translation theory for each public language which takes public-language sentences and provides Mentalese translations of them, then it follows that if Mentalese enjoys a CMT, then so do all public languages. For, for each public language  $L$ , if  $T_L$  is a finitely-statable translation theory for  $L$  that takes  $L$ -sentences and provides  $M$ -translations, and if  $M_M$  is a CMT for Mentalese, then the conjunction of  $T_L$  and  $M_M$  will be a CMT for  $L$ .

Fodor in the passage quoted above talks about what determines the content of mental representations. A mental representation for Fodor, of course, is a Mentalese sentence. The contents of Mentalese sentences for Fodor are propositions. So, when Fodor talks about determining the content of mental representation, he is just talking about determining the meanings of Mentalese sentences. So, Fodor seems to think that

somehow a truth definition, that is, a CTT can determine the meanings of Mentalese sentences.

Fodor appears to be suggesting that if we give a gloss of the primitive non-logical vocabulary of Mentalese, we will have provided a theory of meaning for Mentalese. So, Fodor's claim in the above quoted passage is apparently equivalent to the claim that if we could do with the truth predicate for Mentalese what Field wanted to do with the truth predicates for natural languages, then we will have produced a CMT for Mentalese.

Scott Soames and others have pointed out with respect to Field's suggestion mentioned above, that merely giving a naturalistic gloss of the notions of primitive denotation will not be sufficient for naturalizing a truth predicate: one also has to supply a naturalistic basis for the semantic contributions made by each primitive syntactic structure.<sup>13</sup> The same thing will have to apply to Fodor's suggestion, of course. So, we should suppose, then, that the idea is that if notions of primitive denotation for Mentalese are explained in naturalistic terms, and a naturalistic explication is provided for the semantic contributions of the basic syntactic structures of Mentalese, the result will be a CMT for Mentalese.

This is all fine. And it will help with the notion of meaning if it is true provided there are compositional-translation theories taking public-language sentences into their M-translations, as was indicated above. With all that the actual-language relation, presumably, would be statable in terms of these glosses of the primitive notions of a CTT for Mentalese.

But, in essence, this just is another way of stating the Davidsonian project

discussed above. And it won't work for the same reasons. CTTs can't really determine meanings.

On the other hand, we can suppose that Fodor's suggestion is not that a naturalistic understanding the notions of primitive denotation and of the stock of simple syntactic structures mentioned in CTTs will help with meaning, but that a naturalistic understanding of the notions of primitive *meaning* and of the stock of simple syntactic structures mentioned in CMTs is what will help with our understanding of meaning. So the idea would be that if we could give a gloss of the notions of primitive *meaning* employed in a CMT and state the naturalistic conditions for the contributions to meanings made by each simple syntactic structure, then we would have enough stuff to explain meaning generally.

Well, this is fine too. If all that could be given, then it would seem that we would be able to explain the actual-language relation in terms of the translation of public-languages into Mentalese and a CMT for Mentalese appropriately naturalized as just discussed. I will discuss below the question of whether there are good reasons for supposing that Mentalese will enjoy a CMT.

I would like to point out here, that I know of no good theory of primitive meaning that would help at all with what a CMT needs. Surely Fodor's attempts to provide such a theory are not sufficient for this purpose.<sup>14</sup> For on the sort of theory that Fodor argues for, a predicate like "dog" (pretend that's Mentalese) will have as its meaning the property of being a dog. But, for well known reasons that property will not suffice as the thing assigned to "dog" by a CMT. A story by Schiffer is helpful here:

Ralph came upon a race of creatures which he thought comprised a previously unencountered biological species, and he introduced the word 'shmog' to designate members of that species. "A thing shall be called a 'shmog'," Ralph said, "just in case it belongs to the species of those creatures." Unbeknown to him, however, shmoghood is doghood; Ralph had stumbled not upon a new species but a new race of dogs, and thus the property that 'shmog' has been introduced as standing for is none other than doghood.<sup>15</sup>

But 'shmog' and 'dog' will have to be synonymous for Fodor's theory since they will both stand in the relation that Fodor offers to the same property, the property of being a dog.

Besides this, Fodor doesn't try to explain the naturalistic basis of other sorts of terms at all, like "justice", "sofa", etc. Nor does he try to explain the naturalistic basis for the semantic contributions of the simple syntactic structures of Mentalese. And as far as I know, nobody else has offered a serious attempt at such things either.<sup>16</sup>

So, even if the general story I came to attribute to Fodor would explain meaning if true, there are many crucial missing details that don't seem forthcoming. And on top of all of that, there is the question of whether it is plausible that Mentalese has a CMT. I will get to this below.

### 4.3.3 Chomsky and Intentional Relations to CMTs

Chomsky has made a suggestion concerning what a theory of the actual-language relation will be like:

We can perhaps make sense of Lewis's notion, "the language *L* is used by a population *P*," .... This notion unpacks into something like: each person in *P* has a grammar determining *L* in his mind/brain.<sup>17</sup>

A grammar for Chomsky is a "finite characterization" of a language: "the grammar is a system of rules and principles that determines a pairing of sound and meaning".<sup>18</sup> In other

words, a grammar for Chomsky is a CMT.

But what is it for a grammar to be in a mind/brain? Chomsky introduces a special term, "cognizes", to express the relation that he takes language users to stand in to grammars:

...we cognize the grammar that constitutes the current state of our language faculty and the rules of this system as well as the principles that govern their operation.<sup>19</sup>

And he tells us that "...'cognizing' is tacit or implicit knowledge...".<sup>20</sup> So, Chomsky's theory of the actual-language relation goes something like this: a population P uses a language L just in case each member of P tacitly knows a CMT for L.

Chomsky's motivation for making the relation that we stand in to a grammar one of tacit knowledge is, evidently, concern for the empirical consideration that we surely don't know anything like a CMT for a natural language consciously. Linguistics would be a lot easier than it is if we did. But I am not wholly satisfied with merely suggesting that knowledge of grammars is tacit. For, I don't really know what sorts of things go on with tacit knowledge. I could tacitly know a grammar for Swedish for all I know and just never use it. I am, unfortunately, not a Swedish speaker, however. So, I don't feel clear enough with my intuitive grasp of what tacit knowledge amounts to to feel satisfied with the sort of proposal that Chomsky suggests. Of course, if the notion of tacit knowledge used by Chomsky were clarified, things might be different. But as things stand, Chomsky has not provided a clear statement of what relation we must stand in to a CMT of a language in order to speak the language.

But besides this, there is a compelling counter-example due to Schiffer to the sort

of theory that Chomsky suggests which I will discuss presently.

#### **4.4 What Reasons There are for Supposing Natural Languages Have CSTs**

The theory of meaning should explain all cases of expression-meaning, not just natural-language meaning. But natural language is a central case, and with respect to the issues this chapter is concerned with all the more so. For philosophers who expect that CSTs will be a central feature of an explanation of meaning seem motivated at least in part by the impressive way that CSTs could help in explaining how finite beings like ourselves could have competence in infinite languages, as natural languages are presumed to be. For CSTs are finite statements that entail an infinite number of semantic truths. Presumably, our natural languages are infinite, and yet, any native speaker seems limited in comprehending and producing novel natural-language sentences only by irrelevant constraints like those of memory and interest. To explain this impressive fact about us, it is thought that there must be some finite means of stating the facts about the languages that we speak, such that knowing such a statement will allow us to know all the facts about our languages that we seem to know.

But, though a CST surely might be able to help in understanding our competence in the use of really large languages, it is not the only way to so explain our competence.<sup>21</sup> For, suppose that we think in a LOT.<sup>22</sup> And suppose that each natural-language user has lodged in their head a finite device that translates sentences of their natural language into M and that this device works entirely on the basis of the syntactic forms of the natural language- and M-sentences it deals with. The idea is that this sort of device can be used

in the processing of natural-language sentences and can thereby explain the competence of natural-language users as well as a CST could, and yet, this sort of device would not presuppose the existence of a CST since it works wholly syntactically, without consideration of the semantic features of the sentences it deals with. Let's see how such a translation device might work and so explain competence in natural-languages.

Speech production can work in the following way given a translation device. Suppose that Eleanor tells Simon that Wittgenstein is clever. First, it is imagined that Eleanor desires to tell Simon that Wittgenstein is clever and that this desire is realized by the M-sentence "I tell Simon that Wittgenstein is clever" (pretend that that is an M-sentence) being tokened in Eleanor's desire-box. A subroutine of the translation device which constantly scans Eleanor's desire-box looking for sentences that begin with the string "I tell" locates this sentence. It detaches the part of the sentence that follows the "that", that is, it detaches "Wittgenstein is clever", and it sends this to the translator section of the translation device which finds the English language equivalent using wholly syntactic procedures. Eventually, other things being okay, Eleanor utters in Simon's presence this English-language sentence that was produced by the translator. No CST was required in this process.

Something analogous, but in reverse, sort of, happens in speech comprehension. Simon hears Eleanor's sentence. A subroutine takes Simon's internal representation of Eleanor's sentence and sends it to the translator which this time produces the M-sentence "Wittgenstein is clever" (again, pretend this is an M-sentence). This M-sentence is concatenated to a string which was produced by some other subroutine "Eleanor said that"



to produce the M-sentence "Eleanor said that Wittgenstein is clever". This string is then placed in Simon's belief-box. Simon believes that Eleanor said that Wittgenstein is clever.

This sketch of how a purely syntactic translation device might be employed in language processing leaves much vague, but the general scheme is as plausible as general schemes that explain language processing by way of CSTs. Thus, CST stories are not the only stories in town and so, it seems, our natural-language competence by itself doesn't require the acceptance of CSTs. All the interesting processing abilities that would be explained by a good CST story can also be explained by a good syntactic-translation device story as well, and this latter sort of story doesn't require CSTs.

Fodor has argued against the above sort of line, that even if language processing can be explained by syntactic-translation devices, that is, without CSTs, still, natural languages must enjoy CSTs because Mentalese must.<sup>23</sup> As already discussed above,<sup>24</sup> if Mentalese enjoys a CST, and if there is a finitely-statable translation function from a natural language to Mentalese, then the natural language will also enjoy a CST. The explanation of language processing by syntactic translation devices does seem committed to finitely-statable translation devices. So, the question of whether natural languages have translation devices comes down to whether Mentalese does.<sup>25</sup>

Fodor believes that there are two properties that Mentalese has that require a CMT for their explanation. First, Mentalese is *productive*, meaning that it has infinitely many sentences each with its own meaning, and second, Mentalese is *systematic*, meaning that the simple lexical items and syntactic structures of Mentalese contribute to the meanings

of the larger expressions in which they occur in a systematic way: roughly put, the fact that "Mary loves Bill" means that Mary loves Bill is intimately - systematically - tied to the fact that "Bill loves Mary" means that Bill loves Mary. The productivity and systematicity of Mentalese requires explaining, according to Fodor. Fodor's view is that only a CMT can explain these features of Mentalese.

Schiffer has argued that Fodor is mistaken in believing that *only* by positing a CMT for Mentalese can we make sense of its productivity and systematicity. Schiffer's argument is that (a) what needs to be explained about the productivity and systematicity of Mentalese is how these features can be founded in some finite basis, "some nonendless way the world is"<sup>26</sup>, (b) this can be explained by what he has called a *compositional-supervenience theory* for Mentalese, and (c) a compositional-supervenience theory for Mentalese will not require a CMT for Mentalese. I'll briefly discuss the notion of a *compositional-supervenience theory* and how such a theory can explain the productivity and systematicity of Mentalese.

Pretend that "dogs bark" is an M-sentence. On the LOT hypothesis, then, "dogs bark" will have to have a physical property - we can call it its *belief-making property* - such that having that property and being tokened in the belief-box is metaphysically sufficient for believing that dogs bark. A compositional-supervenience theory will entail a theorem that says that the belief-making property of "dogs bark" is a specific physical property, and it will also entail a similar theorem for every other sentence of M as well, and it will do all this with finite means. A *true* or *correct* compositional-supervenience theory for M gets everything right, that is, it will entail for each M-sentence a theorem

that says correctly what that sentence's belief-making property is. To be more precise, a compositional-supervenience theory for Mentalese will be equivalent to a conjunction of a finite specification of a function, call it  $f_M$ , from sentences to physical properties, and an axiom that states that for each sentence  $\sigma$  of M,  $f_M(\sigma)$  is  $\sigma$ 's belief-making property.

Presumably, such a function  $f_M$  will have to meet the following conditions:

- (c1) for each of the finitely many simple lexical items  $w$  of M there is a particular physical property  $\Phi$  such that  $f_M(w) = \Phi$ ;
- (c2) for each of the finitely many basic syntactic structures  $t$  of M which structures  $n$  subexpressions of M (for some finite  $n$ ,  $n \geq 1$ ), there is a function  $g_t$  from  $n$ -tuples of physical properties to physical properties such that, for any expression  $e$  of M with structure  $t$  and containing the  $n$  subexpressions  $e_1, e_2, \dots, e_n$  (in that order), there is a particular physical property  $\Phi$  such that  $\Phi = g_t(\langle f_M(e_1), f_M(e_2), \dots, f_M(e_n) \rangle)$  and  $f_M(e) = \Phi$ .

And for a compositional-supervenience theory to be *true* of M, the function  $f_M$  that it specifies will also have to meet the following condition:

- (c3) for each sentence  $\sigma$  of M and for each proposition  $\pi$ , if  $\sigma$  means  $\pi$  in M, then  $\sigma$  has the property  $f_M(\sigma)$  and having  $f_M(\sigma)$  and being tokened in the belief-box is metaphysically sufficient for believing  $\pi$ .

It should be moderately clear that, if what is needed by way of an explanation of the productivity and systematicity of Mentalese is that it be shown how these features of Mentalese are based in some "nonendless way the world is", then given a true compositional-supervenience theory, the productivity and the systematicity of Mentalese is explained. For clearly, given a true compositional-supervenience theory for M, it will follow that M is productive since each M-sentence will be associated with its unique belief-making property, that is, with the physical property which is the supervenience base

for that sentence meaning what it does. In other words, each M-sentence will have its unique meaning given a compositional-supervenience theory, and, therefore, M will be productive. Likewise, given a correct compositional-supervenience theory, each M-word will contribute to the belief-making properties of M-sentences the physical property assigned to that word by the theory. This contribution will be the same no matter where the word occurs, and so it seems that the contributions of M-words is systematic, and that therefore Mentalese is systematic.

Given that the productivity and systematicity of Mentalese are explained by a compositional-supervenience theory, and I believe that they are, the question comes down to whether or not having a correct compositional-supervenience theory somehow entails having a CMT as well.

Suppose that  $\Phi_D$  is a physical property such that having  $\Phi_D$  and being in the belief-box is metaphysically sufficient for believing that dogs bark. And pretend that "dogs bark" is an M-sentence that has this property. A correct compositional-supervenience theory will have to assign  $\Phi_D$  to "dogs bark". So, it will follow from a correct compositional-supervenience theory for M that "dogs bark" has  $\Phi_D$ . Will it also follow from a correct compositional-supervenience theory for M that "dogs bark" means in M that dogs bark? Recall that *to mean in M proposition p*, for any *p*, is simply to have a physical property such that having that property and being tokened in the belief-box is metaphysically sufficient for believing *p*. So, the question is, will it follow from a correct compositional-supervenience theory for M that "dogs bark" has a physical property such that having that property and being tokened in the belief-box is metaphysically sufficient

for believing that dogs bark? Well, of course, a correct compositional supervenience theory for M will *metaphysically* entail this, since in every world where a sentence has  $\Phi_D$  it also has a property such that having that property and being tokened in the belief-box is metaphysically sufficient for believing that dogs bark: that is exactly what  $\Phi_D$  must be if it is assigned by a correct compositional-supervenience theory to the sentence that realizes the belief that dogs bark when tokened in the belief-box. But, then, the question *really* is, not just *will it follow*, but will it *logically* follow from a correct compositional-supervenience theory for M that "dogs bark" has a physical property such that having that property and being tokened in the belief-box is metaphysically sufficient for believing that dogs bark? Or, to be really heavy about matters, does [F] follow *logically* from [D]?

[D] "Dogs bark" has  $\Phi_D$ .

[F] "Dogs bark" has a property such that having that property and being tokened in the belief-box is metaphysically sufficient for believing that dogs bark.

Clearly [F] follows from [D] only if we have a premise like [E]:

[E]  $\Phi_D$  is a property such that having that property and being tokened in the belief-box is metaphysically sufficient for believing that dogs bark.

So, whether a correct compositional-supervenience theory for M logically entails a CMT for M comes down to whether it will entail premises like [E] for M-sentences. Generally, if prolixly, the existence of a correct compositional-supervenience theory for M will logically entail the existence of a correct CMT for M if but only if for each M sentence  $\sigma$ , for each physical property  $\Phi$ , and for each proposition  $\pi$ , if both  $\Phi\sigma$  and having  $\Phi$  and

being tokened in the belief-box is metaphysically sufficient for believing  $\pi$ , then a premise like [G] is logically entailed by a correct compositional-supervenience theory for M:

[G]  $\Phi$  is a property such that having that property and being tokened in the belief-box is metaphysically sufficient for believing  $\pi$ .

To put it in a nutshell, a correct compositional-supervenience theory for M will entail a correct CMT for M just in case the former specifies a function from the belief-making properties to the appropriate propositions. But there is no reason to suppose that this will have to be the case. There is nothing that requires a compositional-supervenience theory to entail theorems with the form of [G]. If there were, then a correct compositional-supervenience theory for M would entail the existence of a correct CMT for M. But there is not, so it seems that the productivity and systematicity of M can be explained without commitment to a CMT for M.

Fodor has responded to this argument.<sup>27</sup> Essentially his response is that "supervenience without identity is mysterious"<sup>28</sup> and that if we do identify the belief-making properties assigned to sentences by a compositional-supervenience theory with the appropriate meaning properties, then we will have a CMT for M. But I think there are problems with both of these points. I will briefly say what I take these problems to be.

Of course supervenience is a very murky notion as far as metaphysical explanations go. But its acceptance as a notion that can do at least some metaphysical work has largely been due to a deep dissatisfaction with *identity theses*. So, unless Fodor provides some sort of calming arguments with respect to the woes of identity, it is difficult to see how to easily follow his suggestion here.<sup>29</sup> Fodor does seem to be trying

to allay worries about an identity thesis when he denies that there are issues of multiple realizability with identifying the belief-making properties with meaning properties.<sup>30</sup> But his merely denying this doesn't make it so, and I believe that the possibility that beliefs can be multiply realized simply does tell against *identifying* belief-making supervenience properties with meaning properties.

But, besides this, I don't understand, with regard to Fodor's second point, how exactly the *identification* of the belief-making properties with meaning properties will help.<sup>31</sup> The identity in [E] is not in question, nor are any of the identities reported by sentences with the form of [G]. But it has already been shown that this identity by itself doesn't make a CMT logically follow from a compositional-supervenience theory.

So I find both aspects of Fodor's responses to Schiffer's arguments unsatisfying. It seems that Fodor has not supplied a telling reason why one should accept that there will be CMTs for Mentalese. And, therefore, it seems that there are no reasons for accepting that there are CSTs for natural languages generally.

#### 4.5 Summary

Of course, nothing in the above is an argument that there are no CSTs for natural languages. But, I do believe that the arguments that have been adduced for CSTs are shown by the above considerations to be lacking. And, in fact, nobody has yet provided a CST for a natural language. So, I think it is safest to stay clear of CSTs in trying to construct a theory of meaning. In the remaining chapters of this dissertation I will discuss a number of theories that avoid, or seem to, commitment to CSTs.

## Notes

1. Of course, [D'] can be turned into a true theory for another class of expressions, viz., the base-4 Arabic numerals, provided we change the second axiom of [D'] so that it reads "For any base-4 Arabic numeral  $x$ ,  $x$  conventionally expresses  $f_1(x)$ ". The resulting theory's definition of the function  $f_1$  will have five useless clauses - the ones for the simple symbols "5", "6", "7", "8", and "9" - but this doesn't affect the theory's truth with respect to the base-4 Arabic numerals. But none of this matters to what I am doing in the main text. The point being that [D'] simply entails falsehoods about the Arabic-*decimal* numerals.

2. In the case of a CST that contains as an extra addition the statement of an analysis of its semantic notions, you still need an independent understanding of the semantic notions to judge the CST for truth or falsity since you would need such an understanding to know whether the analyses offered were correct.

3. There are, of course, two notions of *definition* being used here. One notion applies to linguistic expressions, the other to notions, that is, ideas or properties. I am just speaking loosely, but things should be clear enough.

4. I take a quoted sentence to count as a structural description of the sentence here. But maybe that is not the best way to look at things.

5. A problem for this characterization of a CTT is the following sort of theory: " $(\Sigma s)(s'$  is true in  $L$  just in case  $s$ )" where " $\Sigma$ " is a substitutional quantifier, " $L$ " is a name or description of some language  $L$  and the substitution-class for " $s$ " is the set of  $L$ -sentences. This is a finite theory that entails a truth of the appropriate form, but it is not what I want to count as a CTT. But I will pretend I haven't noticed this.

6. Well, perhaps you can invent a language with finitely many sentences and give a CMT for it. For example, Consider J-English which consists of the one sentence "June likes to dance" which means in J-English that June likes to dance. Here is a CMT for J-English: "'June likes to dance' means in J-English that June likes to dance". Okay. For the simplest languages, maybe, then, you can give a CMT. Read me in the text, then, as meaning "the simplest BIG language" where a BIG language is one that is too big to have its members enumerated by us for mundane practical reasons. But a BIG language doesn't have to be infinite.

7. See Davidson (1967). But the seeds of the idea are already in Davidson (1965). Probably his clearest statement of the project he envisioned, though, is found in Davidson (1973). Note that even if Katz and Fodor antecede Davidson with respect to concern for the compositionality of natural languages (see Katz and Fodor (1963)), still, their early proposal for a theory that deals with the compositional features of natural languages is probably better characterized as compositional-translation theory than as a compositional-



semantic theory. I will talk about compositional-translation theories and attempts to use such a theory to explain meaning when I turn in chapter 6 below to Schiffer's recent theory.

8. See Davidson (1968).

9. See Schiffer (1987b), chapter 5, "Sententialist Theories of Belief" for a somewhat compelling critique of Davidson's theory of propositional attitudes.

10. See Peacocke (1976). But note that Peacocke redefines the notion of a language for his talk of an actual-language relation. So, the truth is, he is really talking about a different notion from the one Lewis spoke of. But I am being so general in the discussion right now, this doesn't really matter much.

11. Field (1972).

12. Fodor (1987), p. 98.

13. See Soames (1984).

14. See Fodor (1987) and Fodor (1990), chapters 2, 3, and 4.

15. Schiffer (1992).

16. This might seem slightly uncharitable to Fodor since he has a sophisticated view of the logical form of propositional-attitude ascription that blocks the Frege-style point in a case like in the Schiffer story. See Fodor (1989b). But, Fodor's understanding of propositional-attitude ascription is independent of his views about the naturalization of meaning and there are a number of serious problems with Fodor's view of attitude ascription. See Schiffer (1992).

17. Chomsky (1980), p. 84.

18. *Op. cit.*, p. 82.

19. *Op. cit.*, pp. 69-70.

20. *Ibid.*

21. See Fodor (1975), pp. 119-122, and Schiffer (1987b), chapter 7. Fodor, however, has insisted that in spite of the possibility of the following sort of explanation of language processing, still there must be CMTs for Mentalese and natural languages. His arguments will be addressed in just a bit.

22. It is not necessary to take a LOT here to be the very weak sort of LOT that I discussed at the end of chapter 3. A LOT here can be as strong and as interesting as you

like for the present discussion. That is, the form of the following argument is "Suppose that there we think in a LOT on any version of the LOT hypothesis you like...."

23. See Fodor (1989) and Fodor (1991).

24. See section 4.3.2.

25. In other words, the argument Fodor has in mind is something like the following.

(1) Natural-language users have impressive skills that must be explained.

(2) Certain CST stories explain these skills and certain syntactic-translation-device stories do as well.

(3) There are no other stories besides these that would explain the skills in question.

(4) If Mentalese enjoys a CST, then so will any natural language if the syntactic-translation-device story.

(5) Mentalese enjoys a CST.

(6) So, whichever of our two stories that explains natural language processing turns out to be true, natural languages will enjoy CSTs.

So the really controversial premise here is (5), that Mentalese enjoys a CST.

26. Schiffer (1991), p. 191.

27. Fodor (1991).

28. Op. cit., p. 306.

29. And this is not yet to mention Fodor's attitude towards reduction in others of his writings.

30. Op. cit., p. 318.

31. Fodor does speak of *identifying* the properties assigned to expressions by a supervenience theory with the *semantic values* of the expressions. (See, for example, Fodor (1991), p. 306: "...the temptation might be overwhelming to simply *identify* the semantic values of words....") Usually the expression "semantic value" is used to talk about what a CMT or a CST assigns to expressions. But Fodor clearly doesn't have this sort of thing in mind when he talks about identifying supervenience properties with *semantic values*. He still, I imagine, thinks the *semantic value* of "cow" will be something like *cows*, not the physical property assigned to "cow" by a compositional-supervenience theory which will surely not be anything like *cows*! I believe that Fodor in talking about identifying the properties assigned to expressions by a compositional-supervenience theory with *semantic values* is merely trying to talk about identifying the physical properties assigned to expressions by a compositional-supervenience theory with *meaning properties*, that is, with properties like the property of *meaning cow* or the property of *meaning that snow is white*.

In one place Schiffer responds to Fodor's talk of *semantic values* in the context of this discussion as if Fodor were talking about what is usually meant by "semantic value". No harm in doing so, over all, but to take Fodor in that way, it seems to me, is more to construct a straw-man than anything else. See Schiffer (1994b), p. 322, note 18.

## Chapter 5 Conventions and Meaning

### 5.1 Introduction

David Lewis offers the following theory of the actual-language relation in his paper "Languages and Language":

- [L] A language L is *used by* a population P if and only if there prevails in P a convention of truthfulness and trust in L, sustained by an interest in communication.<sup>1</sup>

Lewis defines the notion of a *convention* as follows:

...a regularity R, in action or in action and belief, is a *convention* in a population P if and only if, within P, the following six conditions hold. (Or at least they almost hold. A few exceptions to the "everyone"s can be tolerated.)

- (1) Everyone conforms to R.
- (2) Everyone believes that the others conform to R.
- (3) This belief that the others conform to R gives everyone a good and decisive reason to conform to R himself. ...
- (4) There is a general preference for general conformity to R rather than slightly-less-than-general conformity - in particular, rather than conformity by all but any one. ...
- (5) R is not the only possible regularity meeting the last two conditions. ...
- (6) ...the various facts listed in conditions (1) to (5) are matters of *common* (or *mutual*) *knowledge*: they are known to everyone, it is known to everyone that they are known to everyone, and so on.<sup>2</sup>

The notion of *truthfulness in a language* used in [L] is glossed by Lewis in the following way:

To be truthful in L is to act in a certain way: to try never to utter any sentences of L that are not true in L. Thus it is to avoid uttering any sentence of L unless one believes it to be true in L.<sup>3</sup>

And Lewis tells us the following about the notion of *trust in a language*:

To be trusting in L is to form beliefs in a certain way: to impute truthfulness in L to others and thus to tend to respond to another's

utterance of any sentence of L by coming to believe that the uttered sentence is true in L.<sup>4</sup>

To begin my discussion of Lewis's theory I will talk about a certain well-known objection to it which I will argue is not really directly an objection after all. But to see clearly that this well-known objection is not directly an objection sets the stage for stating a somewhat more direct objection which I will raise. Then I will object to Lewis's theory for other reasons as well. Finally I will discuss briefly a theory of the actual-language relation due to Brian Loar which deals with some of the same issues that Lewis's theory has to face, if only slightly more successfully.

## 5.2 Truthfulness-By-Silence

Towards the end of "Languages and Language" Lewis considers a possible objection that someone might raise against [L]. For convenience, I will refer to this objection as [O]:

[O]        *Objection:* Let L be the language of P; that is, the language that ought to count as the most inclusive language used by P. (Assume that P is linguistically homogeneous.) Let L<sup>+</sup> be obtained by adding garbage to L; some extra sentences, very long and difficult to pronounce, and hence never uttered in P, with arbitrarily chosen meanings in L<sup>+</sup>. Then it seems that L<sup>+</sup> is a language used by P, which is absurd.

A sentence never uttered at all is *a fortiori* never uttered untruthfully. So truthfulness-as-usual in L plus truthfulness-by-silence on the garbage sentences constitutes a kind of truthfulness in L<sup>+</sup>; and the expectation thereof constitutes trust in L<sup>+</sup>. Therefore we have a prevailing regularity of truthfulness and trust in L<sup>+</sup>. This regularity qualifies as a convention in P sustained by an interest in communication.<sup>5</sup>

Lewis responds to [O] by claiming that the notion of *trust in a language* that he employs in [L] precludes [O] from presenting a counter-example to [L]. I will not

comment on this claim of Lewis's right now. Rather, I would like to consider the following comments that Lewis makes toward the end of his reply to [O]:

The above objection was originally made, by Stephen Schiffer, against my former view that conventions of language are conventions of truthfulness. I am inclined to think that it succeeds as a counter-example to that view. I agree that  $L^+$  is not used by P, in any reasonable sense, but I have not seen any way to avoid conceding that  $L^+$  is a possible language - it might *really* be used - and that there does prevail in P a convention of truthfulness in  $L^+$ , sustained by an interest in communication.<sup>6</sup>

So I take it that Lewis believes that [O] is a counter-example to a theory - I'll call it "[L']" - which Lewis used to hold:

[L'] A language L is *used by* a population P if and only if there prevails in P a convention of truthfulness in L, sustained by an interest in communication.<sup>7</sup>

[O] and arguments descending from [O] have been taken seriously as objections to both [L'] and [L] by some philosophers, including David Lewis.<sup>8</sup> In the next subsection, 5.2.1, I will argue that [O] fails as an objection to [L']. Afterwards, in 5.2.2, I will make some comments on certain of Lewis's views relevant to the argument in 5.2.1. In 5.2.3 I will indicate why [O] must also fail as an objection to [L]; this will be straightforward given the argument from 5.2.1. In 5.2.4 I will argue that a certain assumption of Lewis's that I allowed for the sake of argument in 5.2.1 is also false. In section 5.3 I will discuss how so many smart people could have come to believe there was an objection to a theory where there was none.

### 5.2.1 Truthfulness-By-Silence and the Theory [L']

Let us first consider [O] only as an objection to [L'].

We are asked in [O] to suppose the following, which I will label [S] for convenience:

[S] (i) a population P uses a language L; and (ii) the language  $L^+$  is a proper superset of L such that each  $L^+$  sentence not also in L is never uttered by a member of P and has some arbitrary meaning.

[O] claims that it follows from [S] that there is a regularity in P of truthfulness in  $L^+$  and that this regularity "qualifies as a convention in P sustained by an interest in communication." For the time being, let us suppose that [O] is correct about its following from [S] that there is a regularity in P of truthfulness in  $L^+$ .<sup>9</sup> I want to examine the further claim made by [O] that this regularity qualifies as a convention. I will argue that it does not.

Recall the six conditions that must hold, according to Lewis, for a regularity to be a convention in a population. The first condition requires that everyone in the population conform to the regularity. Since we are supposing that there is a regularity in P of truthfulness in  $L^+$ , we are, in fact, supposing that this first condition is met. But are the other five conditions met?

Each of the second through sixth conditions requires that everyone in the population have certain propositional attitudes with respect to truthfulness in  $L^+$ . In particular, the second condition requires that everyone in the population believes that everyone else in the population conforms to the regularity. So, if there is a convention among the members of P of truthfulness in  $L^+$ , then everyone in P must believe that everyone else in P is truthful in  $L^+$ . I will now show that it does not follow from [S] that the members of P have even one of the propositional attitudes required for there to be a

convention in P of truthfulness in  $L^+$ . I will construct my argument by focusing on whether it follows from [S] that everyone in P must believe that everyone else in P is truthful in  $L^+$ . I will show that it does not. It will be clear how to use the argument that shows this to show in addition that none of the other propositional attitudes necessary for there to be a convention in P of truthfulness in  $L^+$ , according to Lewis's analysis of convention, can be said to obtain in P given only [S].

It is helpful to unpack [S] using the definition of *convention* that Lewis provides<sup>10</sup>:

[S.1] Everyone in P is truthful in L.

[S.2] Everyone in P believes that the others are truthful in L.

[S.3] This belief that the others are truthful in L gives everyone a good and decisive reason to be truthful in L himself or herself.

[S.4] There is a general preference for general conformity to truthfulness in L rather than slightly-less-than-general conformity - in particular, rather than conformity by all but any one.

[S.5] Truthfulness in L is not the only possible regularity meeting the last two conditions.

[S.6] [S.1] - [S.5] are matters of *common* (or *mutual*) *knowledge*.

[S.7]  $L^+$  is a proper superset of L such that each  $L^+$  sentence not also in L is never uttered by a member of P and has some arbitrary meaning.

If [O] is an objection to [L'], then [C] must follow from [S.1] - [S.7]:

[C] Everyone in P believes that the others are truthful in  $L^+$ .

It is clear that if [C] follows from [S.1] - [S.7] it will have to be because it follows from the conjunction of [S.2] and [S.7]. But [C] does not follow from this conjunction.

Consider the following rough schema [A] where I intend "t" and "t'" to stand for expressions:



- [A] (a) Everyone believes that the others ... t ...  
 (b) t stands in the such-and-such relation to t'  
 (c) Everyone believes that the others ... t' ...

Is any instance of [A] a valid inference? I think there are at most three possible types of cases where there might be valid instances of [A], but the inference to [C] from [S.2] and [S.7] does not have the form of any of these possible types. Let's look at the types of cases and see why they won't help [O].

1. Consider cases where the expressions substituted for "t" and "t'" in [A] are synonymous. Perhaps such cases could be valid instances of [A]. But even if they are, this doesn't help [O] since the expressions "L" and "L\*" are not synonymous - they are not even coreferential.

2. Consider cases where the expressions substituted for "t" and "t'" in [A] stand in some special sort of 'sense relation' other than synonymy, e.g., like that which holds between "bachelor" and "male" such that the inference in [B] is valid, supposing that there is a reading on which it is valid<sup>11</sup>:

- [B] Everyone believes that the others saw a bachelor  
 Everyone believes that the others saw a male

Perhaps such cases would represent valid instances of [A]. If [O] is to be helped by there being instances of [A] that are valid in virtue of such a sense relation between terms, then the sense of "L" will have to be such that it 'contains', so to speak, the sense of "L\*". But keep in mind that the terms "L" and "L\*" are singular terms that each refer to a specific set. If there are to be senses, that is, I take it, concepts associated with these terms that might stand in the relevant kind of 'sense relation', still, I suppose that the concepts will have each to pick out uniquely the appropriate set: the sense or concept associated with

the term "L" will have to pick out uniquely L and the sense or concept associated with the term "L<sup>+</sup>" will have to pick out uniquely L<sup>+</sup>. But, then, what is required is that there are two senses or concepts, call them " $\Phi$ " and " $\Phi^+$ " such that L is the unique  $\Phi$  and L<sup>+</sup> is the unique  $\Phi^+$ , and such that all  $\Phi$ 's are  $\Phi^+$ 's, that is, such that the sense of "L" 'contains' the sense of "L<sup>+</sup>". But then L would have to have  $\Phi^+$ . But since L<sup>+</sup>, by hypothesis, is the unique  $\Phi^+$ , it will follow that L is identical to L<sup>+</sup>. But L cannot be identical to L<sup>+</sup> since by definition L<sup>+</sup> is a proper superset of L. So there cannot be two properties  $\Phi$  and  $\Phi^+$  that will do the work required to provide a valid instance of [A] that will help [O].

3. Consider next cases where we read as *de re* occurrences the terms substituted for the "t" and "t'" of the schema [A]. The first two types of cases that were considered above were supposing a *de dicto* reading of the propositional attitudes in instances of [A]. When an expression in the "that"-clause complement of a propositional attitude verb occurs *de re*, the logical form of the sentence in question is such that the expression takes wide scope with respect to the propositional attitude verb. Thus, when "Cicero" occurs *de re* in "John believes that Cicero is wise", the logical form of the sentence is conveniently paraphrased as "Cicero is such that John believes that he is wise". We might say that the *de re* expression *binds* a position within the "that"-clause, but it has become free of the intensionality of the "that"-clause. One manifestation of this freedom is, famously, that substitution of coreferentials *salva veritate* becomes possible with respect to such expressions.

There are clear cases of valid instances of [A] when the propositional attitudes are

taken *de re* with respect to the terms substituted for "t" and "t'". For example:

Everyone believes that the others think Cicero was cool  
Cicero is Tully  
 Everyone believes that the others think Tully was cool

Reading the occurrences of "Cicero" and "Tully" *de re*, this is clearly a valid inference.

This, however, is no help for [O] since L is not identical to L<sup>+</sup> as Cicero is to Tully.

At this point it should be noted that it looks like every straightforward way of construing the inference from [S.2] and [S.7] to [C] has failed to show that inference to be valid. But there is a non-straightforward way of construing the inference that should be considered.

Consider the following argument:

[D] Everyone believes that the others like some item in L  
L is a proper subset of L<sup>+</sup>  
 Everyone believes that the others like some item in L<sup>+</sup>

If we read only the positions filled by "L" and "L<sup>+</sup>" here as *de re* positions, this argument is not valid. If we paraphrase the argument to accent the effect of the *de re* occurrences of "L" and "L<sup>+</sup>" the invalidity of [D] becomes plain<sup>12</sup>:

[E] (a) L is such that everyone believes that the others like some item in it  
 (b) L is a proper subset of L<sup>+</sup>  
 (c) L<sup>+</sup> is such that everyone believes that the others like some item in it

A counter-example to this argument is the situation where everyone mistakenly believes both (i) that no member of L is also a member of L<sup>+</sup> and (ii) that some people dislike every member of L<sup>+</sup>.

The following would be a valid construal of [D]:

[F] (a) Some item of L is such that everyone believes that everyone else likes it  
 (b) L is a proper subset of L<sup>+</sup>

(c) Some item of  $L^+$  is such that everyone believes that everyone else likes it

Here, the expressions "some item of  $L$ " and "some item of  $L^+$ " are read *de re*, that is, with wide scope with respect to the propositional attitude verb. It doesn't matter whether anyone or even everyone has mistaken beliefs about the relation that holds between  $L$  and  $L^+$ . If both  $L$  is a subset of  $L^+$  and some member of  $L$  is such that everyone believes of it that everyone else likes it, then some member of  $L^+$  is such that everyone believes of it that everyone else likes it.

Can this help [O]? The validity of [F] turns on taking the quantifier expressions "some item of  $L$ " and "some item of  $L^+$ " as *de re* occurrences and giving them wide scope over the propositional attitude verb. But [S.2] and [C] don't have quantifier phrases containing the terms " $L$ " and " $L^+$ ". They contain just the singular terms " $L$ " and " $L^+$ ", and the invalidity of [E] shows that the inference from [S.2] and [S.7] to [C] is invalid, strictly speaking, when " $L$ " and " $L^+$ " alone are taken to occur *de re*. But perhaps we can paraphrase [S.2] and [C] so that the occurrences of the singular terms " $L$ " and " $L^+$ " are replaced somehow with quantifier expressions that contain those terms. And perhaps then we would be able to construe [S] as requiring an argument akin to [F] above which is valid. Okay, what would the appropriate paraphrase of [S.2] and [C] be?

I suggest the following as a first approximation<sup>13</sup>:

- [G] (a) Everyone believes that the others are truthful<sup>14</sup> in each member of  $L$   
 (b)  $L$  is a proper subset of  $L^+$   
 (c) Everyone believes that the others are truthful in each member of  $L^+$

We can paraphrase this for clarity by giving the appropriate quantifier phrases wide scope:

- [H] (a) Each member of  $L$  is such that everyone believes that the others are truthful in it

(b) L is a proper subset of L<sup>+</sup>

(c) Each member of L<sup>+</sup> is such that everyone believes that the others are truthful in it

It is obvious that [H], and therefore [G], is invalid. Suppose that everyone is such that if they live in the U.S., then everyone believes that everyone else likes them. It doesn't follow from this and the fact that the set of persons who live in the U.S. is a proper subset of the set of persons who live in North America that everyone is such that if they live in North America, then everyone believes that everyone else likes them. Suppose that Pierre lives in Canada and that Scott doesn't believe that everyone else likes Pierre. Then, everyone is not such that if they live in North America, then everyone believes that everyone else likes them.

I see no way of coming up with another paraphrase that will do the required job here. And there does not seem to be some other clear strategy for construing the sentences in question so that the inference in question is seen to be valid. I am thereby convinced that [C] simply doesn't follow from [S.2] and [S.7], and that it therefore does not follow from [S]. I conclude further that [O] is not a valid objection to [L].

There are two further points that are interesting to take note of here.

1. Given only [S], the members of P cannot be said to have even one of the propositional attitudes that are required for there to be, according to Lewis, a convention in P of truthfulness in L<sup>+</sup>. The reasoning that shows this will be the same as above: the three straightforward ways of construing the appropriate inferences all fail, and the non-straightforward path of replacing the singular terms "L" and "L<sup>+</sup>" with quantifier phrases that contain those singular terms and reading those quantifier phrases *de re* also fails;

since there are no alternative construals, we must conclude that the inferences are simply invalid. I won't write out the details demonstrating the failure of each such inference since, I believe, the claim here is obvious enough.

2. Nothing really important seems to have happened in my argument *because* of the intentionality of the attitudes in question. The real problem with the inference from [S.2] and [S.7] to [C] seems to be the same as the problem with the inference in [I]:

- [I]    Everyone who owns something owns more than Socrates.  
       Everyone who owns something is someone who is kind to dogs.  
       Everyone who is kind to dogs owns more than Socrates.

This can be symbolized as follows:

- [J]     $(\forall x)(x \in F \supset Rxs)$   
        $F \subseteq G$   
        $\therefore (\forall x)(x \in G \supset Rxs)$

And [J] is clearly invalid. Ultimately what made the inference from [S.2] and [S.7] to [C] fail is just what makes the inference in [J] fail, and that has nothing to do with any intentional features surrounding the relation-term "R". It's simply an invalid inference.

### 5.2.2. Lewis on Attitude Ascriptions *In Sensu Diviso*

Lewis himself seems to have held a view about the attitudes required for conventions of language that might have led him to what I have been talking about as the non-straightforward construal of the inference from [S.2] and [S.7] to [C]<sup>15</sup>, had he tried to work out the details of the inference. In Lewis (1969) he says the following:

Recall what L is: a certain function whose arguments are pairs of a sentence (a finite sequence of types of sounds or of marks) and a possible occasion of its utterance (a pair of a possible world and a spatiotemporal location therein) and whose values are sets of interpretations (pairs of a

code number and a set of possible worlds). Now it is incredible that any ordinary user of L has a concept of any such complicated entity. So how can he be party to any convention regarding L? How can we have expectations and preferences regarding truthfulness in L?

My answer is that he can have them *in sensu diviso*; to do that, he does not need to share our complicated concept of a possible language. ... All he has to do is to come up with the right expectations and preferences regarding particular instances of what we - but not he - would call truthfulness in L.<sup>16</sup>

The notion of reading a sentence *in sensu diviso* can be traced to Aristotle, though Lewis seems to have appropriated the notion from Abelard.<sup>17</sup> It applies to many types of sentences, not just attitude ascriptions. But with respect to attitude ascriptions the notion amounts to nothing more than the notion of reading the ascription *de re* with respect to one or more terms in the ascription's "that"-clause. Earlier in Lewis (1969) when Lewis first introduces his use of the notion of reading ascriptions *in sensu diviso* he makes the following remarks:

Generality *in sensu diviso* is problematic because expectation and the like apply fundamentally to states of affairs. If I expect that each driver will keep right, I do expect a state of affairs: each driver will keep right. But if I expect, *of* each driver, that *he* will keep right, what states of affairs do I expect? "*He* will keep right" does not specify *any* state of affairs until the pronoun has been replaced by some sort of description - verbal, pictorial, or otherwise - of the person in question.<sup>18</sup>

What Lewis is noting about ascriptions taken *in sensu diviso* is that even when a term in the "that"-clause complement of a verb of propositional attitude is given wide scope, the person to whom the attitude is ascribed is still required, somehow, to have some sort of description - or picture, or other representation. Suppose that I expect that each driver will keep right. Reading this ascription *de dicto*, it would seem reasonable to suppose that, put roughly, I need to have some way of representing to myself the general idea of

*each driver*, but it need not be that I have for each driver a representation of that driver. But if we read the ascription *de re*, or *in sensu diviso*, with respect to the phrase "each driver", then we are imagining that for each driver I have some way of representing that driver to myself and I expect that that driver - as represented by my representation - will keep right. I no longer need to represent to myself the general idea of *each driver* as I seemed to when the ascription was taken *de dicto*, but now I need to have as many representations as there are drivers. For present purposes there is no need to worry about just what Lewis thought was problematic here. But we can note the following point.

In the first passage quoted above from Lewis (1969) Lewis seems to be trying to prevent his views from entailing that the average citizen needs to have a representation of the complicated entity that he has defined a language to be. He does this by saying that the attitudes that are required according to his theory of the actual-language relation, [L], be understood *in sensu diviso*. But saying this by itself cannot help Lewis avoid the consequence he is trying to avoid since it will amount only to the requirement that any singular term referring to a language be read *de re* when it occurs in a "that"-clause of one of the conditions required for there to be a convention of truthfulness in the language in question. But that doesn't mean that a representation of the language is no longer required somehow. If I believe that Asa is truthful in L, *de re*, that just means that L is such that I believe that Asa is truthful in it. But, then, L is such that I believe what? "That Asa is truthful in *it*" isn't a state of affairs until, more-or-less, I have some way of representing to myself what *it* is. Thus, if the notion of reading an ascription *in sensu diviso* is to help Lewis to avoid the need for every language user to represent the whole



of the language they use, he is, I believe, going to need to replace references to language via singular terms with references to the sentences of languages via quantifier phrases. Thus, I should be taken as believing not that Asa is truthful in L, but that Asa is truthful in each member of L, where "each member of L" is to be read *de re*. Thus, what is the case is that each member of L is such that I believe that Asa is truthful in it. I still need representations - in fact now I need as many representations as there are sentences in L, and that may be an awful lot - but I don't need a representation of the whole language, and that seems to be what Lewis was after in the first passage that I quoted above from Lewis (1969).<sup>19</sup>

So I take it that there is at least some reason for thinking that the straightforward ways of construing the inference from [S.2] and [S.7] to [C] may not have been as important to consider as the non-straightforward way that I considered since Lewis himself seems, arguably anyway, to want to be understood as referring to languages in attitude ascriptions via quantifier phrases read *de re*<sup>20</sup>. In any event, however, the relevant inferences just don't go through.

### 5.2.3 Truthfulness-By-Silence and the Theory [L]

Up to now I have only considered Lewis's old theory from Lewis (1969) - what I have called [L']. How do things stand with respect to [O] as an objection to the theory of Lewis (1975) - what I have called [L]? If there is a convention in a population P of truthfulness and trust in the language L, sustained by an interest in communication<sup>21</sup>, and if L<sup>+</sup> is a proper superset of L, does it follow that there is a convention in P of

truthfulness and trust in  $L^*$ ? No. And this is so independently of the fancier considerations that Lewis adduces in his reply to the objection [O] in Lewis (1975). For if there is a convention in  $P$  of truthfulness and trust in  $L$ , then there must be at least a convention of truthfulness in  $L$ . And if there is to be a convention in  $P$  of truthfulness and trust in  $L^*$ , then there must be at least a convention of truthfulness in  $L^*$ . It has already been shown that a convention of truthfulness in  $L$  does not entail that there is a convention of truthfulness in  $L^*$ . So if [O] is going to be an objection to [L], it will have to be that it follows from [S.7] and there being a convention in  $P$  of truthfulness and trust in  $L$  that there is a convention in  $P$  of truthfulness in  $L^*$ . But there is nothing in the statement of the conditions that must be met for there to be a convention of trust in  $L$  that helps us get truthfulness in  $L^*$  given truthfulness in  $L$ . So, it follows that [O] cannot be an objection to [L] any more than it was to [L'].

#### **5.2.4 Truthfulness in $L^*$ Given Truthfulness in $L$ , and Truthfulness and Trust in $L^*$ Given Truthfulness and Trust in $L$**

There is still one more point that should be made for this discussion to be complete. Up until now I have assumed that given the description of  $L^*$  stated in [S.7] and the supposition that there is a convention in  $P$  of truthfulness in  $L$ , it follows that there is at least a regularity of truthfulness in  $L^*$ , even if that regularity doesn't amount to a convention of truthfulness in  $L^*$ . But even that much is mistaken.

In essence, I took as a working hypothesis that [N] follows from [K] and [S.7]:

[K] Everyone in  $P$  is truthful in  $L$ .

[N] Everyone in  $P$  is truthful in  $L^*$ .

But the notion of being truthful in a language is defined by Lewis, as we saw above:

To be truthful in  $L$  is to act in a certain way: to try never to utter any sentences of  $L$  that are not true in  $L$ . Thus it is to avoid uttering any sentence of  $L$  unless one believes it to be true in  $L$ .<sup>22</sup>

So we can rewrite  $[K]$  and  $[N]$  as  $[K']$  and  $[N']$  respectively:

$[K']$  Everyone in  $P$  tries never to utter any sentences of  $L$  that are not true in  $L$ .

$[N']$  Everyone in  $P$  tries never to utter any sentences of  $L^+$  that are not true in  $L^+$ .

The three "straightforward" ways of construing the inference from  $[K']$  and  $[S.7]$  to  $[N']$  all fail. Trying a non-straightforward construal leads us to the paraphrases  $[K^*]$  and  $[N^*]$ :

$[K^*]$  Each sentence of  $L$  is such that everyone in  $P$  tries never to utter it unless it is true in  $L$ .

$[N^*]$  Each sentence of  $L^+$  is such that everyone in  $P$  tries never to utter it unless it is true in  $L^+$ .

But  $[N^*]$  does not follow from  $[K^*]$  given  $[S.7]$ . It follows that the assumption is false that there is a regularity of truthfulness in  $L^+$  given a regularity of truthfulness in  $L$ .

Notice, again, that the failure of the inference actually has nothing to do with any intentional features of the verb "to try". The problem is more basic: the inference is simply invalid.

Finally, it should be clear, then, that it will not follow from  $[S.7]$  and there being a regularity of truthfulness and trust in  $L$  that there is a regularity of truthfulness and trust in  $L^+$ .

### 5.3 Truthfulness-by-Silence Versus CMTs

There is no problem directly of truthfulness-by-silence for either Lewis's theory of the actual-language relation in Lewis (1969), or for his theory of the actual-language relation Lewis (1975), even though Lewis himself as well as others have thought that there is such a problem.

There remains the question, why do so many philosophers believe there is such a problem when there is none? My answer is that things can be set up so that, though there is no truthfulness-by-silence problem directly for Lewis's theory, there is a supposition, vaguely motivated by certain epistemological concerns, which can be appended to Lewis's theory and for which there are truthfulness-by-silence style problems. Philosophers who have objected to Lewis's theory by raising truthfulness-by-silence style problems, I believe, were really objecting to the epistemologically motivated supposition. Let me describe the epistemological concerns and the supposition that I have in mind here.

Lewis wanted, as was mentioned above, to avoid attributing to language users all sorts of complicated knowledge about set theoretic stuff. People apparently don't have that sort of knowledge, and Lewis rightly wanted to avoid his theory entailing that they do. To do this he felt obliged to construe the attitudes his theory employs in the non-straightforward way that I discuss above. But construing the relevant attitudes in that way, as we saw, results in Lewis's theory requiring enormous numbers of propositional attitudes of each language user, since languages can be enormously large. But having so many attitudes doesn't seem at all plausible unless there is some relatively small basis for

them. But there are two ways that a finite basis can be given, it would seem, for the appropriate attitudes. On the one hand, it can be supposed that some finite axiomatization is available for each usable language that would entail all the necessary meaning facts about the language: this would amount to the supposition that there is a CMT for each usable language. On the other hand, Lewis can suppose that given the sentences that actually get uttered by a population, the rest of the facts about the language the population uses are fixed: this is the supposition that given the actual usage<sup>23</sup> of a population, the language that the population uses is determined.

It seems that Lewis objected to the CMT route, not because he objected to CMTs in themselves, but because, again, this would require that ordinary language users had complex knowledge of set-theoretic stuff that they apparently don't have. So, the only option available to Lewis is to suppose that actual usage can be an adequate basis for pinning down the entire language that a population uses. The truthfulness-by-silence problems *are* objections to this supposition. They show that this supposition is wildly implausible. The point of the truthfulness-by-silence problems here seems to be a special case of the general worries now often attributed to Kripkenstein.

This, anyway, is how I reconstruct things so that it is not simply out of the blue that philosophers have taken seriously truthfulness-by-silence style objections to Lewis's theories of the actual-language relation. Of course, this reconstruction is not altogether charitable to Lewis since it ascribes to him a view that is pretty implausible, namely, the view that actual usage can determine an indefinitely large language. But perhaps this seems less uncharitable if we suppose that being caught on the horns of the dilemma,

CMTs or actual usage, Lewis grabbed at actual usage and hoped for the best.

But the story doesn't just end here.

Lewis's theory<sup>24</sup> of the actual-language relation commits him to holding that language users have enormous numbers of propositional attitudes. This supposition is implausible without some story about how all these attitudes can be founded in some appropriately small basis. As I have already suggested, this amounts, it would seem, to asserting the disjunction: either all those attitudes hold because some small subset of them hold, or there are CMTs for large languages. But the former disjunct here is utterly implausible, as was discussed above: small sets, by themselves, don't uniquely determine big sets in the appropriate way. So, it seems that for Lewis's theory to be at all plausible is for all usable languages to enjoy CMTs. But, as was argued in the last chapter, even if there were CMTs for all usable languages, standing in some sort of cognitive relation to a CMT does not constitute a necessary condition for meaning,<sup>25</sup> and this is true, *even if* it turns out that all usable languages enjoy CMTs.

So, it seems that Lewis's theory is unacceptable because it seems to require as a necessary condition for language use that language users stand in some sort of cognitive relation to CMTs. This, we have seen is not a necessary condition for language use.

In addition to this, of course, it should be repeated that cognitive relations or not, it is not even entirely clear that there are going to be CMTs for all usable languages. As was argued above in chapter 4, there don't seem to be any good arguments that natural languages in fact enjoy CMTs. So, without independent arguments for CMTs, I would take a theory committed to CMTs to be somewhat tenuous at best.

Independent of all this there are other reasons to be doubtful of Lewis's theory of the actual-language relation. I will discuss these now.

#### 5.4 Conventions and Interest in Communication

In this section I will briefly discuss two sorts of objections to Lewis's theory of the actual-language relation that I think are important. The first has to do with Lewis's notion of convention and the second with the "interest in communication" clause of his theory.

1. The first sort of objection that I want to discuss here addresses itself to Lewis's theory of the actual-language relation by way of addressing itself to Lewis's analysis of the notion of *convention*. There are actually a few such objections but I will only mention those I think most telling.

One of the objections of the sort I have in mind is due to Tyler Burge. Lewis's analysis of the notion of *convention* requires that it be *common knowledge* among the parties to a convention that there is an alternative regularity that could have been adopted as a convention which would have served just the same, or near enough, purposes as the convention which they in fact have adopted. But Burge argues rather cogently that not only is this requirement of *common knowledge* of alternative regularities too strong, but the necessity of alternative regularities is, common knowledge of them or not, too strong.<sup>26</sup>

...[C]onsider the...example of the sentimental hat tippers. The convention of tipping one's hat to a passing stranger becomes a national trademark. The citizens are sentimentally attached to this mode of greeting and its associations with their culture, to the extent that each would rather fight

for the traditional greeting, or give up greeting strangers altogether, than switch to another one, even if the others were to switch. ...[T]his traditionalism does not seem to affect the conventionality of the actual practice.<sup>27</sup>

I think this sort of story works rather well as a counterexample to Lewis's account of the notion of *convention*.

Stephen Schiffer offers the following gloss of the notion of *convention*:

There prevails in [population] G a convention to do an act (or activity) of type X when (or only when) . . . iff it is mutual knowledge<sup>\*</sup> amongst the members of G that

- (1) there is a precedent in G for doing X, or an agreement or stipulation that one will do X, when (or only when) . . . ;
- (2) on the basis (in part) of (1), almost everyone in G expects almost everyone in G to do X when (or only when) . . . ;
- (3) because of (2), almost everyone in G does X when (or only when) . . .<sup>28</sup>

This account of the notion of *convention* differs from Lewis's in a number of obvious regards. Among these is the lack of Lewis's requirement that more-or-less equally attractive alternatives to regularities be available to the members of a population for the existence of a conventional regularity to prevail among them.<sup>29</sup> Schiffer's account of convention, therefore, would seem to escape Burge's counterexamples to Lewis's account. But Schiffer's account of convention shares other problems with Lewis's which I will get to in a bit.

Grandy, in his review of Lewis (1969), after noting Burge's arguments also suggests a counterexample to the view that language is a matter of *convention* in the sense of Lewis's analysis:

It is clear that...ordinary use applies 'convention' to regularities that do not fit Lewis's analysis. For example, suppose that, in Orwell's 1984



society, when the Newspeak language was introduced almost everyone detested that language and would have much preferred not to speak it, but, because of the power of the state, everyone spoke the language out of fear of the consequences. There is a common sense of 'convention' in which it is a convention that these people speak the language, but they clearly do not fit Lewis's conditions.<sup>30</sup>

Of course, it is possible to suppose that Grandy's Orwellian society does not really use Newspeak *by convention*, regardless of the loose sense in which we suppose that *any* use of language is conventional. This supposition would protect Lewis's analysis of *convention*. But it would seem that this is not a supposition open to the theorist who wants to understand *meaning* in terms of the notion of *convention* since the case does seem a genuine case of language-use and therefore of *meaning*. A theorist who denied that the language-use here was a matter of convention would have to allow that some cases of expression-meaning are non-conventional. Since Lewis wants to hold that meaning is a matter of convention and is gearing his analysis of *convention* to just that sort of thesis, the only alternatives available to him are to deny that the Newspeak language is really being used or to amend his analysis of *convention*.

Grandy's case, I believe, is also a counterexample to Schiffer's view of *convention*. For since the members of Grandy's Orwellian society speak Newspeak not because they expect everyone else to do so but because they are afraid not to, it cannot be that any of the members of the society *know* that the others speak Newspeak because they expect the others to. We can even add to Grandy's story that everyone knows that the others speak Newspeak because they are afraid not to. So Schiffer's condition (3) is not satisfied: the members of the society don't mutually know\* that the others speak Newspeak because they expect everyone else to. So Schiffer must either hold that Grandy's Orwellians don't

use Newspeak, or that some cases of language use are non-conventional, or that his analysis of *convention* is amiss. The first two alternatives don't seem available directly to Schiffer, in Schiffer (1972) anyway, so he seems to need to revise his analysis of *convention*.

The real problem, in my view, for both Lewis and Schiffer with respect to Grandy's case is the strength of the *common knowledge* condition.<sup>31</sup> For this requires that each person who is party to a convention believe about all the others who are party to the convention that they participate in the convention for certain reasons. But, at least with respect to language-use, it seems that the sorts of reasons we can attribute to others for speaking our language can vary quite a bit, and yet the others still speak our language. If common knowledge of the motives of others in being party to a conventional regularity is necessary for *conventional regularities*, it seems clear that language-use cannot be a matter of conventional regularity. Believe anything you want about why I speak English and you will still speak the same language as me if you are an English-speaker too.

Now this doesn't mean that *common knowledge* is not required for conventions. Lewis and Schiffer both have reasons for motivating their respective common knowledge conditions.<sup>32</sup> So, perhaps the moral to draw is that the conditions for something being a *convention* are, on reflection, such that we shouldn't want to say that language-use is really a matter of convention. I will not attempt a view on this matter here at all. I just point it out.

I believe there are other good counterexamples to Lewis's analysis of the notion as well,<sup>33</sup> and my point in mentioning all this is to help diminish the sense that we should

really want to make free use of the notion of *convention* in a theory of meaning. The use of the notion of *convention* will still remain something of an explanatory debt even if a satisfying theory of meaning availing itself of that notion were provided. Granted that it is a platitude that language is conventional, still, this need not entail that a theory of meaning need use a notion of *convention*. The use of such a notion is not wholly unproblematic and it might be hoped that a theory of meaning could be constructed using other notions and that, when this is done, the *conventionality* of language can be explained somehow less formally in terms of this theory.

Thus, the absence of a completely satisfying analysis of the notion of *convention* might make a theorist slightly less eager to use the notion of *convention* in constructing a theory of meaning.

2. Another important objection to Lewis's theory of the actual-language relation is due to Stephen Schiffer. Lewis's theory of the actual-language relation has it that a population uses a language just in case there prevails among them a convention of truthfulness and trust in the language *sustained by an interest in communication*. It is this "sustained by an interest in communication" clause - I will call it the *communication-clause* for convenience - that the present objection deals with.

Schiffer claims that the communication-clause was added by Lewis to avoid a certain sort of counterexample.<sup>34</sup> Without the communication-clause Lewis's theory would simply hold that a convention of truthfulness and trust in a language were necessary and sufficient for a population to use the language. To be truthful in a language is to try not to utter the language's sentences unless they are true in the language and to be trusting

in L is to suppose that others are truthful in L. But, in that case every convention can be made out to be one of truthfulness and trust in a language. For, suppose that there is a convention to drive on the right side of the road among a certain group of people P. The act of driving paired with the proposition that I am driving on the right (ignore the indexicality here) will be a language, call it D, with one sentence. Since we are supposing that there is a convention among the members of P to drive on the right side of the road, then each member of P will avoid driving unless they are driving on the right, or, in other words, they will try to drive only if they are driving on the right. To say that they avoid uttering the single D-sentence unless it is true in D is just another way of saying that they avoid driving unless they drive on the right. And that is to say that the members of P are truthful in D. And since they each expect the others adhere to the convention of driving on the right, they are also trusting in D. So, there would seem to be a convention of truthfulness and trust in D. But it also seems that merely having a convention of driving on the right is not to speak a language. Thus, something is badly lacking in the theory that says that a convention of truthfulness and trust in a language is sufficient for using the language in the relevant sense.

Lewis, according to Schiffer, added the communication-clause in order to avoid this sort of counterexample. But as Schiffer also points out, the communication-clause is not sufficient to avoid this sort of counterexample completely. For, suppose that the members of our group P drive on the right because they believe that if they don't their ability to communicate with each other will suffer somehow, perhaps because they have some complex astrological beliefs that correlate driving on the right with communication.

In that case their convention seems sustained by an interest in communication, but they still don't speak a language. The notion of *an interest in communication* is apparently too vague or general to be of good use to Lewis. And it is hard to imagine how any rewording of the communication-clause alone can block counterexamples of the present sort. Schiffer claims the following about this matter:

Talk of trying not to utter a sentence of *L* unless it is true in *L* will perforce give way, I submit, to talk of not uttering a sentence  $\sigma$  unless one *means* the proposition  $L(\sigma)$ .

This seems right. The sort of convention Lewis is after seems to be a convention of *speaker-meaning* of some sort. It is hard to see how a theory that doesn't avail itself of some notion of speaker-meaning will manage to get things right.

### 5.5 Loar's Actual-Language Relation

I would like to discuss briefly another account of the actual-language relation that is due to Brian Loar.<sup>35</sup> Loar's theory uses a notion of *convention* like Lewis's<sup>36</sup>, but unlike Lewis, Loar uses a notion of *speaker-meaning*. So Loar's theory can be seen, perhaps, as a theory that tries to answer the problem, just discussed, that Schiffer raises for Lewis's theory with regard to Lewis's *communication-clause*.

Basically, Loar's theory is, in a slightly simplified paraphrase:

P uses L just in case there prevails among the members of P a convention such that for any sentence  $\sigma$  and proposition  $\pi$  such that  $\pi=L(\sigma)$ , if a member of P *seriously* utters  $\sigma$ , then that person means  $\pi$ .<sup>37</sup>

The notion of a *serious* utterance here is meant to filter out cases where a speaker utters a sentence non-literally or for some non-communicative purpose, and other such cases.

I am not entirely sure that such a notion can be appropriately spelled out, but I will accept it in the present discussion to make other points.

This theory clearly avoids the counterexample Schiffer raised against Lewis's theory with regard to Lewis's communication-clause. But it retains, pretty much, the same spirit of Lewis's early theory, [L']. Recall that Lewis says that to be truthful in a language L is "to avoid uttering any sentence of L unless one believes it to be true in L". That is, roughly, Lewis's theory has it that language-use is a matter of there being a convention such that one tries to make it a necessary condition for uttering a sentence that one believes it expresses in the language a true proposition. Loar's theory has it that language-use is a matter of there being a convention such that a necessary condition for uttering a sentence is that one means the proposition it expresses in the language. So, there is something of a parallel between the two theories: they both require conventional regularities describable in terms of material conditionals stating a necessary condition for uttering a sentence of a language.

Loar, in Loar (1976) at any rate,<sup>38</sup> feels, like Lewis, that it is best to avoid requiring language users to have some sort of representation of the whole of the language that they use. We can take that to mean that Loar wants to avoid requiring language users to stand in some sort of cognitive relation to a CMT for their language. Thus, Loar suggests, though I state this in my terms, that the reference to the language within the scope of the propositional attitudes that make up the convention he describes be read as a *de re* quantifier expression which deal with each of the sentences of the language individually. So, effectively, Loar wants his theory to be read as follows:

P uses L just in case for each sentence  $\sigma$  and proposition  $\pi$  such that  $\pi=L(\sigma)$ , there prevails among the members of P a convention such that if a member of P *seriously* utters  $\sigma$ , then that person means  $\pi$ .

Since Loar eschews the idea of language-users standing in cognitive relations to CMTs, he suggests that sense can be made of the enormous number of attitudes required by this theory by founding the infinitude of attitudes in some large but finite corpus of actually uttered sentences. His basic idea is that there will be some large but finite set of actually uttered sentences of a language used by a population and that this set will determine, somehow, grammatical rules that in turn determine the rest of the language. This, of course, is a requirement that there be a CMT for the language used, but Loar's scruples about CMTs were about ordinary language-users *knowing* such theories, not about natural languages *enjoying* them. In any event, Loar requires that a CMT for a language be determined by some suitably large finite corpus of actual utterances by members of the population in question.<sup>39</sup> The idea here is, I think, highly implausible for reasons Schiffer spells out in response to a suggestion by Lewis which is essentially the same as the suggestion presently under consideration.<sup>40</sup> The problem, in a nut-shell, is just the one that you would expect: a finite corpus will determine infinitely many incompatible CMTs and there are no good standards for picking out one of these. In other words, a finite corpus will determine infinitely many languages. I think the arguments against this proposal are compelling and I won't dwell on it further here.

Loar thinks CMTs will be required in a theory of meaning for another reason as well. He raises an important counterexample to the above theory.<sup>41</sup> It is plausible that no one would utter the English sentence 'Apollo set the evening sky ablaze' with the

intention of trying to get an audience to believe that, literally, Apollo set the evening sky ablaze. For Loar, that means that it is plausible that it is unlikely that anyone would utter that English sentence to *mean* that Apollo set the evening sky ablaze. But, still, it is possible, and let's suppose that it is true, that English speakers are such that were anyone to utter that sentence, what they would be trying to get their audience to believe is something like that there is an intensely beautiful sunset this evening. Consider the language English' which is such that 'Apollo set the evening sky ablaze' is paired with the proposition that there is an intensely beautiful sunset this evening. Notice that on present assumptions, if Loar's theory is correct, then English-speakers actually use English' and not English. For English-speakers are not such that utterance of 'Apollo set the evening sky ablaze' would be a sufficient condition for *meaning* that Apollo set the evening sky ablaze, but they are such that it would be a sufficient condition for *meaning* that there is an intensely beautiful sunset this evening. Thus, it appears that Loar's theory fails to provide a sufficient condition for expression-meaning.

Loar's diagnosis of the problem here is that his conditions for literal meaning are not sensitive to the contributions to meaning made by syntactic structures and simple lexical items employed in sentences. 'Apollo set the evening sky ablaze' does literally mean among English-speakers that Apollo set the evening sky ablaze, and it seems to mean this among us independently of what actual communicative intentions we might have on any actual occasion of its utterance: it seems to mean what it does because the words and syntactic structures that make it up contribute to its meaning in ways that are set in advance of any actual utterance of it. Loar suggests, on the basis of this sort of



diagnosis, that the theory of meaning will have to evoke a grammar for a language, that is, a CMT for the language, somehow.<sup>42</sup>

But there is another diagnosis for the problem here. It could be that while Lewis's notion of a convention of truthfulness *sustained by an interest in communication* is too broad a notion for the theory of meaning, the notion of *speaker-meaning* understood as a sort of intention to get an audience to have a certain belief is too narrow. Perhaps what is needed is a notion of speaker-meaning which will allow that in cases where one speaks metaphorically or non-literally by exploiting features of the literal meanings of sentences, as in the 'Apollo...' case one *says* the literal meaning though one *suggests* or *implicates* what one really wants to get the listener to believe. The standard architecture of Gricean analyses of speaker-meaning isn't generally understood to allow for such a notion, though it is an intuitive enough one. One can *say* one thing and yet not *mean* it. Perhaps, then, this sort of notion of *saying* is better suited to a theory of expression-meaning. I make this point here only to leave it alone for the time being. But I will return to it in my final chapter.

## 5.6 Summary

Lewis wants to avoid requiring language users to know lots of set-theoretic stuff. To do this he seems compelled to read the attitudes towards languages that his theory of the actual-language relation requires as *de re* attitudes towards each of the sentences of these languages. But this ends up requiring that language users need to have an enormous number of propositional attitudes. This new requirement only seems plausible if either

there are CMTs for usable languages or the large numbers of attitudes could be had somehow by only having some small subset of them. But the second of these disjuncts is wildly implausible and truthfulness-by-silence problems aim at showing this. Most of this chapter was devoted to making this point. But it follows from all of this that the more serious point is that a Lewis-style theory will not be able to avoid commitment to language-users having cognitive relations to CMTs. But since it was already seen in chapter 4 above that having a cognitive relation to a CMTs is not necessary condition for using a language, it was concluded that, whatever else Lewis's theory does, it does not state a necessary condition for using a language.

This point being made, I went on to mention two further issues with respect to Lewis's theory of the actual-language relation. First, I noted that there are problems with Lewis's and Schiffer's analyses of the notion of *convention* and that to use this notion will incur some explanatory debt. And second, I noted that it seems unlikely that a theory in style like Lewis's theory can succeed without deploying some notion of *speaker-meaning*.

Finally, I discussed Brian Loar's theory of the actual-language relation which seems to address the second of the two issues just mentioned. But, I suggested that the notion of *speaker-meaning* it suggests turns out to be too strong and that perhaps a weaker notion will be required.

In the next chapter I will discuss a type of theory that tries to do without CSTs and without the use of the notion of *convention*, but it also tries to work without directly tying expression-meaning to notions of communicative intention.

## Notes

1. Lewis (1975), p. 169.
2. Lewis (1975), p. 165.
3. Lewis (1975), p. 167.
4. Ibid.
5. Lewis (1975), p. 187.
6. Ibid.
7. Lewis (1969), p. 177.
8. See Hawthorne (1989), Lewis (1991), Hawthorne (1992), and Schiffer (1993). In Loar (1976), p. 159 and in Loar (1981), p. 256, Loar addresses a related problem for a proposal of his of the actual-language relation. The solution he suggests in those places is essentially the same as that suggested later by Lewis in Lewis (1991). See section 5.5 below for a brief discussion of Loar's theory of the actual-language relation and his solution to the present problem. Martin Davis also raises a version of the problem and offers Loar's solution to this problem in Davis (1981), pp. 15-16. Davis cites Platts (1979) and Loar (1976) as sources of the discussion of this problem and explicitly credits Loar (1976) with the solution he suggests.
9. I will take issue with this claim in the section 2.2.4 below.
10. Since right now I am considering only whether [O] states a counter-example to [L'], I will talk only of a convention of truthfulness in a language and not of a convention of truthfulness and trust in a language. I will discuss [O] as an objection to [L] later.
11. I am not really sure that I think [B] is valid on any reading except, perhaps, where the phrases "a bachelor" and "a male" are read *de re* - and even there, the notion of validity is some sort of informal notion, obviously, and therefore somewhat questionable to my mind. But, suppose Edna believes that Gilbert is a bachelor because she heard David say that Gilbert was a bachelor. But for some odd reason she believes that Gilbert is female. Or suppose that Phil believes that he has arthritis because his doctor told him that he does, but he does not believe that he has a disease of the joints because he has some odd belief about the nature of arthritis. These would be cases where the inference read *de dicto* would seem to fail. Anyway, I consider these cases for the sake of thoroughness, but I am prone to be sceptical of a notion of validity based on 'sense-containment' relations. See Katz (1986) for a discussion of such cases.

12. Quine's notation in Quine (1955) is also helpful for seeing the point that I make here as well as some points that I will be making below.
13. See Loar (1976), p. 158, Loar (1981), p. 256, and Schiffer (1987), p. 259, where they paraphrase different but related attitudes with *de re* quantifier expressions as I do here.
14. Recall that a language is understood as a function from expression types to propositions. A single pair, therefore, of a sentence type and a proposition will be a language. Thus, I can speak of truthfulness in a single such pair.
15. As well as the other inferences required for it to follow from [S] that there is a convention of truthfulness in  $L^+$ .
16. Lewis (1969), pp. 182-183. Lewis is using a notion of language here that is slightly more complicated than the one he uses in Lewis (1975) and that I am using, but that doesn't affect anything in the present discussion. The same objection is considered in Lewis (1975), pp. 180, and Lewis answers it in pretty much the same way there as he does in Lewis (1969).
17. See Heytesbury (c. 1339a), Heytesbury (c. 1339b), Bocheński (1961), Knuuttila (1982) and Tweedale (1982). I thank Tereza Saxlova for the last two of these references. Also helpful are Chisholm (1962), Sosa (1970), and Geach (1964).
18. Lewis (1969), p. 65. Lewis cites Quine (1955), Montague and Kalish (1959), Lewis (1968), and Kaplan (1969) at the end of the paragraph from which this quotation is taken. But none of these papers, it seems to me, sheds any great light on what Lewis is really bothered about.
19. Nothing that I am saying here is meant to suggest that there is nothing problematic with the view that I am ascribing to Lewis. I think that there is quite a bit that is problematic with it. But that doesn't matter to the point that I am making right now. See Loar (1976), p. 158 and Loar (1981), p. 256, for an obvious but serious problem with the view that I am ascribing to Lewis.
20. In Loar (1976) Loar indicates that he understands Lewis as I have: "David Lewis has suggested that the mutual knowledge of the relevant regularity involves knowledge of all the sentences of  $L$  *in sensu diviso*...." (p. 158). See also Loar (1981), p. 256. And, again, see Schiffer (1987), p. 259, where Schiffer runs through an argument like mine that concludes, more-or-less, that Lewis needs to replace references to languages via singular terms with references to languages via *de re* quantifier phrases.
21. Henceforth I will leave off the "interest in communication"-clause. I will say some things about this clause in section 5.3 below.
22. Lewis (1975), p. 167.

23. When I speak of *actual usage* here, I am not using "usage" in the more comprehensive way in which it is sometimes used in which it includes all sorts of internal sentence-processing matters. I mean something wholly external here.

24. From now on I will just speak of Lewis's *theory*, even though he had more than one: what I say from here on actually applies to all of them.

25. I have in mind here Schiffer's counter-example to this view whereby processing, and thereby meaning, proceeds via a non-semantic translation device of some sort.

26. See Burge (1975).

27. Burge (1975), p. 252. There are other counter-examples to Lewis's notion of *convention* in this paper as well, and I think that they are all quite good.

28. Schiffer (1972), p. 154. See Schiffer (1972), pp. 30-31 for Schiffer's definition of his notion of *mutual knowledge*. For present purposes Schiffer's *mutual knowledge* can be read as the same as Lewis's *common knowledge*.

29. See Schiffer (1972), chapter 5. Schiffer's gloss of *convention* is found there on p. 154.

30. Grandy (1977), p. 133.

31. I will speak in terms of "common knowledge", but I am to be understood as talking about both Lewis's *common knowledge* and Schiffer's *mutual knowledge*.

32. See Lewis (1969), p. 59 and Schiffer (1972), p. 150.

33. See, for example, Gilbert (1983). Perhaps Schiffer's account of *convention* escapes Gilbert's problems for Lewis's account. For more technical problems with Lewis's use of game theory in motivating the account of *convention* he ultimately settles on, see Gilbert (1981).

34. See Schiffer (1993), pp. 232-233.

35. See Loar (1976), pp. 151-161 and Loar (1981), pp. 256-260. Loar's theory is related in important respects to Schiffer's theory of expression-meaning in Schiffer (1972), chapters 5 and 6. It is also somewhat related to the so-called "IBS" accounts of expression-meaning that Schiffer discusses in Schiffer (1987a), pp. 249-255 and Schiffer (1993), pp. 239-242, though these accounts don't explicitly use the notion of *convention*.

36. He refers readers to Lewis (1969) and Schiffer (1972), chapter 5 for the details of the notion of *convention*.

37. Loar (1976), p. 153. Loar, later in Loar (1976) and in Loar (1981), unpacks the notion of *serious circumstances* he uses here. I am not entirely sure that Loar's notions of being *free for meaning* a proposition and of *associative* and *non-associative improbabilities* which he uses to clarify the notion of *serious circumstances* are unproblematic. But since I won't take issue with Loar here about this I won't bother using the unpacked version of his theory in my discussion.

38. Loar seems to have changed his mind about this somewhat by Loar (1981).

39. This is essentially the suggestion that Lewis makes much later in Lewis (1991).

40. See Schiffer's discussion in Schiffer (1993), pp. 232-239.

41. See Loar (1981), p. 257. Loar didn't discuss this sort of counterexample in Loar (1976).

42. In his earlier piece, Loar seems to think that talk of cognitive relations to CMTs is out of place in a theory of meaning, even if it could be that, in fact, we do use our languages by standing in such relations to CMTs: "There may be a Chomsky sense of knowledge - having an internal representation - in which a speaker knows the rules of his language, but that is a psychological hypothesis and, however reasonable it is, we do not want to build it into an *explication* of what it is for L to be the language of P" (Loar (1976), p. 158). But in Loar (1981), Loar suggests that a fuller theory of meaning cannot be provided until we know more about our own psycholinguistics and how we might be related to CMTs for languages: "I suggest the Chomskyan idea of the internalization of the generative procedures of a grammar has got to be invoked to make sense of the entrenchment of a grammar, and therefore to make sense of literal meaning. The exact force of this can't be spelled out antecedently to a detailed psycholinguistic theory" (Loar (1981), p. 259). Thus, Loar seems to be going back on the earlier view that he seemed to have had about people standing in cognitive relations to CMTs and how this sort of possibility should figure into a theory of meaning.

## Chapter 6 Translational Theories of Meaning

### 6.1 Introduction

In chapter 2 I suggested that there are at least three important traditions in twentieth-century theorizing about meaning: the *intention-based*, the *causal*, and the *compositional-semantic* traditions, as I called them. These traditions, I maintain, are not mutually exclusive and they intertwine in various ways. The present chapter is essentially about a, historically speaking, somewhat less important tradition, but a conceptually no less compelling one that also can be seen as intertwinable with the others. I will call this tradition the *translational tradition in the theory of meaning*. In this tradition, the basic idea is that meaning is to be explained in terms of relations of speakers to translation schemes of some sort which provide translations of public-language sentences into Mentalese. Let me begin by telling something of this tradition's recent history before I discuss the details of a theory of meaning which belongs primarily to it.

First a little of the prehistory. Quine is probably a central figure in the prehistory of the translational tradition that I have in mind now. For he seems to take the task of understanding the utterances of others in terms of an interpreter translating those utterances into the interpreter's personal idiolect. Quine speaks of the construction of translation manuals to aid in the *radical translation* situation, but he seems to expect that the work of interpreting in less radical situations is more-or-less the same:

Thinking in terms of radical translation of exotic languages has helped make factors vivid, but the main lesson to be derived concerns the empirical slack in our own beliefs.<sup>1</sup>

Quine, however, doesn't take the "target" language of the translations that make for

interpretations to be Mentalese. So I hesitate to see Quine as aiming at a *translational theory of meaning* in the present sense.<sup>2</sup> Still, the idea in Quine seems to be that language-processing can be understood in terms of possession somehow of a translation manual that takes you from utterances in the language of another to expressions in a language that you have some sort of pre-interpretational understanding of and vice-versa. And that is a view not far from the translational theorist's.

Another important factor in the prehistory of the translational tradition is the history of the sort of 'semantic' theory that began with Katz and Fodor's paper "The Structure of a Semantic Theory".<sup>3</sup> The scare quotes around 'semantic' are there, in fact, because today the Katz/Fodor-style 'semantics' is generally understood to be less a *semantic* theory, that is, less of a theory that says what expressions mean or denote, than a theory that translates natural-language expressions into a special symbolism of what Katz and Fodor called *semantic markers* which can be used to determine certain of the semantic features of the original expressions.<sup>4</sup> I'll return to this point in just a bit. But first I will say a little about what a Katz/Fodor-style theory is supposed to do.

The special symbolism of *semantic markers* is sometimes referred to today as "Markerese". The idea of Katz/Fodor-style theories, then, is that given the Markerese translations of natural-language expressions, certain semantic features of the natural-language expressions are supposed to be determinable. For example, in principle, it is supposed to be possible to determine whether a natural-language sentence is analytic or not, or whether it has a meaning at all, given just its Markerese translation. And given the Markerese translations of two separate sentences it is supposed to be possible to



determine whether the sentences are synonymous or not. The semantic competence of natural-language users is, according to Katz and Fodor (1963), supposed to be characterizable in terms of some such a translation scheme along with definitions in terms of Markerese expressions alone of a certain class of semantic features and relations, for example, *analyticity*, *meaningfulness*, *meaninglessness*, *synonymy*, etc.<sup>5</sup>

Beginning, as far as I can tell, with Bruce Vermazen's review, Vermazen (1967), a popular criticism of the Katz/Fodor-style theories began to circulate in philosophical literature. This criticism has it, more-or-less, that whether or not they could ever succeed in being able to predict the extensions of all those notions in the class of semantic features and relations with which it explicitly deals, such theories do not try to *say* what the meanings of expressions are; and *saying* what the meanings of expressions are is what a theory called a "semantic" theory should do: you can know the Markerese translation of any sentence you like and all the things Katz and Fodor say that you can figure out on the basis of this translation, but all that wouldn't entail that you knew what the meaning of the sentence is. So the criticism goes and so it got repeated in various forms in quite a number of places.<sup>6</sup> And for many philosophers, it seems, this sort of criticism settled the matter with respect to theories of meaning that relied on translation and so such theories were largely dropped from discussion in philosophical literature through most of the 70's and 80's.

One final aspect of the prehistory of the translational tradition is probably good to mention and this aspect will bring us right into the actual history I am aiming for.<sup>7</sup> Computer scientists often talk in terms of the 'semantics' of higher-level programming

'languages' - languages like C, PROLOG, etc. - as being given by the *'translators'* that take programs written in a higher-level language into a computer's executable 'machine-language'.<sup>8</sup> This talk by computer scientists was originally, perhaps, metaphorical, but George Miller and Philip Johnson-Laird suggested that human language processing could be understood in just such terms.<sup>9</sup> In fact, they took the computer metaphor seriously enough, it seems, as to even suggest that the semantics of natural-languages should be spelled out in procedural terms:

In our view ... [the] broader psychological theory of linguistic competence must be formulated in procedural terms.<sup>10</sup>

Fodor dropped the proceduralism of Miller and Laird-Johnson and suggested that natural-language understanding could be accounted for by a theory which involves translation of natural-language sentences into the LOT:<sup>11</sup>

...[I]t is characteristic of the organization of general purpose digital computers that they do not communicate in the languages in which they compute and they do not compute in the languages in which they communicate. The usual situation is that information gets into and out of the computational code via the operation of compiling systems which are, in effect, translation algorithms for the programming languages that the machine 'understands'. ...[I]f the view of communication that I have been commending is true, then these remarks hold, in some detail, for the mechanisms whereby human beings exchange information via natural languages. To all intents and purposes such mechanisms constitute 'compilers' which allow the speaker/hearer to translate from formulae in the computational code to wave forms and back again.<sup>12</sup>

Fodor also argued that the Vermazen-style arguments against Katz/Fodor-style theories were irrelevant to this claim about natural-language processing. It might be true that knowing the M-translation alone wouldn't *tell* you the meaning of the sentence it translated, but if the M-translation were processed in the appropriate way this wouldn't

matter since understanding itself is constituted by processing of M-sentences according to the LOT theory. Understanding a public-language sentence, on the LOT view Fodor has argued for, is a matter of standing in a certain computational relation to an M-sentence token which is the result translation from the public-language sentence. This understanding is constituted, we might say, not by *interpreting* an M-sentence, but by *processing* it.<sup>13</sup>

Thus, we might characterize the Fodor view more generally in the following way. For a population to speak a language is for the members of the population to stand in some sort of cognitive relation to a translation manual that takes sentences of the language and provides their M-translations. This is the central thesis, roughly put, of the translational tradition that I am trying to call attention to here. A bit later in this chapter I will give a slightly more rigorous gloss of this thesis, but for now this will do.

The question of Mentalese expression-meaning now becomes prominent since, essentially, if a translational account of public-language expression meaning is correct, then public-language expression-meaning really depends on Mentalese expression-meaning. I will discuss this question below as well.

I don't know whether anybody picked up on the theme of translational theories of meaning between Fodor (1975) and Schiffer's recent paper "Actual-Language Relations" (Schiffer (1993)).<sup>14</sup> Schiffer's attraction to a translational theory can be understood in terms of his desire to do without compositional-semantics in the explication of the notion of *expression-meaning*. Schiffer provides what appears a somewhat more rigorous statement of the translational theory of meaning suggested by Fodor in Fodor (1975).

But, in order to make his version of the theory more generally acceptable, he tries to do away with the empirical aspects of Fodor's LOT hypothesis by suggesting that there is a sense in which the LOT hypothesis is either trivially true or merely a useful but harmless metaphor.<sup>15</sup> I believe, as I argued in chapter 3 above, that Schiffer is right about the LOT hypothesis and that it can, indeed, be given a reading in which it is trivially true, or, at least, as trivially true as the supervenience of propositional-attitude properties on physical properties.

But I believe that Schiffer's translational theory of the actual-language relation is untenable and will argue so in this chapter. First, I will discuss Schiffer's theory and some of the motivations for its details. Then I will state what appears to be a rather serious counterexample to it and some further problems for it. I will discuss why I think that it is unlikely that Schiffer's theory is reparable given my counterexample. And after all of that is done, I will mention another important counterexample to Schiffer's theory that works wholly independently of the main one I discuss in this chapter.

## 6.2 Schiffer's Theory of the Actual-Language Relation

Before providing his theory of the actual-language relation in his paper "Actual-Language Relations", Schiffer defines two technical notions that he employs in the theory. These are the notion of a *practice in [a population] P of meaning in [a language] L* and the notion of an *L-determining translator*.

Schiffer's definition of the first of these notions is as follows: for any population P and language L,

- [P] [T]here is a *practice in P of meaning in L* iff often when a member of *P* means *q*, for arbitrary *q*, he does so by uttering some sentence that means *q* in *L*.<sup>16</sup>

The notion of an *L-determining translator* is defined by Schiffer as follows:

- [L] An *L-determining translator* is a language-processing mechanism that determines a mapping of each sentence of *L* onto a Mentalese sentence that means in Mentalese what the *L* sentence means in *L*.<sup>17</sup>

Schiffer's states his theory of the actual-language relation, then, as follows:

- [T] A language *L* is *used by* a population *P* iff there is a practice in *P* of meaning in *L* and the processing of *L* utterances proceeds via an *L-determining translator*.<sup>18</sup>

I would like to give names to the two conjuncts on the right of the biconditional of [T]. I will call the first conjunct, "there is a practice in *P* of meaning in *L*", the *employment-clause*, and the second conjunct, "the processing of *L* utterances proceeds via an *L-determining translator*", the *whole-language-clause*. Let me discuss these in turn.

I will assume for the sake of argument throughout that the employment-clause states<sup>19</sup> something that is a necessary condition for a population to use a language. It is useful to see why, by itself, however, the employment-clause does not state a sufficient condition for a population to use a language.

Suppose that there is a practice in a population  $P^*$  of meaning in a language  $L^*$  and that there is no language  $L^*$  that  $P^*$  uses such that  $L^* \not\subseteq L^*$ . According to the definition [P] this means, closely enough, that often when a member of  $P^*$  wants to communicate some proposition  $\pi$  to someone, he or she will utter an  $L^*$  sentence that means  $\pi$ . But even if there is a practice in  $P^*$  of meaning in  $L^*$ , there will still only be a finite number of  $L^*$  sentences actually uttered by members of  $P^*$ . Consider the finite set  $Q^* =_{df} \{ \langle x, y \rangle \mid x \text{ is}$

uttered at some time by a member of  $P^*$  in order to mean  $y$ }. It will have to be in virtue of  $P^*$ 's actual use of the sentences of  $Q^*$  that it can be said that there is a practice in  $P^*$  of meaning in  $L^*$ . But  $Q^*$  will also be a subset of infinitely many other languages all of which are different from each other and from  $L^*$ . Any practice in  $P^*$  of meaning in  $L^*$ , then, will also be a practice in  $P^*$  of meaning in any one of these infinitely many other languages.<sup>20</sup> If we were to suppose that the employment-clause of [T] by itself stated a sufficient condition for a population  $P$  to speak a language  $L$ , then we would have to conclude that  $P^*$  used each of these infinitely many languages. But this would contradict our supposition that there is no language that is not a subset of  $L^*$  that  $P^*$  uses. So we cannot suppose both that there can be a single language  $L$  such that a population  $P$  uses no languages but  $L$  and its subsets, and that there being a practice in a population  $P$  of meaning in a language  $L$  is a sufficient condition for it being the case that  $P$  uses  $L$ . It seems certain that we should be able to suppose the former conjunct, so the latter conjunct must be taken as false. Thus the employment-clause of [T] does not state by itself a sufficient condition for a population's speaking a language.

The *whole-language-clause* is intended by Schiffer to pin down what the *employment-clause* cannot. I will now discuss Schiffer's motivation for the whole-language-clause of [T].

Consider two members, June and Bingo, of some population  $P^*$ . Suppose June utters some sentence  $\sigma$  in Bingo's presence which means among the members of  $P^*$  the proposition that Metallica rules. It seems more-or-less harmless to suppose that Bingo understands June just in case Bingo comes to have the belief that June said that Metallica

rules. But if Bingo thinks in Mentalese, then her belief that June said that Metallica rules will be realized by the tokening in her belief-box of an M-sentence that means in M that June said that Metallica rules. The M-sentence that means that June said that Metallica rules will have to contain a part that means that Metallica rules. That part, since it means the same thing as the sentence  $\sigma$  that June uttered, namely, that Metallica rules, can be said to be an M-translation of  $\sigma$ . Thus, somehow a *translation* of the public-language sentence  $\sigma$  into M can be said to have been achieved in the process of Bingo's coming to understand June's utterance of  $\sigma$ .

The picture of language understanding presented in this tale would appear to be generally correct given the LOT hypothesis, so that, more-or-less, for a hearer to understand what a speaker said in uttering a sentence that means a proposition  $\pi$  among the members of the population to which they belong is for the hearer to have tokened in his or her belief-box an M-sentence that means that the speaker said, or meant,  $\pi$ . Further, let us suppose that for any proposition  $\pi$ , any M-sentence that means in M that some so-and-so said  $\pi$  must contain an M-sentence that means  $\pi$ . Then it would seem that a necessary condition, assuming the truth of the LOT hypothesis, for a hearer to understand what a speaker said in uttering a sentence that means a proposition  $\pi$  among the members of the population to which they belong is that a sentence that means  $\pi$  in M be tokened in the hearer.<sup>21</sup> So language understanding would seem to require translation into M if the LOT hypothesis is true.

But even if translation into Mentalese is necessary for understanding the sentences that actually get uttered in a population, it surely cannot be necessary for every sentence

of a language to be *actually* translated into Mentalese in order for the language to be used by a population. For infinite languages this would be impossible and if actual translation were required, no population could speak an infinite language.

Schiffer's insight at this point is that if there is a device that, in some relevant sense, *determines a mapping* from public-language sentences onto their M-translations, even if it is never used for actually translating most public-language sentences into M, then such a device almost surely must be what determines that a population uses the particular language it uses. Thus the *whole-language-clause* of his theory [T] of the actual-language relation. An L-determining translator for a language L, by definition, determines a mapping of L sentences onto their M-translations. Such a mapping, it would seem, is sufficient to pick out the language L because each M-sentence is uniquely tied to the proposition that is its meaning in M via the belief-making property it has. Thus, adding to the *employment-clause* the *whole-language clause* might seem, *prima facie* at least, to provide a necessary and sufficient condition for a population to use a language.

Now, supposing that Schiffer is right about [T] - I will argue just below that, in fact, he is quite wrong -, then the meaning of public-language expressions is determined by the meaning of their M-translations. And one might object to [T] if there was nothing to say about how M-expressions have their meanings. To say that an M-sentence means a proposition  $\pi$ , it will be recalled, is to say that the M-sentence has a physical property - what I have been calling its *belief-making property* - such that having that property and being tokened in the belief-box is metaphysically sufficient for believing  $\pi$ . So to explain how M-expressions have their meaning is to explain how the belief-making properties of



M-sentences realize the beliefs that they do. For Schiffer, this question is answered by the provision of a correct compositional-supervenience theory for M.<sup>22</sup> For the meaning of every M-expression is uniquely determined by a correct compositional supervenience theory for M.

I am willing to accept Schiffer's theory meaning for M, though the matter has been debated.<sup>23</sup> So I don't think there are serious objections to Schiffer's account of meaning due to a lacuna in that account with respect to issues of Mentalese expression-meaning. But, in fact, I believe that Schiffer's account of the actual-language relation is badly off the mark for other reasons.

I will now show why Schiffer's whole-language-clause, as it is stated in [T] fails to provide, along with the employment-clause, a sufficient condition for a population's using a language. Afterwards I will argue that there is no obvious way to repair [T] and that for interesting reasons this reflects poorly on the whole project of giving a translational theory of meaning.

### **6.3 A Counterexample to Schiffer's Actual-Language Relation**

The counterexample I will now present is pretty straightforward. The basic idea is that an L-determining translator for a language L can be a proper part of an L'-determining translator for a language L' which is not identical to L. Processing of L'-sentences by a population P' will proceed via the L-determining translator since it proceeds via an L'-determining translator and the L-determining translator is a proper part of this L'-determining translator. That's the rough idea. I will now provide a little detail

and show precisely how this sort of scheme can count as a good counterexample against Schiffer's theory [T].

Consider a population  $P_1$  that uses the language  $L_1$ . Now suppose the following four things:

- [C]        (i) the set  $S_1 =_{df} \{ \langle x, y \rangle \mid \text{both } x \text{ is actually tokened at some time by a member of } P_1 \text{ in order to mean } y \text{ and } x \text{ means in } L_1 y \}^{24}$ ;
- (ii) there is a language  $L_2$ , different from  $L_1$ , such that there is a non-empty set  $S_2$  such that (a)  $S_2 \subseteq L_1 \cap L_2$ , (b)  $S_2 \subset S_1$ , (c) there is a practice in  $P_1$  of meaning in  $S_2$ , and (d) there are no  $L_2$  utterances by members of  $P_1$  that are not tokenings of  $S_2$  sentences;
- (iii) in the head of each member of  $P_1$  is an  $L_1$ -determining translator which is used in processing  $L_1$  utterances;
- (iv) the  $L_1$ -determining translators inside the heads of members of  $P_1$  each contain an  $L_2$ -determining translator as well as a mechanism that determines the mapping  $f_2$  which is such that, for any  $\sigma$ , if  $\tau$  is the value of the mapping determined by the  $L_2$ -determining translator for argument  $\sigma$ , then  $f_2(\tau)$  is the  $M$  sentence that means in  $M$  what  $\sigma$  means in  $L_1$ .

The situation described by these suppositions represents a counter-example to Schiffer's theory since, though it follows from these suppositions both that there is a practice in  $P_1$  of meaning in  $L_2$  and that processing of  $L_2$  sentences proceeds via an  $L_2$ -determining translator,  $P_1$  does not use  $L_2$ . Let me show why all this follows from [C].

Note first that the set  $S_2$  is a subset of  $L_2$ : since every member of  $S_2$  is a member of the intersection of  $L_1$  and  $L_2$ , and since every member of the intersection of  $L_1$  and  $L_2$  is a member of  $L_2$ , then every member of  $S_2$  must be a member of  $L_2$ . Also, it follows from supposition (ii), along with the definition [P], that often when a member of  $P_1$  means some proposition  $\pi$ , she or he utters a sentence that means in  $S_2$   $\pi$  to do so. Since every member of  $S_2$  is a member of  $L_2$ , then often when a member of  $P_1$  means some proposition  $\pi$ , she or he utters a sentence that means in  $L_2$   $\pi$  to do so. Thus, it will

clearly follow that there is a practice in  $P_1$  of meaning in  $L_2$ .

Supposition (iii) has it that processing of  $L_1$  sentences proceeds in  $P_1$  by way of an  $L_1$ -determining translator. But since processing by way of an  $L_1$ -determining translator proceeds, according to supposition (iv), by way of an  $L_2$ -determining translator, processing of  $L_1$  sentences proceeds in  $P_1$  by way of an  $L_2$ -determining translator.  $S_2$  is a subset of  $L_1$ , so processing of  $S_2$  sentences proceeds in  $P_1$  by way of an  $L_2$ -determining translator. And finally, since the only  $L_2$  sentences ever encountered by  $P_1$  members are  $S_2$  sentences, it follows that processing of  $L_2$  utterances by members of  $P_1$  proceeds by way of an  $L_2$ -determining translator.

So it follows from the story supposed above that there is a practice in  $P_1$  of meaning in  $L_2$  and that processing of  $L_2$  utterances by  $P_1$  members proceeds via an  $L_2$ -determining translator. According to Schiffer's theory [T], it should follow that  $P_1$  uses  $L_2$ . But  $P_1$  does not use  $L_2$ . So Schiffer's theory is false.

This counter-example plays on the simple idea that [T] does not say enough about the role of the L-determining translators in utterance processing. Roughly, as long as an L-determining translator is implicated in some way in utterance processing - it doesn't actually matter how it is implicated - the whole-language clause of [T] is satisfied. Even though, for a particular L, an L-determining translator in some sense does help "nail down", as Schiffer says, the entire language L, still the nailing-down done by an L-determining translator is beside the point when the translator is placed in certain processes like the one described by my supposition (iv) above. There is, presumably, a right sort of way that a language can be nailed down, and Schiffer has not succeeded in telling us

what that way is.

It might at first appear that there is an easy remedy of Schiffer's theory in the face of my counter-example. Roughly put, just have the whole-language clause disallow an L-determining translator that is a proper part of some other translator. But such a requirement, supposing it could be stated with greater clarity, would be too strong. Consider a population  $P_2$  that uses language  $L_1$ . Let  $L_3$  be a language radically different from  $L_1$ . The members of  $P_2$ , we can suppose, all have  $L_1$ -determining translators in their heads and these are proper parts of  $L_3$ -determining translators. But in processing of  $L_1$  utterances, let's suppose, only the  $L_1$ -determining translator is used somehow. This situation, I believe, shows quite clearly that it is not necessary that a translator used to determine a language *never* be a proper part of some other translator which determines a different language.

The trick is to say something more than merely that an L-determining translator is used in processing of L-utterances: the necessary and sufficient conditions for its *meaning-making use*, as we might perhaps call it, need to be specified. But there are other problems still for Schiffer's [T] to which I now turn.

#### 6.4 Hell-Determining Translators

I will now convert the counter-example presented in 6.2.1 into a sort of nightmare for Schiffer's theory. Then I will show that a rather serious objection to Schiffer's theory [T], independent of the sort of counterexample given in section 6.3, can be raised.

Schiffer tells us in [L] that, for a language L, an L-determining translator is "a

mechanism that determines a mapping of each sentence of *L* onto a Mentalese sentence that means in Mentalese what the *L* sentence means in *L*." But what counts as a mechanism that *determines a specific mapping*?

Any mechanism with inputs and outputs describable in physical or topic-neutral terms will determine what systems-engineers call a *transfer function*. A transfer function for a mechanism describes in physical or topic-neutral terms what output state will occur for the mechanism given some input.<sup>25</sup> In mechanisms used for doing the sorts of things that are commonly described as "computing functions", e.g., calculators or computers, it is not the inputs and outputs described in physical or topic-neutral terms that really determines what function we will say the mechanism computes, but both interpretations assigned to the input and output states and all sorts of complex idealizations that we make of the input and output states of such machines.<sup>26</sup> But such a heuristic/intentional notion of determination of a function is not something that Schiffer can rely on in his theory. If a function is going to be determined by a machine, by a mechanism, in Schiffer's theory, then there will have to be some good sense to be made of the notion of determination here in *non-intentional* terms.

Ignoring the interpretations that people place on input/output states of machines as well as other apparently *intentional* aspects of the attributions of functions to machines, it would seem the only sober view that one could entertain is that a function, or mapping, is *realized*, or *determined*, by a mechanism if and only if it is isomorphic to the transfer function of the mechanism.<sup>27</sup>

Using this notion of *determination of a mapping* to interpret Schiffer's [L], it will

follow that, for any language  $L$ , any mechanism with a transfer function isomorphic to a *translation-function*, as I will call such things,  $f_L$  from  $L$ -sentences to their  $M$ -translations will determine  $f_L$  as well as all of the infinity of translation-functions isomorphic with  $f_L$ .<sup>28</sup> In, my counterexample [C] of section 6.3 above, I supposed that an  $L_2$ -determining translator was employed in utterance processing by  $P_1$  members. Let  $\tau$  be the infinite set of translation-functions isomorphic with the translation-function from  $L_2$  into  $M$ . And let  $\tau'$  be that infinite subset of  $\tau$  such that each member of  $\tau'$  has as a subset the finite translation-function from  $S_2$  sentences to their  $M$ -translations. For each function  $f$  in  $\tau'$  there will correspond a unique language  $L$  such that  $L = \{ \langle x, y \rangle \mid x \text{ is in the domain of } f \text{ and } y \text{ is what } f(x) \text{ means in } M \}$ . Let  $\lambda$  be the set of all the languages that so correspond with members of  $\tau'$ . Then, for each of the infinitely many languages  $L$  in  $\lambda$ , both, there is a practice in  $P_1$  of meaning in  $L$ , and processing of  $L$  utterances proceeds in  $P_1$  by way of an  $L$ -determining translator. So according to [T], on the present interpretation of the notion of *determination of a function by a mechanism*,  $P_1$  must use every language in  $\lambda$ . I will call this the *hellish consequence*. It's hellish because  $P_1$ , by hypothesis, doesn't even use one of the languages of  $\lambda$  and yet, there are infinitely many languages in  $\lambda$ .

An important thing to note is that even if my counterexample [C] were somehow avoided, there would still be a *somewhat-hellish consequence* of Schiffer's theory [T] given the present interpretation of the notion of *determination of a function by a mechanism*. For consider again the  $L_1$ -using population  $P_1$ . Suppose that  $f_1$  is the translation-function for  $L_1$ . There will be infinitely many languages different from  $L_1$  but

with translation-functions isomorphic to  $f_i$ . Let  $\lambda_i$  be the set of these languages. Any  $L_i$ -determining translator will also be an  $L$ -determining translator for each language  $L$  in  $\lambda_i$  on current assumptions. And that will mean that each member of  $\lambda_i$  will be a language used by  $P_i$  according to [T] even though the only member of  $\lambda_i$  used by  $P_i$ , by hypothesis, is  $L_i$ .

The somewhat-hellish consequence, actually, constitutes a second serious issue for Schiffer's theory since it is independent of the problem raised in section 6.3 above. The somewhat-hellish consequence can be drawn from Schiffer's theory given the present reading of the notion of the *determination of a function by a mechanism*. The hellish-consequence can be blocked, that is, by somehow blocking the counterexample of section 6.3, but not the somewhat-hellish consequence. To block the somewhat-hellish consequence a reading of the notion of the *determination of a function by a mechanism* that is more fine-grained than the present *isomorphism*-reading, but that uses no intentional notions, will have to be supplied. I turn to this possibility now.

## 6.5 Determining a Function

There is a way that the hellish and somewhat-hellish consequences of section 6.4 might be avoided, but it would require giving up the thought that an  $L$ -determining translator was a *mechanism* in any ordinary sense of that notion and it will open itself to further counterexamples in part because of this.

An  $L$ -determining translator for a language  $L$  can be taken to be an  $M$ -expression - tokened in some "subdoxastic" attitude-box - that expresses exactly one function, viz.,

the mapping which takes each L-sentence onto the M-sentence that means in M what the L-sentence mean in L. An expression that denotes a function is not generally considered a sentence, so I will suppose that an L-determining translator is an M-sentence such that for some translation-function  $f$ , the sentence logically entails for each L-sentence  $\sigma$  that the M-translation of  $\sigma$  is  $f(\sigma)$ . And I will say that an L-determining translator, so construed, is an M-sentence that *expresses a translation-theory for L*.<sup>29</sup> An L-determining translator will now determine exactly one function by virtue of its having the specific belief-making property that it has.<sup>30</sup> So understood, an L-determining translator doesn't look like a *mechanism* exactly anymore. But, we can take Schiffer's talk of an L-determining translator as a mechanism as merely a sort of metaphor. Problems arise, however, because of this relaxing on the notion of *mechanism* here and I will get to these in a moment. For now I would just like it to be pretty clear that we do seem forced to the present reading by the arguments of section 6.4.

Two things should be noted immediately. First, Schiffer's theory now looks something like Chomsky's theory of the actual-language relation which I discussed in chapter 4. The only difference is that Schiffer has us standing in a cognitive relation of some sort to a finite definition of translation-function where Chomsky had us standing in a cognitive relation of some sort to a finite definition of a language, that is, to a CMT. Schiffer's theory has over Chomsky's, perhaps, that it is far more plausible that there could be a finite definition of a translation-function than of a language. But, just as it was seen that Chomsky needs to be clearer about the nature of the cognitive relation he takes language-users to stand in to CMTs, so Schiffer needs to be clearer about the attitude-box



that his L-determining translators are thought to occupy. And I will return to this point in just a bit.

A second thing that should be pointed out is that Schiffer's theory now requires language-users to have propositional attitudes about neural states somehow since now an L-determining translator is an M-sentence that talks about such things. An L-determining translator, that is, specifies M-translations of public-language sentences. So it must use expressions that refer to M-sentences. But M-sentences are supposed to be neural states of some sort. So, if an L-determining translator is in some subdoxastic attitude-box of a language user, then that language user must be having a propositional attitude about all sorts of complicated neurological things. This might seem rather implausible, even on the assumption that this knowledge is unconscious. It seems *prima facie* pretty unintuitive to believe that people generally have some such knowledge, not to mention how unintuitive it is to suggest that people more than a couple of hundred years ago had unconscious knowledge of their own neural states. Of course, a defender of the view might try to explain how this could be by saying that language users don't need to think of neural states *as* neural states, that is, they could have some other *mode-of-presentation* of these states. That's fine as far as it goes, but to make such a story plausible more would have to be said. Though I do think this is a problem for Schiffer's theory, I am willing for the remainder of my discussion, at least, to ignore it.<sup>31</sup>

It should be obvious that construing L-determining translators in the present way, that is, as M-sentences that state a translation-theory for a language, though it apparently doesn't help at all with respect to the counterexample of section 6.3, saves Schiffer's [T]

from the hellish and somewhat-hellish consequences. Isomorphism is not the issue. The translation-function determined by an L-determining translator will be unique because the belief-making property of the L-determining translator establishes only a single interpretation for it. So, we have apparently calmed the worries represented by the hellish and somewhat-hellish consequences.

But new problems will now arise. If L-determining translators are construed as M-sentences determining translation-functions, then utterance processing via an L-determining translator can no longer be seen as a necessary condition for using a language L. There are two counterexamples to the necessity of this condition which I will now discuss.

1. M-sentences by themselves don't do anything. They just sit in attitude-boxes where thought processes can make use of them. So, by itself, an L-determining translator, on the latest construal of that notion, can hardly be understood to be a mechanism in an ordinary sense. Mechanisms do things, M-sentences don't.

The following analogy may be helpful. You may have a book perfectly describing how to translate Finnish into Urdu, but by itself the book doesn't do anything. If the book, however, is given to someone who knows how to read it and make use of what is read, it can help to make accurate translations of Finnish sentences into Urdu.

An L-determining translator doesn't actually translate, but it describes, so to speak, what the results of translation will be for any input. Actual translation, if there is to be any, must be carried out by some mechanism or process that takes as input L-sentence tokens and produces as output M-sentence tokens. Let's call any such mechanism a  $\Sigma$ -

*processor*. Notice that a  $\Sigma$ -processor for a finite language  $L_F$  need not use an  $L_F$ -determining translator, i.e., it need not make use of an M-sentence that describes a function from each  $L_F$ -sentence to its M-translation: such a  $\Sigma$ -processor could be hard-wired to produce the proper output for any given input independently of any M-sentence whatsoever.<sup>32</sup> A population that used such a  $\Sigma$ -processor in an appropriate way in  $L_F$ -utterance processing could be said to use  $L_F$ . So clearly, to construe an L-determining translator in the way we have in the present section of this paper, will force [T] to state something unnecessary for language use. So unless we can find another way of construing the notion of an L-determining translator, we are faced with a dilemma: either *hell-determining* translators, that is, L-determining translators on the original *isomorphism*-construal discussed in section 6.4, or the failure of [T] to state a necessary condition for language use.

But, in fact, the dilemma can be avoided with a definition like the following, inelegant though it may be:

[L'] for any item  $x$  and language  $L$ ,  $x$  is an L-determining translator iff either (a)  $x$  is an M-sentence that expresses a translation-theory for  $L$ , or (b)  $L$  is a finite language and  $x$  a  $\Sigma$ -processor hard-wired such that, for any L-sentence  $\sigma$ , if a token of  $\sigma$  occurs at  $x$ 's input, then a token of an M-translation of  $\sigma$  occurs at  $x$ 's output.

2. There is yet another counterexample to the necessity of processing via an L-determining translator understood as an M-sentence stating a translation-function. For suppose that some language enjoyed a CMT. This is surely possible for a finite language where a CMT could be merely a list of all the meaning facts of that language. But perhaps even infinite language can turn out to enjoy CMTs as well. Whatever the

circumstance, if a language enjoys a CMT, it is perfectly conceivable that users of that language can process utterances by some sort of knowledge of a CMT. But in that case neither [L] nor [L'] are broad enough to fit this case.

One tricky matter here is that Schiffer seems originally to have intended to capture just such a case with his notion of an L-determining translator as given in [L]:

The translator can take more than one form. It can use an internally represented grammar of *L*, à la Chomsky; or it can eschew such a grammar altogether and use an internally represented translation manual à la Harvey.<sup>33</sup> Perhaps there are other forms the translator can take.<sup>34</sup>

But, it is hard to know how to be entirely charitable to Schiffer here. It just doesn't look possible to read his definition of an L-determining translator in such a way that all that comes out true. He needs to be clearer about what he takes a mechanism to be and what he takes the *determination of a function by a mechanism* to be.

In any event, I don't believe that Schiffer can state a single simple condition that can meet all *possible* cases here. Of course, the following definition of the notion of an L-determining translator is possible:

[L''] for any item *x* and language *L*, *x* is an L-determining translator iff either (a) *x* is an M-sentence that expresses a translation-theory for *L*, or (b) *L* is a finite language and *x* a  $\Sigma$ -processor hard-wired such that, for any L-sentence  $\sigma$ , if a token of  $\sigma$  occurs at *x*'s input, then a token of an M-translation of  $\sigma$  occurs at *x*'s output, or (c) *x* is an M-sentence that expresses a CMT for *L*.

It's not pretty, but it seems to avoid both the counterexamples I have raised here. What's sad is that this is just the sort of thing Schiffer seems to have wanted to capture with an elegant generalization. But also, it is not entirely clear that there is *not* some other way that has been overlooked by way of which utterance processing can go on. So

it is not entirely clear that [T] states a necessary condition for language use given the definition [L"] of the notion of an L-determining translator.

## 6.6 Repairing Schiffer's Theory [T]?

In section 6.3 I showed that Schiffer's theory [T] fails to state a sufficient condition for language use. In section 6.4 I showed a horrible consequence of this failure and I presented an independent argument against the sufficiency of [T]. The independent argument drew what I called the *somewhat-hellish consequence* from [T] and in section 6.5 I suggested a way that Schiffer's theory could avoid this consequence. But the price for avoiding the somewhat-hellish consequence was that [T] no longer stated a necessary condition for language use. The central issue here was Schiffer's definition [L]. I was able to patch up [L] as [L"] and avoid the two specific counterexamples to the necessity of [T] that I suggested, but I can think of no argument that demonstrates the necessity of [T] given [L"]. Still, I can't think of any obvious counterexamples to the necessity of [T] given [L"], so I am willing now to suppose tentatively at least that, given [L"], Schiffer's [T] does state a necessary condition for language use.

But what about the counterexample from section 6.3? For, whether a specific L-determining translator is a hard-wired L-determining  $\Sigma$ -processor, an M-sentence expressing a translation-theory for L, or an M-sentence expressing a CMT for L, it still must be embedded in an utterance-processing system, and [T] seems not to care enough about the nature of this embedding. So the counterexample of section 6.3 seems to retain its force against [T], in spite of the change from [L] to [L"] and the supposition that this

change avoids the hellish and somewhat-hellish consequences as well as the statement of anything unnecessary by [T].

Can Schiffer's theory be saved from the counterexample of section 6.3?

I don't think so. I'll make some general comments about the sort of thing that I take Schiffer to be trying to do and then I will say why I don't think that he will be able to remedy [T] to avoid the sort of counterexample that [C] represents.

As I see things, Schiffer's insight really is that if we think in M, then the question of what nails down the language L that is used by a population P is equivalent to the question of what relation  $R_p$  is such that P uses L just in case both there is a practice of in P of meaning in L and  $R_p(P, f_L)$ , where  $f_L$  is the translation-function which takes L-sentences into their M-translations.<sup>35</sup> We can call  $R_p$  the *actual-translator relation*.<sup>36</sup> To give a non-trivial specification of  $R_p$  is, clearly, to give the actual-language relation since every translation-function determines a unique language given the semantics of M. And we can say that the goal of a *translational theory of meaning* is to specify the actual-translator relation.

Schiffer's talk about L-determining translators being used in utterance processing is his attempt to specify the actual-translator relation. His suggestion, more specifically, is that there is some sort of thing - he calls it an L-determining translator - that is responsible for making sure that the correct translation-function gets determined, and then there is some sort of relation to this thing that is responsible for determining whether a person uses the language picked out by the translation-function determined by the L-determining translator. But Schiffer isn't terribly clear about either what an L-determining

translator is or what relation we are supposed to stand in to such a thing. Even if [L"] does better in saying what an L-determining translator is than Schiffer's [L] did, still, we don't know at all what relation we are supposed to stand in to such a thing in order to use a language.

Suppose that [L"] tells us what an L-determining translator is. A relation R must be specified such that for any population P, language L, and L-determining translator  $D_L$ , P uses L just in case both there is a practice in P of meaning in L and  $R(P, D_L)$ . Since there are three separate sorts of things that can count as L-determining translators, this relation R will have to state the necessary and sufficient conditions for the use of each so that a language gets spoken. Thus, the form of R will have to be something like the following:

$R(P, D_L)$  just in case either (1)  $D_L$  is an L-determining  $\Sigma$ -processor and  $C_1$ , or (2)  $D_L$  is an M-sentence stating a CMT for L and  $C_2$ , or (3)  $D_L$  is an M-sentence stating a translation-function for L and  $C_3$ .

To state the condition  $C_2$  would, in essence, be to complete the job that Chomsky left incomplete in his attempt to provide a theory of the actual-language relation.  $C_2$  will have to state the necessary and sufficient conditions for the use of a CMT such that this use constitutes language use in the appropriate sense. This surely is not a small task.

The statement of the condition  $C_3$ , will be equally difficult and for reasons that it is somewhat easier to see than in the case  $C_2$ . For  $C_3$  is the condition necessary and sufficient for the use of a translation manual such that this use constitutes language use in the appropriate sense. To believe that such a set of necessary and sufficient conditions can be stated is more or less like believing that we could provide a non-trivial set of

necessary and sufficient conditions for the perfect-translating use of a book that specified the translation-functions from Finnish into Urdu and vice-versa. I suppose that if an Urdu speaker had such a book and that if that book were written in Urdu, there is some way that it can be used for her or him to get along in Finnish. But, if Finnish and Urdu are infinitely large languages, then I can't imagine what a set of necessary and sufficient conditions for the perfect use of this book could be such that the Urdu speaker having it would be a Finnish speaker. I am extremely sceptical that there is such a set of necessary and sufficient conditions.

It should be clear that the problems with specifying what might be called *the perfect- translating use of a translation manual* will be analogous to the problems of specifying what might be called *the perfect interpreting use of a CMT*. I believe that to specify either of these notions adequately will be equivalent in important respects to solving the problem of specifying the necessary and sufficient conditions for someone to be a Finnish speaker given mastery of Urdu and the above described translation manual. And I believe that this latter problem is impossible to solve if Finnish and Urdu are both infinite languages. Thus, I am extremely sceptical that a theory too much like Schiffer's theory [T] can be ever be true.

### **6.7 A Further Problem for Schiffer's Theory**

Besides the above, Schiffer's theory seems unable to avoid the counterexample Loar raised to his own theory in Loar (1981) and that I discussed in chapter 5 above. Consider the case where, let's suppose, were any English speaker to utter 'Apollo set the



evening sky ablaze', they would be understood as meaning that the sunset is intensely beautiful this evening. Presumably, that is, English speakers would have a device that translated 'Apollo set the evening sky ablaze' with the M-sentence 'the sunset is intensely beautiful this evening' (pretend that is an M-sentence). Nothing in Schiffer's characterization of an L-determining translator would exclude considering the process, whatever it is, that produced this translation from being an *English'*-determining translator where *English'* is just like English except that it pairs 'Apollo set the evening sky ablaze' with the proposition that the sunset is intensely beautiful this evening. Thus, English speakers actually speak *English'*, on the present assumptions, according to Schiffer's theory. Thus, this is another objection to the sufficiency of Schiffer's conditions for a population to use a language. This problem will be important to the discussion in the next chapter.

## 6.8 Summary

Schiffer's theory has many problems. I have suggested some rather *ad hoc* solutions for some of these problems with the proposal of [L"] to replace Schiffer's [L], but the problem represented by the counterexample in [C] of section 6.3, I don't believe can be solved and I have given at least a rough indication of why I think this is so. Schiffer's theory, independently of all of that, also fails to accommodate the sort of case that Loar found to be a counterexample to his own theory in Loar (1981) and that I discussed in chapter 5.

## Notes

1. Quine (1960), p. 78.

2. Davidson, in the following passage, can be read as claiming that Quine doesn't aim at a theory of meaning at all: "The idea of a translation manual with appropriate empirical constraints as a device for studying problems in the philosophy of language is, of course, Quine's. This idea inspired much of my thinking on the present subject, and my proposal is in important respects very close to Quine's. Since Quine did not intend to answer the questions I have set, the claim that the method of translation is not adequate as a solution to the problem of radical interpretation is not a criticism of any doctrine of Quine's" (Davidson (1973), p. 129). Of course, providing a theory of what Davidson calls *radical interpretation* is not exactly the same as providing a theory of meaning in the sense of this dissertation. So Davidson's denial that Quine is trying to solve the problem of radical interpretation need not be seen as a denial that he is trying to suggest a theory of meaning.

3. Katz and Fodor (1963). See also Katz and Postal (1964).

4. Most philosophers today seem to use the expression "semantic theory" to mean a theory that somehow says what expressions mean or denote and I will stick to this usage. But this is a verbal point, not one of substance.

5. Bruce Vermazen in his review of Katz and Postal (1964) and Katz (1966) lists twenty-five such semantic properties and relations (Vermazen (1967), p. 364, note 3).

6. Vermazen, discussing a specific Markerese translation of an English sentence says the following: "...it hardly seems to 'provide a characterization of the meaning' in any more interesting sense; considered as a string of English words in parentheses, it is more or less unintelligible, and since Katz assures us that it is not a string of English words, but a collocation of representatives of concepts, it is only more clearly unintelligible. And the meaning of a sentence is a fairly important concept to accommodate in a semantic theory" (Vermazen (1967), p. 355).

A version of this criticism is to be found in Lewis (1969), pp. 171: "...I have not endorsed Katz's account of the nature of...interpretations.... Katz takes them to be expressions built out of symbols called 'semantic markers'.... I find this account unsatisfactory, since it leads to a semantic theory that leaves out such central notions as truth and reference." See also Lewis (1970) where he essentially repeats this criticism.

Searle also gives a version of the criticism in Searle (1972), pp. 26-27.

Davidson, without mentioning Katz, Fodor, or Postal or explicitly making reference to Katz/Fodor-style theories seems nonetheless to have something like that sort of theory in mind when he criticizes translation-style theories in his "Radical Interpretation": "...in the general case, we can know which sentences of the subject language translate which sentences of the object language without knowing what any of

the sentences of either language mean (in any sense, anyway, that would let someone who understood the theory interpret sentences of the object language)" (Davidson (1973), p. 129).

Evans and McDowell in their introduction to Evans and McDowell (1976) also give a version of the criticism, explicitly calling attention to Katz/Fodor-style theories and citing Lewis (1970) as a source for their views.

Martin Davies repeats Evans and McDowell's criticism, citing them and Lewis (1970); see Davies (1981), p. 4.

For a recent reply by Katz to this style of criticism of Katz/Fodor-style theories, see Katz (1990), pp. 211-214. Essentially, as far as I can see, Katz's response to the sort of criticism found in the above places is that they all overlook that Markerese comes with an "intended interpretation" (Katz (1990), p. 212). But it seems like the criticism in question here is that the interpretations are not *explicit* and that no provision is made for how to make the interpretations *explicit*. It is hard to understand why Katz should have thought that a property like *analyticity* should have been something for which to provide an *explicit* definition in terms of semantic markers while a property like *meaning that snow is white* should be left to a grasping of an *implicit* convention. And this is especially so if, as was claimed in Katz and Fodor (1963), a Katz/Fodor-style theory is supposed to be a characterization of a natural-language user's linguistic competence: for surely a natural-language user doesn't understand natural language by grasping "intended interpretations" of Markerese formulas! The complaint by the above philosophers is exactly that as Katz/Fodor-style theories are sketched, they won't explicitly entail that a sentence like "snow is white" has the property of *meaning that snow is white* or at least the property of *being true just in case snow is white*. To appeal to implicit intended interpretations doesn't look like an adequate response to this complaint. The provision of rules that would entail that a sentence means what it does would.

Of course, another move in defense of Katz/Fodor-style theories can be the identification of Markerese formulas with Mentalese formulas. This is essentially the move suggested by Fodor that begins the translational tradition in question in the main text. This defense doesn't provide for an explicit statement of the semantics of Markerese formulae either, but it doesn't have to directly. Standing in a cognitive relation to a translation theory could, for all we know, explain the competence of natural-language users. But standing in a cognitive relation to a Katz/Fodor-style theory without the identification of Markerese formulae with Mentalese formulae leaves big holes in the theory of natural-language competence: there is still the question how do native speakers know the meanings of sentences that such a theory doesn't answer.

There are other criticisms of Katz/Fodor-style theories besides the above. For example, Katz/Fodor-style theories have been criticized by a number of writers on the grounds that they require an untenable conceptual atomism, as it might be called. See Putnam (1970) for this sort of criticism. I am inclined to think that this sort of criticism of Katz/Fodor-style theories is telling. I am not really discussing the conceptual-atomistic aspects of Katz/Fodor-style theories in the main text though.

7. Actually quite a bit more could probably said concerning the prehistory of the translational tradition as there are many theorists besides those who I have mentioned here who have thought that *translation* is the central notion for a proper understanding of speech and language. Welby is among these; see Welby (1983) and Welby (1985). See also Steiner (1975) where various views of translation are compared with various other views of speech and language. See also Parkinson (1977) and Stewart (1977), both in Vesey (1977).

8. For a good basic discussion of how all that works, see Tanenbaum (1976). 'Translators' in the present sense come in two varieties called 'interpreters' and 'compilers'. Sometimes people talk only in terms of *compilers* when they really mean to speak more generally of any sort of higher-level language 'translator'. Fodor does this in Fodor (1975) which I am about to discuss.

9. See Miller and Johnson-Laird (1976), pp. 158 ff. Miller and Johnson cite sources for their views as well, so though I start this vague history with them, this is probably not quite correct.

10. Miller and Johnson-Laird (1976), p. 159.

11. See Fodor (1975), chapter 3. Fodor cites Miller and Laird-Johnson as sources for his view, and he explicitly rejects their procedural semantics. See Fodor (1975), p. 116, note 12.

12. Fodor (1975), p. 116.

13. None of this, by the way, is to say that Fodor in Fodor (1975) accepted the conceptual atomism of Katz/Fodor-style theories. That is an independent issue that I am not treating here.

14. Schiffer does seem to be on the way to something like a translational-theory of meaning in *Remnants of Meaning* (Schiffer (1987a)), chapter 7 with his discussion of Harvey who processes sentences by translation to and from Mentalese. But Schiffer doesn't seem to have come up with the idea of formulating a theory of meaning in terms of such translation until later.

15. Schiffer (1993), pp. 244-245.

16. Schiffer (1993), p. 241.

17. Schiffer (1993), p. 246.

18. Ibid.

19. Of course, both the employment-clause and the whole-language-clause are open sentences by themselves, so if we are really being picky, we couldn't say they *state* anything. When I speak of these clauses as *expressing* or *stating* things I should be taken as meaning *within [T]*.

20. [P] can be restated as: for **any language L** and population P, there is a practice in P of meaning L iff for some subset S of the set  $\{ \langle x, y \rangle \mid x \text{ is actually tokened at some time by a member of P in order to mean } y \}$ , the sentences of S are often used by members of P to mean what they mean in S and  $S \subseteq L$ . I say "for some ... subset S ..." because the members of a population may speak more than one language. The idea here is that for any set S that is a subset of a language L, and for any population P, if often a member of P in order to mean a proposition  $\pi$  utters an S-sentence that means in S  $\pi$ , then often that same member of P in order to mean  $\pi$  utters an L-sentence that means in L  $\pi$ .

21. Note that I do not say "tokened in B's belief-box" as if B came to believe what A said. Of course, it would be true to say that but perhaps confusing - the M-expression that means what A said is, indeed, tokened in B's belief-box on present presuppositions but it is embedded in another sentence. A doesn't have to believe what B said to understand it.

22. Schiffer (1993), p. 251. See also Schiffer (1994b), p. 297 and Schiffer (1994c).

23. See Ray (1995) and Schiffer's response, Schiffer (1995). See also Voltolini (1995).

24.  $S_1$  may, of course, be either identical to or a proper subset of the set  $\{ \langle x, y \rangle \mid x \text{ is actually tokened at some time by a member of } P_1 \text{ in order to mean } y \}$ .

25. The term "transfer function" is usually used by engineers and in engineering literature to denote an input/output mapping for some limited range of inputs and outputs, and not a for every possible input and output of a system. But there is no harm in speaking in the way I do in the main text.

26. Often, for mechanisms that people build to compute functions, we design the physical input and output states so that one specific interpretation of input and output states is quickly available to us: a certain sequence of key strokes on a calculator represents the number 39.432, and a specific arrangement of the activated LCD's of an LCD-display represents the number 946.83. But these don't really pin down one function as the one *determined* by the mechanism in anything but a sort of heuristic sense. For some discussion of related issues, but in a wholly different context, see Katz (1985b), pp. 198-199.

27. I believe that this sort of view lies behind Wittgenstein's *picture theory* in the *Tractatus* and behind Hertz's views - which Wittgenstein acknowledged in the *Tractatus* as sources of his own - of how and why differential equations work so nicely in

modeling physical systems.

28. Two functions  $g$  and  $g'$  will be isomorphic just in case there is a mapping  $f_i$  such that (a) both for any item  $x$  in the domain of  $g$ ,  $f_i(x)$  is in the domain of  $g'$ , and for any item  $x$  in the domain of  $g'$ , there is an item  $y$  in the domain of  $g$  such that  $f_i(y)=x$ , (b) for any two items  $x$  and  $x'$  in the domain of  $g$ , if  $x \neq x'$ , then  $f_i(x) \neq f_i(x')$ , and (c) for any two items  $x$  and  $x'$  in the domain of  $g$ , if  $g(x)=g(x')$ , then  $g'(f_i(x))=g'(f_i(x'))$ . I suppose that for a given infinite translation function  $f$ , there are infinitely many translation functions isomorphic with  $f$  since there are infinitely many sentences to construct such functions from.

29. Be careful not to confuse the notions of a *translational theory of meaning* and a *translation-theory for a language L*. The former is a certain kind of theory of the actual-language relation. The latter is merely a statement of a finite definition of a function from each sentence of a language  $L$  to its  $M$ -translation. On the present suggestions, Schiffer is attempting to provide a translational theory of meaning by supposing that to speak a language  $L$  is to stand in a certain cognitive relation to a translation-theory for  $L$ .

30. Schiffer has told me in conversation that he would like  $L$ -determining translators to be understood as  $M$ -sentences in somewhat the way I have just outlined things here. But there is quite a bit of tension here that will become quite vivid when I discuss my second counterexample to Schiffer's theory on the present reading below.

Note also that when I say that an  $L$ -determining translator construed in the present way determines exactly one translation-function, I of course mean not that the function has no members which are themselves functions, but rather that there is no function that is a proper superset of the function determined by an  $L$ -determining translator. I am speaking somewhat loosely then, but I expect that this should be clear enough.

31. If one took the present point more seriously, there is the following point too. Whereas it is somehow sort of plausible that we have some sorts of unconscious knowledge of the things the compositional-semantics theorist of meaning wants to say that we have knowledge of - things like word-meanings and denotations and conditions on syntactic structures - it really doesn't seem too plausible on the face of things that we know any of this neurological stuff that on Schiffer's theory we would have to know, at least on the present reading. Compare the questions, "do I really unconsciously know anything about my own language of thought?" and "do I really unconsciously know that a sentence of the form 'some  $F$  is  $G$ ' is true just in case there is something in the extension of ' $F$ ' that is also in the extension of ' $G$ '?" I wouldn't be all that surprised to learn that the answer to the latter question is yes. But it would take an awful lot, I think, to get me to understand how the answer to the former question could be yes. So, on this score, it seems that compositional-semantic theories of meaning are somewhat more plausible than the present sort of translational theory of meaning. But I won't pursue this line any further.

32. I always took Schiffer's character Harvey from chapter 7 of Schiffer (1987a) to work by some such hard-wired process, so I was surprised to learn from Schiffer that he would like his L-determining translators to be understood as M-sentences in the way I discussed in the main text above.

33. Harvey is a character Schiffer first introduced in chapter 7 of Schiffer (1987a) who processes utterances via possession of a non-semantic translation scheme of some sort.

34. Schiffer (1993), p. 245.

35. Note that  $f_L$  is the set-theoretic function, an abstract object, and not an L-determining translator of some sort.

36. Schiffer, in Schiffer (1993), refers to an L-determining translator as "the translator", but don't confuse my use of the term "translator" in my expression "actual-translator relation" with Schiffer's. Again, my "translators" are abstract objects, not brain states or mechanisms of some kind. They are the translation-functions which map L-sentences, for some language L, onto their M-translations.

## Chapter 7 Towards a Theory of Meaning

### 7.1 Introduction

Having considered and found fault with a number of attempts to provide an adequate theory of the actual-language relation, I will now make a number of proposals concerning some of the background assumptions of the pursuit of such a theory. One can suppose that previous theories have failed because there just can't be an adequate theory of the actual-language relation, that is, one can suppose that somehow something like what Wittgenstein thought was right and that theories of meaning are impossible. But I really don't know of any good reason why this has to be so. So I prefer to believe that there are solvable problems with some of the assumptions that have been in the background of past attempts at theorizing about meaning. What I will do in this chapter is say what I think some of these problems have been and how I think that these problems might best be solved.

In section 7.2 I will discuss what I think a Gricean needs to do to escape the sort of counterexample that Loar found for his own theory of meaning and that I discussed in section 5.5 above as well as in section 6.7 where I showed that this counterexample is also a serious problem for Schiffer's theory. In essence I will propose a notion of *saying* to replace the more common notions of *speaker-meaning* that Gricean's have traditionally favored. My notion of *saying* will require that a certain common conception be dropped of some of the requirements of Grice's theory of implicature. But I will argue that, on the change of conception required by adopting my proposal concerning *saying*, nothing in Grice's theory of implicature is lost and that if there is a gain for the theory of literal



expression-meaning, then we do well to adopt my proposal along with the requisite changes in the conception of the theory of implicature. In section 7.3 I will talk about a certain presupposition concerning the magnitude of natural languages that I think is responsible in part for forcing theorists to accept the disjunction discussed in chapter 5 of founding infinitudes of meaning-facts either in finite sets of facts about actual usage or in CMTs. I will argue that there is no reason to accept this presupposition and that it should therefore be dropped. In section 7.4 I will present what will appear a further serious counterexample to Schiffer's theory discussed in chapter 6. But I will argue that this counterexample is only a counterexample given a false, if widely held, presupposition about the aims of the theory of meaning. I will then argue that this presupposition is yet another that should be dropped. Finally, in section 7.5, I will say what I think a theory of meaning should look like given the revisions I have suggested to the presuppositions that have been a standard part of theorizing over the past 35 or so years.

So the main goals of this chapter are to loosen up the belief in a number of widely held presuppositions that have led to problems for the theory of meaning in the past and also to suggest what an adequate theory of meaning might look like.

## 7.2 Saying

In section 5.5 I discussed a counterexample Loar presented to a version of his own theory. Loar noticed that on his proposal for a theory of the actual-language relation, given some plausible assumptions about English speakers, the English sentence "Apollo set the evening sky ablaze" means among English speakers something like that the sunset

is intensely beautiful this evening rather than that Apollo set the evening sky ablaze. Loar diagnoses the problem with his theory to be its lack of an adequate consideration of the compositional structure of sentences. But I believe the real problem with Loar's theory is that the notion of speaker-meaning he employs is too strong a notion to be useful in a theory of expression-meaning. I will now propose a slightly weaker notion to replace the Gricean notion of speaker-meaning in attempts to provide a theory of expression-meaning. My notion will be one of *saying*, and though there will be some relation between my notion of *saying* and the notion expressed by the English word "saying", the point now is to engineer a notion that is suitable to the theory of expression-meaning, not to analyze an ordinary one.

The notions of *speaker-meaning* that Grice and the theorists in the Gricean tradition have employed all have it, more-or-less, that to *speaker-mean* is to intend to get a listener to believe the proposition that is *speaker-meant*. For convenience, I will talk from now on as if there is just one proposed analysis of *speaker-meaning* floating around and that this is one of its essential features. I want to oppose to this notion a notion of *saying*, where what is said need not be what the speaker intends to get an audience to believe, even if *saying* always is an attempt to get an audience to believe something. To *say* something, that is, is to try to get an audience to believe something by trying to get them *to consider* what is said. Sometimes a person *speaker-means* what is *said*, but sometimes not. Thus, the notion of *saying* I have in mind can be seen as comporting nicely with the ordinary way of speaking whereby a person can say what they mean and mean what they say, but need not do so. So, I can say to you that Apollo set the evening

sky ablaze and not mean it, that is, not mean that you come to believe it, or even believe that I believe it. I say it to have you consider it and recognize, partly on the basis of your considering it that I want you to believe something else, namely, that there is an intensely beautiful sunset this evening.

My proposal, then, is that the following notion of *saying* is more suitable to a theory of expression-meaning than the more common notion of *speaker-meaning*.

[S] A speaker S *says* a proposition  $\pi$  by uttering a sentence  $\sigma$  just in case there is an audience A such that

(I) for some proposition  $\pi'$ , S intends that

- (1) A comes to believe  $\pi'$ ,
- (2) A comes to consider whether to believe  $\pi$ ,
- (3) A recognizes S's intentions (1) and (2), and
- (4) A comes to believe  $\pi'$  by reasoning at least partly from A's recognition of S's intentions (1) and (2),

and (II) there is no proposition  $\pi'$ , different from  $\pi$ , such that S intends that

- (5) A comes to consider whether to believe  $\pi'$ ,
- (6) A recognizes S's intention (5), and
- (7) A comes to believe that S intends A to consider whether to believe  $\pi$  by reasoning at least partly from A's recognition of S's intention (5).<sup>1</sup>

A central insight of Grice's is that a speaker's intention to get an audience to believe a proposition can itself count as evidence for the audience that the proposition is true. I preserve this insight in my analysis of *saying*. To *say* is to intend to get an audience to believe something and to get an audience to believe that thing on the basis of their recognition of the speaker's intention to get them to believe it. But, the thing that a speaker wants an audience to believe does not have to be the thing the speaker says.

The speaker will intend the audience to consider a certain proposition and, on the basis of this consideration and recognition of the speaker's intention to bring it about, to infer what proposition the speaker actually wants the audience to believe. Thus, I might utter "Apollo set the evening sky ablaze" intending that you understand that I am trying to get you to believe something and trying to get you to believe that particular something on the basis, in part, of your considering the truth of the proposition that Apollo set the evening sky ablaze. I don't want you to believe that Apollo set the evening sky ablaze, or to believe that I believe it. I just want you to consider it and reason from your recognition of my wanting you to consider it to a recognition of what proposition I actually do want you to believe, namely, that the sunset is intensely beautiful this evening. Then, once you have grasped that I really want you to believe that the sunset is intensely beautiful this evening, I want you to reason, in the usual Gricean way, from your recognition of my intention to get you to believe that the sunset is intensely beautiful this evening to the belief that the sunset is intensely beautiful this evening. I say that Apollo set the evening sky ablaze as a part of my *speaker-meaning* that the sunset is intensely beautiful this evening.

Thus, I intend to get you to recognize my intention to get you to believe that the sunset is intensely beautiful this evening and thereby believe that the sunset is intensely beautiful this evening by getting you to recognize first that I want you to consider whether to believe that Apollo set the evening sky ablaze. I know, let's say, that you will immediately upon consideration reject it's being the case that Apollo set the evening sky ablaze because I know that you know that Apollo is a mythical god. I also know that you

will immediately reject the thought that I believe that Apollo set the evening sky ablaze because I know that you know that I know that there is no such thing as Apollo. But I expect that you will try to make sense of my having tried to get you to consider this belief about Apollo in the context of your recognizing that I *do* want you to believe something. And I expect you to figure out, given all of this along with, let's suppose, some general conversational principles and other background assumptions, that what I am trying to get you to believe is that the sunset is intensely beautiful this evening.

In the case where I *mean* what I say, the mechanism is in general the same, but I intend that you figure out that the proposition that I want you to consider is also the proposition that I want you to believe. This explicitly adds a step to the story that Grice suggested, but ultimately the mechanism is the same: an audience figures out on the basis of contextual information that my interest in having them recognize my intention to have them consider a proposition is, in fact, to have them come to recognize my intention to have them believe that proposition and thereby to come to believe it. And I believe that it is not implausible that this is what does happen even in the simplest cases of saying.

My story doesn't contradict Grice's story about speaker-meaning, but makes explicit a step that I believe was implicitly there in the first place. For Grice never says what mechanism or reasoning process is used or intended by a speaker when attempting to get an audience to recognize the speaker's intentions. I have found it helpful to make *part* of such intended mechanisms or reasoning processes explicit. Namely, in cases of speaker-meaning a speaker intends to have an audience come to recognize the speaker's intention to get the audience to believe a proposition by reasoning in part from their

recognition of the speaker's intention to get the audience to consider whether to believe that proposition.

Notice that if I intend (a) that you consider a proposition  $\pi'$ , (b) that you recognize this intention, and (c) that you reason from this intention to the belief that I want you to consider the proposition  $\pi$ , then I haven't said  $\pi$  to you, even if I might have *suggested* it. It seems that in that case I have said  $\pi'$  to you intending that you consider another proposition,  $\pi$ , and thereby come to adduce what I want you to believe. Consider the following case cited by Grice:

Take the complex example of the British General who captured the town of Sind and sent back the message *Peccavi*. The ambiguity involved ('I have Sind'/'I have sinned') is phonemic, not morphemic; and the expression actually used is unambiguous....<sup>2</sup>

In this case it seems that the General has *said* that he sinned, and he said this because he knew that his listener would come to consider the proposition that the General had captured Sind and go on from there ultimately to the belief that the general had captured Sind. It seems equally clear that the General did not *say* that he had captured Sind. Clause (II) of [S] is meant to help capture this latter fact. The speaker invites the listener to consider one proposition in order to go on from there to consider another, and then, ultimately, to go on to figure out what the speaker intends to get the listener to believe. But the second proposition considered here is not *said* by the speaker and an analysis of *saying* should predict this as mine does.<sup>3</sup>

Thus, the motivations for the various parts of [S] should be clear. It should also be clear how a theory of the actual-language relation that used such a notion as *saying* can avoid the Loar-style problem discussed above in section 5.5. For, if a theory of the

actual-language relation does not work by correlating utterances with what their utterer's intend to get their audiences to believe but by correlating utterances with what their utterer's primarily intend to get their audiences to consider, then the possibility that a sentence might never be uttered by a speaker to *mean* what it means need not matter. In the Loar case, the (supposed) fact that English speakers would tend to *speaker-mean* by "Apollo set the evening sky ablaze" that the sunset is intensely beautiful this evening forced his theory to identify that proposition as the meaning of the sentence. But English speakers would *say* that Apollo set the evening sky ablaze by uttering that sentence whatever they might *mean*. And it seems intuitively right to say that sentences mean what they can be used to *say*, not what speaker's intend to get their hearers to believe.

In Grice's "Logic and Conversation" Grice says the following about the notion of *saying* that he employs in discussing his theory of *implicature*:

In the sense in which I am using the word *say*, I intend what someone has said to be closely related to the conventional meaning of the words (the sentence) he has uttered.<sup>4</sup>

I am not entirely sure what Grice took the close relation to be between what is *said* and what is *conventionally meant*. But I believe that the relation between the notion of *saying* required by Grice's theory of implicature and the notion of *conventional meaning* is not a conceptual relation, that is, that the notion of *saying* required by Grice's theory of implicature does not metaphysically require *conventional meaning*. My notion of *saying*, I submit, can be used in the theory of implicature without any loss of explanatory power for that theory. My notion of *saying* does not require that utterances used for saying *conventionally mean* what they are used to say. It does turn out, in actual

situations, that we use the conventionally meaningful as devices for *saying*, but this is not of metaphysical necessity.

It is certainly conceivable that one can *say* a proposition by uttering a sound that has no conventional meaning and that one can do this in order to *implicate* some other proposition. Suppose that "Grrr" has no conventional meaning between you and I. But suppose that somehow I have reason to believe that if I utter the sound "Grrr" you will infer that I am trying to get you to consider whether to believe that I am angry. And suppose that I know that you know that I am not, in fact, angry. I want you to believe that a certain friend of ours is angry. So I utter "Grrr" intending to get you to consider whether to believe that I am angry, knowing that you know that I am not, and intending you to reason from your recognition that I intended you to consider whether to believe that I am angry along with certain facts I know to be known to you about our friend, that I am actually trying to get you to believe that our friend is angry. And I intend for you to reason from your recognition of my intention to get you to believe that our friend is angry to the belief that our friend is angry. Thus, in this case I haven't *said* that our friend is angry. Rather, I said that I was angry and I *implicated* that our friend was angry. And I did this without using any conventionally-meaning expressions.

It may be that for *us*, such a story as this is unlikely to occur and sounds a bit awkward for that reason. But, such a story is conceivable, and that is all that matters here. Neither *saying* nor *implicating* requires the use of conventional devices.

Now, it does turn out that conventionally-meaning expressions will generally be more suitable for generating *implicatures* because they are more *dependable* as devices



for getting people to consider whether to believe propositions. And this dependability is a way that *saying* ends up, as Grice suggested, closely related to using *conventionally-meaningful expressions*. But the connection is no closer than this: when we want to get someone to believe one thing by saying another perhaps we, as a matter of fact, always use conventional devices; but we don't have to. Grice's saying that *saying* is closely related to *conventional meaning* might be suggestive of a true identification given only our actual practices, but it is not conceptually necessary. There is no reason that the psychological mechanisms involved in creating implicatures should not work for communicators who have no conventional devices at their disposal.

In fact, I believe that Grice's theory of implicature even requires a more substantial notion of *saying* than merely *uttering an expression that conventionally means*. For when I *implicate* something, I do so, according to Grice's view, by intending an audience to consider the proposition that I *say*. I know that my audience will reason a certain way about that proposition, and I, in fact, intend them to do so. It is through recognition of such a complex intention concerning their reasoning and the proposition that I suggest they consider that I expect that I can succeed in getting them to understand what I am trying to *implicate*. But this complex intention is not merely the intention to utter a sentence that conventionally means something. Rather, I will sometimes utter a sentence that conventionally means something because I have this complex intention. And I believe that my notion of *saying* characterizes the complex intention required here.

It is hard, then, to see how a theory that tries to identify expression-meaning as some sort of correlation between sentences and propositional-speech-acts can avoid the

Loar-style problem if the propositional-speech-act it centrally employs is the traditional one of *speaker-meaning*, that is, of a speaker intending to get an audience to believe what is said. Thus I believe that my notion of *saying* is the more suitable notion and should be adopted for the theory of expression-meaning. This affects the theory of implicature somewhat. But the idea that the theory of implicature requires a notion of *saying* that is less robust than the notion that I have glossed is mistaken, as I have argued. Adopting my notion of *saying* may have a slight affect on the general way that one looks at the theory of implicature, but not on anything of substance in it. So there are no reasons not to adopt my notion of *saying* that stem from considerations of the theory of implicature.

### 7.3 The Unusable

If a theorist believes that, in fact, the number of meaningful sentences for communities of natural-language users is infinite, then that theorist will be faced with the task of saying how this infinitude of meaning facts can be possible for finite creatures such as us. Most attempts to do this have involved either supposing that some method can be found for basing the infinitude of meaning facts on facts about the finite corpus of actually uttered sentences or on relations in which language-users stand towards CMTs; some theorists have even tried both strategies at once.<sup>5</sup> The former strategy seems to entangle the theorist in *truthfulness-by-silence*-style problems, while the latter obligates the theorist to the existence of CMTs. I believe that neither of these options is viable for reasons that I have already discussed. And I know of no other way of understanding infinitudes of meaning facts for finite beings. So, I believe it is time to re-examine the

presupposition that the number of meaningful sentences for communities of natural-language users is infinite.

An important background idea that helps one to believe that natural languages are infinite in magnitude is Chomsky's famous *competence/performance* distinction. It is supposed that though we cannot process really large sentences, this is merely a fact about our actual abilities in performance, not our competence; our linguistic *competence*, so it is believed, is not limited by such facts about our actual *performance*. There is supposed to be an abstract level of description whereby, though we cannot *use* certain sentences, still, these sentences remain sentences of our language. And if there are to be infinitely many sentences in natural languages, then most of the sentences of natural languages will be unusable. So, I claim, behind the thesis that natural languages *in fact* contain infinitely many meaningful sentences lies this view that sense can be made of a notion of *competence with unusable sentences*. In the following I will argue that there really isn't a very good sense that anyone has ever made of this notion of *competence with unusable sentences*. For the idea that there can be such competence simply is the idea that there can be unusable but meaningful sentences. And note that my attack on the notion of *competence-with-the-unusable* is not an attack on the notion of *competence-with-the-unused*: there will surely be many many - if not infinitely many - sentences that we do have competence with, but which we simply have never and perhaps will never use. But whereas I believe that no good sense can be made of the notion of *competence-with-the-unusable*, I do believe that there is perfectly good sense to be made of the notion of *competence-with-the-unused*.

Many, if not most or even all philosophers, who have concerned themselves with such matters have taken the view quite seriously that there can be unusable but meaningful sentences. And this supposition has constrained theorizing about meaning as I indicated above. In fact, the only philosopher I know who has ever even suggested the possibility of a theory of meaning that denied that unusable sentences could be meaningful is Brian Loar. But as quickly as Loar suggests this possibility, he attempts avoiding it:

It is not wholly far-fetched to claim that the incomprehensibly complex English 'sentences' are not really part of *our* language at all - after all, we cannot understand them! If English were thus reduced to a finite fragment of what it would be if our brains were larger, there would be no problem for [a certain reading of a theory Loar suggests]. But that is a wildly controversial solution - and an avoidable one.<sup>6</sup>

I believe that the claim that the unusable lacks meaning is correct, even if it appears, as Loar says, "wildly controversial". But some have thought that at least some unusable sentences are meaningful. I believe that for the past 40 years or so the main source of this view has been a certain argument that *seems* to be suggested by Chomsky in various places - I don't know whether he ever has made an explicit such argument. Here is a passage that suggests this argument I have in mind from Chomsky's earliest publication on transformational grammar:

We might arbitrarily decree that such processes of sentence formation in English as those we are discussing cannot be carried out more than  $n$  times, for some fixed  $n$ . This would of course, make English a finite state language, as, for example, would a limitation of English sentences to length of less than a million words. Such arbitrary limitations serve no useful purpose, however.<sup>7</sup>

What exactly Chomsky intends to be arguing for in this passage is a debatable

matter. But, the argument that I think many philosophers have read into such considerations of Chomsky's goes something like the this: "if you only consider the data that comes from the 'intuitions' of natural-language users concerning the languages they speak, it will be impossible to draw a principled line that marks off where sentences become too complex to be processable; there is, therefore, no empirically motivated line to be drawn in characterizing linguistic competence and, therefore, we shouldn't draw one; so even really large sentences are to be considered meaningful if they are constructed from the syntactic rules that seem best to explain the finite set of intuitions actually considered in constructing a syntactic theory."

If this is an argument that Chomsky endorses, it seems to create a tension with views he later proposes concerning the arbitrariness of restricting the data which the grammarian in principle may use in constructing a theory to the 'intuitions' of natural-language users:

In principle, evidence ... [for the linguist] could come from many different sources apart from judgments concerning the form and meaning of expressions: perceptual experiments, the study of acquisition and deficit or of partially invented languages such as creoles, or of literary usage or language change, neurology, biochemistry, and so on. ... As in the case of any inquiry into some aspect of the physical world, there is no way of delimiting the kinds of evidence that might, in principle, prove relevant.<sup>8</sup>

If you believe that what is being investigated by the grammarian is a part of nature in the way that Chomsky seems to be suggesting here, it is hard to see how a conclusion about the potential complexity of sentences that is drawn from mere consideration of the corpus of 'judgments' or 'intuitions' of natural-language users can be held in high regard. If it seems doubtful that consideration of the corpus of everyday intuitions that we have about

the physical world would lead to a true physical theory, it should similarly seem doubtful that consideration of the corpus of intuitions of natural-language users about their language will lead to a true theory of that language. Perhaps sometimes extrapolations from such a corpus will lead to a truth about language, but it seems that if you believe language to be a phenomenon akin in relevant respects to physics or some other natural science, then you will have some healthy doubt about conclusions that you draw on the basis of people's intuitions. It may be that there are no limits on sentence-complexity discernable given only user intuitions. But given further data it could well become apparent that there are very definite limits on the grammar employed by a natural-language user. This is not something that can be determined ahead of empirical investigation.

Pressing the analogy with physics is interesting here. For it may have seemed not too long ago - perhaps even now still - that the physical world according to the "simplest" summary available of people's actual physical intuitions is such that it is possible to continuously extend a line without it's ever intersecting with itself. But, aside from questions about the notion of a *straight line*, it is understood that whether this is possible depends on complex issues about the density of matter in the universe or some such matters. It might be countered here that in the absence of further data, extrapolation from the corpus of intuitions would be the only rational means of guessing what the universe is actually like. So in previous days, conclusions drawn from the corpus of everyday physical intuitions *were* the rational conclusions to draw, however problematic they might be: it seems I am merely pointing to the underdetermination of theory by a finite set of

data.

The fact is that in the language case there already is additional data available. We know that we are finite creatures and that our processing abilities are finite. To extrapolate from the finite corpus of intuitions about particular sentences to conclusions about the potential complexity of any sentence whatsoever will have to be informed by this finiteness somehow.

Chomsky tries to accommodate infinite complexity in languages that are used by finite creatures by invoking the competence/performance distinction. But he doesn't tell us what competence amounts to: to do this is to give a theory of meaning in some sense close to that which is the topic of this dissertation. Chomsky doesn't do this. There is no theory of competence without a theory of meaning or ahead of a theory of meaning. To simply say that there is is to beg really important questions.

Seen in this light, the argument I gloss above clearly seems fallacious. This was originally pointed out by Ziff:

But where is the line between the deviant and the nondeviant to be drawn? Why think one has to draw lines? And to see that after all one needn't...one should ponder the fallacy of the sorites which one can learn to live with. ...One cannot argue that because there is no plausible line to draw between a poor man and a rich man when it comes to single pennies that therefore there's no difference between the two of them. ...One cannot sensibly argue that because one cannot find a cut-off point therefore one must go on forever. And yet that's exactly the move that is made in connection with sentences: Because there is no plausible cut-off point (as indeed there seems not to be) therefore one goes on forever. No: One cannot sensibly go on forever in a natural language.<sup>9</sup>

I agree with Ziff's diagnosis of the above argument: it appears to be a pedestrian instance of a sorites fallacy. This is not to say that given the finite corpus of user

intuitions about natural-languages a line is, after all, discernable. But the absence of a clear line is not an argument that sentences of arbitrary complexity are somehow actually available to us in some sense.

But if the argument above fails, then either there is some other argument that natural languages may be infinitely large, or else natural languages can't be infinitely large. What other argument is there that natural languages are infinitely large?

Perhaps consideration of the disquotational schema

" $\sigma$ " means among English speakers that  $\sigma$

along with the thought that the simplest syntactic theories for English will provide infinitely many syntactically well-formed English sentences lends support to the view that there will be infinitely many meaningful sentences of English and, by analogous considerations, of natural-languages generally. For many people seem willing to accept that each instance of the above disquotational schema will have to be true for instances where ' $\sigma$ ' is replaced by a something that is a well-formed English sentence according to a correct syntactic theory for English.

But such an argument rests on a hasty generalization. For, even if there were reason to think that English syntax somehow provided for infinitely many sentences, there is no reason to suppose the disquotational schema applies to all of these. I doubt that there will be any such thing as a unique correct syntactic theory for English - there will always be rival but adequate syntactic theories for any language given finite sets of linguistic 'intuitions' as data. Perhaps psychological data will help, somehow, to pick out a class of syntactic theories that in some sense describe aspects of what English speakers



represent to themselves in processing their sentences. But even if some such data isolated a single syntactic theory as the one that best describes such aspects of English speakers, and even if such a theory predicted that there were infinitely many syntactically well-formed English sentences, there is still absolutely no reason to believe that all of those sentences are meaningful for English speakers. To argue that all such sentences are meaningful it is necessary to provide some sort of characterization of *meaning* such that given such a syntactic theory the sentences it entails as well-formed are *meaningful*. That is, the present argument assumes that a theory of meaning has already been provided that makes sense of the idea that unusable sentences can be meaningful. I have no fancy *a priori* reasons for thinking that there cannot be such a theory. But no such theory seems forthcoming, as I have already argued, and it would run against the grain of the rather intuitive idea that expression-meaning is a matter of what can be used to *mean* by speakers.

I know of and can think of no other arguments that lends support to the thesis that natural languages are infinitely large. I also see no theoretical use that this supposition can have. So, I recommend that the supposition be dropped. To drop the thesis, however, is not to suppose that it is false. Perhaps it will turn out that there are unusable but meaningful sentences. My recommendation is that theorizing proceed on the basis of consideration of the usable. If we fail to come up with a theory of meaning for usable sentences, it seems unlikely that we will be able to come up with a theory of meaning that included the unusable ones.

Free from the constraint that provision has to be made for unusable but meaningful

sentences, the theorist of meaning can generalize over the sentences that are, in fact, meaningful for members of a community in the sense that they can be used by these people to say things to each other. If languages are infinite, then a theory that has it that the sentences of a language mean by virtue of facts about how they are used in such-and-such a way under these-and-those conditions will be questionable to the extent that it is implausible that anything interesting can be said about what people do or *would* do with unprocessable sentences. But without this constraining infinitude, a theory that made meaning a matter of correlations of sentences with propositional attitudes seems more of a possibility. I don't know what I would do if someone uttered a sentence that was more than two thousand words long with lots of center-embeddings or other complicated syntactic structures. But I am pretty sure that if someone uttered "Sinatra wants to tour next year with Metallica", I would come to believe that the person said that Sinatra wants to tour next year with Metallica, even if I never heard that sentence before. And I think that this is more important a fact for understanding the nature of meaning than the fact that the nicest syntactic theories that people have come up with for describing natural languages have allowed for infinitudes of sentences.

#### **7.4 The Non-conventional and the Unuttered**

Suppose that between you and I the expression "Grrr" has no conventional meaning but that either of us *can* use it to say that I am angry (ignore the indexical "I" here and throughout). This sort of situation can be made into what appears to be a counterexample to Schiffer's theory discussed above in chapter 6. For, consider the

language consisting of the single pair <"Grrr", that I am angry>. Call this language  $L_G$ . And suppose that I utter "Grrr" on a certain occasion to say to you that I am angry. Suppose that the first time I utter "Grrr" to you was the first time that either of us had done so to indicate our anger and let's say that my reason for believing that "Grrr" would have its intended effect on you is  $R_G$ . Whatever  $R_G$  is, since "Grrr" has never been used before by either of us, it will not be about the past usage of "Grrr". Suppose next that we come to utter "Grrr" frequently to each other after this, but that each time we have completely forgotten our past uses. Each time we use it our reason for believing that "Grrr" will be effective is  $R_G$ . This constitutes a practice among us of meaning in  $L_G$  since often when one of us wants to mean that we're angry, we choose uttering "Grrr" to do so. Since both of us will come to have the appropriate M-sentence in our belief-box and since this M-sentence will have to contain as a part an M-sentence that means in M what "Grrr" means in  $L_G$ , it follows that each of us must have an  $L_G$ -determining translator since an  $L_G$ -determining translator is *by definition* just something that produces such translations, whatever else it is. Thus, there is a practice among us of meaning in  $L_G$  and processing of  $L_G$ -sentences proceeds via an  $L_G$ -determining translator. So Schiffer's theory predicts that  $L_G$  is a language that we use, or, equivalently, that "Grrr" means among us that I am angry. But, it seems "Grrr" doesn't conventionally mean anything among us. Our reason for using "Grrr" is always  $R_G$  and if the presence of this reason for using "Grrr" the first time one of us used it wasn't sufficient somehow for "Grrr"'s being meaningful, then surely it is still not sufficient for its being meaningful after "Grrr" has been often repeated. If "Grrr" wasn't meaningful the first time it was

used, then it won't be meaningful the  $n$ -th time it is used no matter how big  $n$  is. So, this sort of case seems like it is a counterexample to Schiffer's theory.

But the reason I didn't raise this sort of case in chapter 6 when I discussed Schiffer's theory is that I don't think it needs to be seen as a counterexample. I think that, given the conditions of the story, "Grrr" can be said to mean among us that I am angry, even if it doesn't *conventionally* mean among us that I am angry. My view is that the notion of *conventional* meaning need not be seen as what we should primarily be aiming for when we search for a theory of meaning. *Meaning* can be looked at as a more general phenomenon than *conventional meaning*.<sup>10</sup> I will call a case like the "Grrr" case a case of *non-conventional expression-meaning*. I think that no harm is done in allowing a theory of expression-meaning to capture both *conventional* and *non-conventional* cases of expression meaning.

My main reason for holding this is that I cannot think of any goals that the theory of expression-meaning could serve that would not be just as well served by the more general theory as by the narrower one that restricted itself to *conventional meaning*, except for the goal of marking the line between the conventionally meaningful and the non-conventionally meaningful. This latter goal will itself be partially served by the more general theory since all cases of *conventional meaning* will be cases of *meaning* more generally construed: meaning, though not a sufficient condition, is a necessary condition for conventional meaning.

The main goal of a theory of expression-meaning is to say what it is about our marks and sounds, etc. that makes them mean what they mean. It seems perfectly

reasonable to suppose that the important answer to this question will include "Grrr"-like cases. For, it seems perfectly reasonable to suppose that the important aspect of our marks, sounds, etc. with respect to their meaning is their ability to be used to *say* things. Conventional signs might be tools more ready to hand, requiring less creativity for their availability than non-conventional signs, but they mean what they do for exactly the same reasons that non-conventional signs mean, namely, they both *can be used* by one person to *say* something to another. I believe that this is the essence of the notion of meaning and that the distinction between conventional and non-conventional meaning is not so important.

It might also very well be that there will not be a principled distinction between the non-conventional and the conventional cases of meaning. Perhaps the two types of cases bleed gradually into one another and there is not a sharp line between them. The so-called arbitrariness of an expression - its arbitrary relation to what it expresses - has often been taken to be a mark of its conventionality.<sup>11</sup> But notice that both non-conventional and conventional expressions can show a high degree of arbitrariness. Consider June's buffalo dance discussed above in section 1.2.4. The dance was used by June to say to her friend Bingo that a buffalo is charging nearby. But the dance is not tied to buffalo charging by much more than sheer coincidence - it is not in any interesting sense a *natural sign* of nearby buffalo charging. But what needs to be added to arbitrariness and the ability to be used to *say* something to make an expression a conventional one? I have already argued in section 5.4 that the notion of *convention* itself on closer scrutiny seems too strong a notion to capture the conventionality of an

expression. I don't know if any weaker notions will be able to mark the distinction between the non-conventional and the conventional in a principled way. But my point here is that even if this distinction couldn't be marked neatly, we need not give up the aims of theorizing about meaning since these aims will be fulfilled by a general theory that accounts for non-conventional as well as conventional meaning.

Of course, there will be many unuttered utterance types that in the relevant sense *could* be used by one person to say something to another. I am willing to think that these also ought to be included in the sorts of thing a theory of meaning is supposed to be about. I don't see any evil consequence of slicing things up in this way.

## 7.5 The Theory of Meaning

As I am conceiving things, the important thing for a theory of meaning is to state under what conditions an utterance type can be used by one person to say something to another. This is a somewhat different project from stating what are the necessary and sufficient conditions for an utterance type to conventionally mean what it does since some utterance types can be used to say things even though they don't have conventional meaning. But as I have tried to argue, I think that this shift in focus might be the right way to go. But still, this would be better shown if I could state what were the conditions under which an utterance type were meaningful in my new sense and if I could give an analysis of the notion of meaning that I think I am after. Here I can only give general considerations.

Since I am not constrained by the idea that the natural-languages that we actually

speak are infinite in magnitude, I can make a move that seems to have been unavailable to Schiffer in his theorizing. Schiffer's L-determining translators were not tied in any specific way to processing and this led to the possibility of the sort of counterexample I presented in chapter 6 where an L-determining translator is employed as a proper part of an L'-determining translator for an L'-speaker who does not speak L. If Schiffer could have tied the input and output of his L-determining translators to something identifiable in the noodles of language users, he might have avoided this sort of problem.

So it might be considered progress if an L-determining translator is defined as something that actually puts into the belief-box of a hearer an appropriate M-sentence when a public-language sentence is heard. So, we might want to say that an L-determining translator now is a device that doesn't merely determine a mapping, but is one that actually causes a certain sentence to end up in the belief box: if I hear you say "Sinatra really swings," then my English-determining translator should put in my belief-box an M-sentence that means that you said that Sinatra really swings.

This move can partly block the sort of counterexample I gave to Schiffer's theory since now a device that is a proper part of an L-determining translator will be irrelevant unless it also puts things in the belief-box. The counterexample seems defused.

The price here is the finiteness of natural-languages. No actual L-determining translator will be likely to determine an infinite language given the actual facts about our nervous systems and their limitations. But as I have argued, the finiteness of natural-languages is not at all a foregone conclusion.

The story wouldn't be over here anyway. For amending Schiffer's definition of

an L-determining translator in the way that I have suggested does not lead to an adequate theory of meaning, even for the finite languages that such a device would in some sense determine. The theory we would have would be something like this:

P uses L just in case processing of L-sentences proceeds in the members of P by means of an L-determining translator.

The requirement by this theory is obviously too strong. It need not be that every time I process an English sentence I come to believe that somebody said something in uttering it. For the above theory is equivalent more-or-less to the following:

P uses L just in case whenever a member of P processes an L-sentence  $\sigma$  uttered by a speaker S, that member of P comes to believe that S said  $\pi$ , where  $\pi=L(\sigma)$ .

But if this theory states too strong a condition, then what weaker condition will do? To answer this question is to state a condition on the input to an L-determining translator. For, the trick is to say what are the meaning-determining conditions under which a sentence gets passed to the input of an L-determining translator.

The form of a theory of meaning will then be something like this:

P uses L just in case whenever condition  $C^*$  is true of a member of P who is presented with an L-sentence  $\sigma$ , P comes to believe that the speaker said  $L(\sigma)$ .

I am not yet sure how to state what  $C^*$  is.



## Notes

1. My account of *saying* is not immune as it stands to the Strawson/Schiffer-style counterexamples to Grice's analysis of speaker-meaning. But I believe that this can be remedied by the sorts of suggestions Schiffer offers to help Grice's account of speaker-meaning. See Strawson (1964) and Schiffer (1972). I do not expect the analysis of *saying* that I am presenting here is immune to other sorts of problems as well. I am putting forward this notion of *saying* as "more suitable" than the Gricean notion of *speaker-meaning* for the purposes of the theory of expression-meaning. I don't expect that this is the final word here.
2. Grice (1967), p. 36.
3. Still, the point of my glossing *saying* is not centrally to provide an analysis of the ordinary notion expressed by the English "saying", but to come up with a notion that can be used in a theory of expression meaning.
4. Grice (1967), p. 25.
5. I have in mind, of course, Loar in Loar (1976) and Loar (1981), and Lewis in Lewis (1991) where he essentially repeats Loar's ideas. See above chapter 5.
6. Loar (1976), p. 158. See also Loar (1981), p. 256.
7. Chomsky (1957), p. 23.
8. Chomsky (1986), pp. 36-37.
9. Ziff (1974), p. 530. Ziff also cites Zellig Harris as a source of the argument that natural languages are in fact infinitely large.
10. Note that when I speak of *conventional meaning* here, I am not presupposing anything about the use of the notion of *convention* in a theory of *conventional meaning*. I take it that it is understood well enough what sort of cases count as cases of *conventional meaning*. As I discussed in chapter 5, it seems doubtful that the notion of *convention* should be used in glossing a notion of *conventional meaning*. But it is still customary to call the cases that I want to talk about here cases of *conventional meaning* and so I do so. Perhaps other terminology would be clearer here, but I prefer to stick with the customary locution right now.
11. See Whitney (1904) and Saussure (1916). Davidson makes the following remark about arbitrariness: "What is obvious enough to be a platitude is that the use of a particular sound to refer to, or mean, what it does is *arbitrary*. But while what is conventional is in some sense arbitrary, what is arbitrary is not necessarily conventional"

(Davidson (1981), p. 265).

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