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SCIENTIFIC EXPLANATION AND THE PHILOSOPHY OF LANGUAGE

DISSERTATION

Presented in Partial Fulfillment of the Requirements for
the Degree Doctor of Philosophy in the Graduate
School of The Ohio State University

By
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The Ohio State University
1976

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INTRODUCTION

The intent of this entire essay is to cast some substantial doubt on that view of human nature which would have us treat people as if they were automata. People are not literally automata, of course, but if one thinks of them as analogous to automata then, I believe, one is beginning to enter the mainstream of thought on the subject in contemporary British and American (certainly, American) philosophy. Thinking of people as analogous to machines in their psychological and social behavior as well as physiological behavior is a way of thought that it has taken several centuries to ingrain in us and even those who recognize it and do not like it still seem today unable to break free of it. What makes the analogy so pervasive so difficult to break free of, is the fact (what I think is a fact, anyway) that it is symbiotically related to a certain style, a certain strategy of explanation. What I shall call the scientific - what Hempel has called the deductive nomological. So long as one persists in evaluating all explanations in terms of this explanatory strategy one is giving tacit support to the analogy.

What this essay is about is the philosophy of language, in particular it is about the application of the scientific explanatory strategy to the cluster of problems, questions and quandries that center around this question: How is it possible for a person to speak and understand a particular natural language? My thesis is
simply that it is a mistake to think that one can produce satisfying explanations of our linguistic abilities by following this sort of strategy. In the first three chapters of this essay I describe just how this explanatory strategy has been employed by at least four current theorists - Chomsky, Grice, Searle and Davidson. So far as I can see, the point or end of scientific theorizing is the construction of an edifice as closely parallel in its formal structure to a logical calculus as possible. The abstracted pattern which a deductive argument must follow to be counted a valid argument is really a theory in miniature, so to speak. The acceptance of this geometrical (logical) model for theory construction has led to a standard for intellectual criticism that is as old as Plato, and as recent as Hempel. Indeed, it is the Hempelian model that, in my opinion, the four theorists mentioned above have made their own.

Now this logicogeometric model is not compulsory. Its charm rests in part at least, on the assumption that arguments and explanations in the formal or abstract sense - regarded as the products or results of the activities of arguing and explaining - can be, without any loss, separated or abstracted from these human activities. Any abstraction stands in danger of having focused on the wrong elements, of having abstracted from some complex, the wrong parts. Rather than concentrate on the structure of human arguments and explanations one could, it seems to me, approach these activities by considering instead their function, which in turn will lead one to give attention to their historical context and to the aims and purposes of the people engaged in these activities. In sketching (in the first two chapters in particular) a picture of the Hempelian explanatory strategy I have
Indeed introduced a condition on the adequacy of any explanation which is derived from the function of explanation, rather than from its structure. I call it the information condition. Adequate explanation of whatever structure must provide one with sufficient information to tell, solely from the explanans whether the explanandum is true. This, I take it, is the point of explanation - to have sufficient information at hand to tell from that information why or how something happened or more formally why a specification of what happened is true.

In the two critical chapters of this essay, four and five, I utilize this information condition to argue that the strategies of explanation recommended by Searle and Davidson are bound to be unsatisfactory. Neither can result in explanations which can satisfy this condition. In the concluding chapter I present my alternative, albeit rather incompletely worked out, to the Hempelian strategy. It is one that pays far more attention to the why and wherefore of satisfying explanation than the Hempelian model will allow. It focuses on the initiating assumptions of any explanation and finds the source of authority for their acceptance in something other than their logicogeometric role in performing valid deductions. Given my starting point, this result was perhaps inevitable; and no doubt a bit question begging. But I know of no way to argue for the inclusion of the information condition as a criteria for evaluating explanations, except to point out, as I have in the first two chapters, just how one or two of the most controversial issues in the philosophy of science (the interpretation of theoretical terms, the status of laws or lawlike statements) may be seen as stemming from a neglect of, or an over emphasis on this condition.
Nevertheless, I make no apologies for my introduction of the information conditions. What follows this introduction should be taken for what it is, an essay in the original sense of that word (which the dictionary tells us is archaic) - a tentative effort, a trial or incomplete attempt. Robert Nozick in his preface to a book which hasn't terribly much to do with my work (Anarchy, State and Utopia) gives voice to sentiments I would like to echo, and which I hope the reader will take to heart.

Works of philosophy are written as though their authors believe them to be the absolutely final word on their subject. But its not, surely, that each philosopher thinks that he finally, thank God, has found the truth and built an impregnable fortress around it. We are all actually much more modest than that. For good reason. Having thought long and hard about the view he proposes a philosopher has a reasonably good idea about its weak points; the places where great intellectual weight is placed upon something perhaps too fragile to bear it, the places where the unravelling of the view might begin, the unprobed assumptions he feels uneasy about. (p. xii)
CHAPTER I

WHAT IS A SCIENTIFIC THEORY

Most of what I shall have to say in this section will follow closely the remarks of C. G. Hempel in a number of different articles and books on this topic. I shall not pretend that the following discussion will provide a characterization of scientific theories, laws and explanations in any way as complex or subtle as Hempel's. But I do claim that what follows will provide a framework within which the explanatory strategies of Chomsky, Davidson, Searle and Grice may be discussed without misunderstanding.

It will be instructive to begin with a general characterization of the nature of scientific explanation. Following Hempel, we shall call the characterization of the phenomenon to be explained the explanandum; and the statements putatively explaining the truth of that characterization, the explanans. Consider the following example.

L: Water boils at 212 F.

C: The water on my stove is at 212 F.

E: The water on my stove is boiling.

E is the explanandum and L and C together constitute the explanans. In a non-scientific context, E would most likely be put in the form of a why-question; and the answer to the question (Why is the water on my stove boiling?) would most likely be limited to C. But it is
the conjunction of L and C that scientifically explains (the truth of) E. In general, a discussion of the nature of scientific explanation will be a discussion of the contribution made to ordinary, non-scientific explanation by the addition of statements like L. It will be helpful, then, to note two characteristics of non-scientific explanation before going any further.

In the first place, what I shall call the explanatory power of non-scientific explanation seems to reside (partly, at least) in the fact that the explanans consists of a statement (or set of statements) providing information of which the agent asking for the explanation was not aware. I shall call this point, the ignorance factor. Clearly, the demand for explanation is a demand for some information over and above that already possessed by the agent. And this brings us to the second characteristic we shall be interested in.

To say that the ignorance condition must be satisfied by an explanation, properly so called, is not to say that the agent does not understand the explanans which is offered him. To the contrary, the success of an explanation depends in part on the fact that the agent may be said to understand the explanans in question. If the explanans is not understood, if it contains terms with which the agent is not familiar, for example, '212 F.', then the point of the explanation will have been frustrated. The agent will have acquired no information over and above that which he already possesses. I shall call this factor, the information factor. We shall have occasion to discuss both of these factors (conditions) later in this chapter. We shall see that a good deal of the controversy over the nature of scientific explanation concerns the roles played by these two factors
Now what difference to non-scientific explanation does the addition of statements like \( L \) make? We may note immediately that the addition of \( L \) turns the explanation into what looks like a deductive argument. \( L \) and \( C \) are related to \( E \) as the premises and conclusion in a deductive argument are related. From the information conveyed by \( L \) and \( C \), and only from that information, one is in a position to tell whether the explanandum is true. \( E \) follows deductively from \( C \) in virtue of the generality of \( L \). \( L \) is the statement of "...a uniform connection between different empirical phenomena or between different aspects of an empirical phenomenon." In this case, \( L \) is a statement of a uniform connection between the behavior and temperature of water. "It is a statement to the effect that whenever and wherever conditions of a specified kind \( F \) occur, then so will always and without exception, certain conditions of another kind, \( G \)." (NS, p. 54)

In scientific explanations, statements like \( L \) are called empiric laws or lawlike statements. "They provide the link by reason of which particular circumstances...can serve to explain the occurrence of a given event." (NS, p. 54) However, not all statements like \( L \) may be characterized as empiric laws.

For example, the sentence, 'All rocks in this box contain iron.' is of universal form (\( F \) is the condition of being a rock in the box, \( G \) that of containing iron); yet even if true, it would not be regarded as a law, but as an assertion of something that "happens to be the case", as an accidental generalization.

Thus a scientific law cannot be adequately defined as a true statement of universal form: this characterization expresses a necessary, but not a sufficient, condition for laws of the kind under discussion. (NS, p. 55)
The question of what distinguishes empiric laws from accidental
generalizations is one that has been much discussed by philosophers
of science. Without going into detail, it will suffice for our
purposes in this chapter to note only the following. Whether a
statement like \( L \) - an empirical generalization - counts as an empiric
law "...will depend in part upon the scientific theories at the
time." (NS, p. 57) While having the form of a generalization is
necessary without being sufficient, we may say that being implied
by a currently accepted theory is sufficient without being necessary,
for a statement to be counted an empiric law.

We shall look a little more closely at the relation between
empiric laws and scientific theories in a moment. For now there
is one more point to be noted concerning the difference the inclusion
of a statement like \( L \), an empiric law, makes to what would otherwise
be a non-scientific explanation. The addition of \( L \) makes it possible
to say that \( E \) could have been predicted, or as it is sometimes called,
retrodicted.

If \( E \) is given, i.e., if we know that the phenomenon described
by \( E \) has occurred and a suitable set of statements is provided
afterwards, we speak of an explanation of the phenomenon in
question. If the latter statements are given and \( E \) is derived
prior to the occurrence of the phenomenon it describes, we
speak of a prediction. It may be said, therefore that an explanation
is not fully adequate unless its explanans, if taken account
of in time, could have served as a basis for predicting the
phenomenon under consideration. ²

Indeed, according to Hempel, part of the success of non-
scientific explanation resides in the fact that statements like \( L \),
when they are not explicitly mentioned, are elliptically or enthymatically
omitted from the explanans. C non-scientifically explains \( E \) only
because \( L \) is assumed perhaps as too obvious to mention.

Such explanations are sometimes expressed in the form 'E because C', where E is the event to be explained and C is some antecedent or concomitant event or state of affairs. Take, for example, the statement: 'The slush on the sidewalk remained liquid during the frost because it had been sprinkled with salt.' This explanation does not explicitly mention any laws, but it tacitly presupposes at least one: that the freezing point of water is lowered whenever salt is dissolved in it. Indeed, it is precisely by virtue of this law that the sprinkling of salt acquires the explanatory...role that the elliptical because-statement ascribes to it. (NS, p. 52)

The inclusion of statements like \( L \) in the explanans of otherwise non-scientific explanations enables us to treat such explanations as if they were deductive arguments. And the fact that the explanandum is then overtly deducible from the explanans enables us to say that the explanandum could have been predicted (retrodicted) had the information provided by the explanans been available prior to the occurrence of the phenomenon.

However, focusing too much on the predictive potential provided by statements like \( L \), while it seems to highlight the role played by (what I have called) the information factor, tends to obscure the role played by the ignorance factor. Clearly, we could not have predicted that the slush would have remained liquid during the frost had we not been in possession of the information provided by the explanans in question. But there must have been a time when we were ignorant of the information provided by the explanans. Otherwise the statements composing the explanans could not have sufficed to explain why the slush remained liquid. If satisfaction of the information condition is necessary for prediction then satisfaction of the ignorance condition is necessary for explanatory power; and no scientific explanation is fully adequate unless it satisfies both of these conditions.
With this in mind, let us turn our attention to the role which scientific theories play in scientific explanation. We have already noted that part of their function is to help us distinguish empiric laws from accidental generalizations. This is not to say that "...'empirical generalizations' - the statements of universal form that are empirically well confirmed but have no basis in theory - never qualify as laws: Galileo's, Kepler's and Boyle's laws, for example, were accepted as such before they received theoretical grounding." (NS, p. 58) It is to say that one desideratum behind the construction of scientific theories is the desire to explain the truth of empiric laws in the same way that empiric laws may be used to explain the truth of some characterization of some particular phenomenon.

Theories are usually introduced when previous study of a class of phenomena has revealed a system of uniformities that can be expressed in the form of empirical laws. Theories then seek to explain those regularities and, generally, to afford a deeper and more accurate understanding of the phenomena in question. To this end, a theory construes those phenomena as manifestations of entities and processes that lie behind or beneath them. These are assumed to be governed by characteristic theoretical laws, or theoretical principles, by means of which the theory then explains the empirical uniformities that have been previously discovered.... (NS, p. 70)

Within the realm of scientific explanation we may distinguish, what we may call, empiric explanation, from what we may call, theoretic explanation. Empiric explanations are distinguished from non-scientific explanations by the appearance in the explanans of an empiric law. Theoretic explanations may be distinguished from empirical explanations by the appearance in the explanans of what we may call a theoretic law. They may also be distinguished by the fact that the explanandum of an empiric explanation is the characterization of some
particular phenomenon. The explanandum of a theoretic explanation is the statement of an empiric law.

Consider the following (simplified) example of theoretic explanation.

T: Water is composed of particles in constant motion which when heated to 212 F. acquire enough energy to escape the forces which would otherwise keep them in the same locale.

I: Boiling is the appearance presented when the particles so escape.

L: Water boils at 212 F.

T and I together explain the truth of L in the same way in which, in our previous example of empiric explanation, L and C explained the truth of E.

A theoretic explanation then is one in which the explanans is related to the explanandum in the same way in which the premisses of a deductive argument are related to its conclusion. And its explanans must contain some statement characterizing some kind of phenomenon as possessing or exhibiting certain properties under certain conditions.

We can now provide a prima facia answer to the question that launched this chapter. A scientific theory is a set of statements like T and I above - a set of statements that may be used to explain the truth of a statement like L, an empiric law. To the extent that we are attempting to explain theoretically the truth of an empiric law, the explanans must provide information of which we are ignorant prior to the explanation, (the ignorance factor); but the statement of that information must be understandable once it is formulated (the information factor).
In theoretic explanation, the ignorance condition is virtually guaranteed satisfaction. Theoretic laws putatively characterize phenomena that are hidden from view, that lie behind or beneath the phenomena characterized in empiric laws. Indeed, it is just this fact that seemingly insures their increased generality (over that of empiric laws) and so, insures an increase in their explanatory power (over that of empiric laws).

In a field of inquiry in which some measure of understanding has already been achieved by the establishment of empirical laws, a good theory will deepen as well as broaden that understanding. First, such a theory offers a systematically unified account of quite diverse phenomena. It traces all of them back to the same underlying processes and presents the various empirical uniformities they exhibit as manifestations of one common set of basic laws. (NS, p. 75)

However, this strength is also a weakness. To the extent that theoretical laws do characterize phenomena that are (at least metaphorically) hidden from view, it is not easy to see how theoretic explanation may satisfy the information condition. It is at least not obvious that the explanans of a theoretic explanation will be understandable once formulated, given that the theoretic laws essential to its formulation apparently make us of expressions characterizing entities and processes with which we are not (and perhaps cannot be) acquainted (if that is how we are to understand the metaphors, 'hidden from view', and 'behind or beneath the surface' - in terms of acquaintance (or our lack of it) with the phenomena in question).

Moreover, if the explanans of a theoretic explanation is not understandable it is difficult to see how such explanans could have any predictive force. That is, it is not obvious that the explanans could have been used to predict the truth of the explanandum had the
information provided by the explanans been available prior to the attempt to (theoretically) explain the explanandum. Since the explanans, if not understandable, would not seem to provide any information at all. And without predictive, or at least retrodictive, potential, scientific theories would not be susceptible of empirical test.

Now the above few remarks clearly require some modification on at least two fronts. In the first place, as our earlier example illustrates, the explanans of theoretic explanations are not composed entirely of theoretic laws. Statements like I - "Boiling is the appearance presented when the particles escaped." - also occur. Hempel has called such statements, 'bridge principles' (Carnap has called them, 'correspondence rules').

...[bridge principles]...will indicate how the processes envisaged by the theory are related to empirical phenomena with which we are already acquainted, and which the theory may then explain, predict or retrodict. (NS, p. 72)

Bridge principles insure that the increase in explanatory power achieved by the use of theoretic laws is not dissipated, as it were, by insuring (in a way that, if current controversy is any indication, is not yet fully understood) that the information provided by such laws is susceptible of being understood. Hence, bridge principles provide some guarantee that theories are, after all, testable.

Without bridge principles, as we have seen, a theory would have no explanatory power. Without bridge principles, we may add, it would also be incapable of test. For the internal principles of a theory are concerned with the peculiar entities and processes assumed by the theory...and they will therefore be expressed largely in terms of characteristic "theoretic concepts", which refer to those entities and processes. But the implications that permit a test of those theoretical
principles will have to be expressed in terms of things and occurrences with which we are antecedently acquainted, which we already know how to observe, to measure, and to describe. In other words, while the internal principles of a theory are couched in its characteristic theoretical terms ('nucleus', 'orbital electron', 'energy level', 'electron jump') the test implications must be formulated in terms (such as 'hydrogen vapor', 'emission spectrum', 'wavelength associated with a spectral line') which are "antecedently understood", as we might say, terms that have been introduced prior to the theory and can be used independently of it. (NS, p. 74)

Now the second clarifying modification I have to make has to do with the above mentioned controversy concerning bridge principles. As I have earlier noted, the power of an explanation (whether non-scientific, empiric, or theoretic) resides, partly at least, in the fact that the information provided by the explanans is information of which the agent wanting the explanation was not antecedently aware. What makes an explanation satisfying (what gives it its power or force) seems to be the fact that its explanans brings to light, makes known to us, acquaints us with, some factors, entities, processes or reasons, which were hidden, unknown, of which we were ignorant or unaware, prior to the formulation or statement of the explanans. Whatever else is involved, the satisfaction of this ignorance condition seems to be a sine qua non of explanation (whether non-scientific, empiric or theoretic).

It is quite interesting to note in this connection that as fond as Wittgenstein apparently was of claiming that the point of philosophical investigation was neither explanation nor the formulation of theories, he nevertheless located the source of philosophical problems in something hidden from us, something that we could see if only we had eyes, if only we were not bewitched by our language.
It was true to say that our considerations could not be scientific ones... And we may not advance any kind of theory... We must do away with all explanation, and description alone must take its place. And this description gets its light, that is to say its purpose, from the philosophical problems...

These are, of course, not empirical problems, they are solved, rather, by looking into the workings of our language, and that in such a way as to make us recognize those workings: in despite of an urge to misunderstand them. The problems are solved, not by giving new information but by arranging what we have always known. Philosophy is a battle against the bewitchment of our intelligence by means of language.

The power of philosophical investigation, the satisfaction it can provide, resides for Wittgenstein in just the same neighborhood as does the satisfaction provided by any explanation, properly so called; namely in its presenting us with information of which we were ignorant or unaware prior to its formulation or statement. I find it quite curious that he should insist that it is not the business of philosophy to explain anything, while at the same time, so to speak, recommending as a sine qua non of philosophic investigation that it consist in "...assembling reminders for a particular purpose..." (PI, 127); as if its purpose were to make known to us something we were not aware of, whether because we never know or because we forgot. And I find it curious that he should say that "...philosophy simply puts everything before us, and neither explains nor deduces anything - since everything lies open to view there is nothing to explain. For what is hidden, for example, is of no interest to us." (PI, 126), and then, only three paragraphs later, say that "...the aspects of things that are most important for us are hidden because of their simplicity and familiarity. (One is unable to notice something - because it is always before one's eyes)." (PI, 129)
If we are to lay my curiosity to rest, if we are to make some (charitable) sense of Wittgenstein's remarks here, it would seem that we may take his comments to be in reaction to a certain school of thought (one he endorsed in his early years). We may suppose that Wittgenstein is reminding us that the phrases, 'hidden from view', and 'behind and beneath the surface of familiar phenomena', are metaphorical phrases, not to be taken literally. We may suppose that what Wittgenstein is emphasizing here is that such phrases may be used to indicate that the information provided by any explanans is only new information, information of which we were not previously aware, in the sense that such information is being used to explain something; and not in the sense that such information is totally new, new in the sense that it would be new even if it had not been used to explain something. To be told that the car crashed because one of its tires blew out, is an explanation of the car crash, not because one has no idea of what having a tire blow at fifty miles an hour is like, but, rather it is because one does know, only one was simply not aware that that is indeed what happened.

This school of thought (as I have characterized it) is not made of straw. To realize this, one has only to review the literature published on this subject during the last few decades. The distinction between empiric laws and theoretic laws has historically been put in something like the following way.

...[empiric]...laws contain only terms that refer to observables or are operationally definable (such terms as 'pressure', 'velocity of fall', 'sulfuric acid', or 'proportion of tall sweet pea plants in the sample'), whereas the statements of theories contain at least some terms that do not refer to observables and are not operationally definable.4
The presumption behind such a distinction is that a statement characterizing some empiric phenomenon is understandable only if some, at least, of the terms used in its formulation refer, or denote, some phenomenon (or type of phenomenon) with which we are or have been acquainted. Theoretic laws, since theoretic, seem to involve the use of some terms referring to, or denoting, phenomenon with which we are not, and have never been, acquainted. Hence, the statement of such laws satisfies the ignorance condition in the strongest sense; but because they do, it seems that they cannot prima facia satisfy the information condition. If we suppose our understanding of any statement to be (in part, at least) a function of our acquaintance with the entities and processes referred to, or denoted by, some of the terms of such statements, then, in lieu of such acquaintance (i.e., in satisfying the ignorance condition) one just cannot be said to understand the statement of a theoretic law; the information condition would seem to be doomed to frustration by the satisfaction of the ignorance condition when it comes to theories.

Insisting on this acquaintance view of understanding has led to two alternative accounts of (what I have been calling) bridge principles, reductionism and instrumentalism. Briefly, reductionism supposes that theoretic terms, terms used to denote or refer to phenomena with which we are not acquainted (phenomena which are hidden from view) may be exhaustively defined in terms of expressions which denote or refer to phenomena (or operations) with which we are acquainted (observation terms). Bridge principles, then, provide such definitions. In the terms we have been using to discuss this issue, we may say that reductionism strives to insure satisfaction of the
information condition, but the price it pays is frustration of the ignorance condition. In general, reductionism, because it frustrates the ignorance condition, fails to account for the fact that theories do increase our knowledge of the world.

...it has been demonstrated in detailed artificial examples by Frank Ramsey and Braithwaite and argued in general terms by many others that if explicit definitions of all theoretical terms by means of observables could be carried out, theories would be incapable of growth and therefore useless. Theories are required to be general and predictive and therefore capable of assimilating an indefinite number of new observations without themselves radically changing in meaning. Explicit definitions could not leave room for this and could not exhaust the potentially infinite and largely unknown range of observables to which the theory might be relevant. (EP, p. 406)

Instrumentalism, on the other hand, may be viewed as an attempt to deny that the information condition must be satisfied at all in the sense that the information provided by the explanans of a theoretic explanation is to be information regarding entities and processes with which we could, in principle, at least, become acquainted.

Instrumentalists assume that theories have the status of instruments, tools, or calculating devices in relation to observation statements. In this view it is assumed that theories can be used to relate and systematize observation statements, and to derive some sets of observation statements (predictions) from other sets (data); but no question of the truth or reference of the theories themselves arises. (EP, p. 407)

Bridge principles on this view provide one (among a number of at least possible) interpretations of the otherwise uninterpreted non-logical terms appearing in the test sentences somehow generated by the machinery of the theory. The test sentences, once interpreted, will be empiric laws stated in terms of expressions refering to phenomena with which we are acquainted. The information condition will not be satisfied by the explanans of any such theoretic explanation because the explanans was not intended to provide us with any information
in the first place, but only with a convenient calculating device whereby new empiric laws may be generated.

A less extreme version of instrumentalism would have us treat a theory as the result of interpreting all (or most) of the non-logical terms used in laying out some formal calculus. Such a view makes the distinction between observation terms and theoretic (non-observation) terms a function, not of our acquaintance or non-acquaintance with the phenomenon denoted by some term, but rather of the term's location, as it were, in an interpreted calculus. If the term is to occur in the axioms of the calculus, then we might call it a theoretic term, if in the theorems or postulates deduced from the axioms, an observation term.

This modified version of instrumentalism (sometimes called, realism) requires the overthrow of the prejudice that there is some basic language the terms of which may be understood independently of any knowledge of their role in some (perhaps implicit) theory. Arguments in behalf of the overthrow of this prejudice have sometimes taken the form of attempts to show that the distinction between observation terms and theoretic (non-observation) terms cannot be made without taking into account the current development of science and the sophistication of the observers.

For example, sulfuric acid is not an "observable" to those ignorant of chemistry; 'air' might now be said to refer to an observable for most laymen, but for the pre-Socratics it was a debatable theoretical term; and a physicist might say he observed in a cloud chamber the creation of a particle-pair when a layman would see only two white streaks. Furthermore, some terms which must surely be said to be theoretical if any are, such as 'the mass of the electron', are also in a sense operationally definable, since one can specify experimental situations in which electrons are present and one can measure their mass. (EP, p. 405)
Moreover, attempts to preserve this distinction by making it
dependent upon the meaning of the statements to which such terms
make a contribution have also been questioned. The statement of
empiric laws has been supposed (by Nagel, for example) to have a
truth value (and a meaning) that is independent of the changing
theories that are used to explain them.

...[empiric]...laws in this view have a rock bottom,
incorrigible status that is denied to theories. Any
theory is subject to revision or replacement, but any
subsequent theory, if it is to be acceptable, must be consistent
with the laws previously known to be true. (EP, p. 405)

Occasionally a law deduced from some theory may be preferred to
one generalized from observation, either because the deduction is
more accurate than any direct experimental evidence available, or
because it actually corrects a law generalized from data subsequently
found to be inaccurate. A theory will occasionally show that

...the previously formulated empirical laws that it is
meant to explain do not hold strictly and unexceptionally,
but only approximately and within a certain limited range
of application...a theory does not simply refute the earlier
empirical generalizations in its field; rather, it shows that
within a certain limited range defined by qualifying
conditions, the generalizations hold true in fairly
close approximation... (ns, p. 76)

If one is convinced by such arguments then it becomes incumbent
upon one to explain just how it is that theoretic explanation may satisfy
the information condition. If an understanding of the use of some terms,
theoretic or not, is not to be supposed to be a function of our
acquaintance with the phenomenon (or type of phenomenon) denoted by
such terms, then just how are we to explain our understanding of
the use of any term; and in particular, how are we to explain our
understanding of the use of some theoretic terms?
A few pages back in discussing (some of) Wittgenstein's remarks on the nature of philosophic explanation I commented on the apparent metaphoric force of the phrases, 'hidden from view', and 'behind and beneath the surface'. Rather than literally characterizing the epistemic status of the kind of phenomenon described in theoretic explanation, such phrases may be taken to metaphorically characterize our own state of ignorance regarding the information conveyed by the explanans of any explanation. In empiric explanation our ignorance is reflected, not in our lack of understanding of the explanans, but in our ignorance of the information conveyed by the explanans prior to its statement. In theoretic explanation, however, our ignorance seems indeed reflected in our lack of understanding of the explanans in question (as well as in our consequent ignorance of the information conveyed by such theoretic explanans).

Consequently, in order for theoretic explanation to have any explanatory power, this apparent, rather dramatic, failure to satisfy the information condition must be somehow explained away, particularly so, if, for example, 'air' was once, but is no longer, a debatable theoretic term. We do apparently come to understand, given time at least, the explanans of some theoretic explanations. How is this possible?

In general, there are two schools of thought on this question. We can find a succinct description of each in an article by Braithwaite.

...usually, and in all interesting cases, the initial hypothesis of...[a]...theory will contain concepts which are not purely logical but which are not themselves observable (call these, theoretical concepts); examples are electrons, Schrodinger wave functions, genes, ego-ideals. A fundamental problem for philosophy of science is how these theoretical concepts should be understood. One school of thought, represented
most prominently by N. R. Campbell, holds that the only way to understand these concepts and hence to understand the explanatory hypothesis of the scientific theory, is to represent the theory by a model whose deductive arrangement corresponds to that of the theory but in which all the concepts concerned are familiar concepts...Familiar concepts, but not necessarily observable properties or relations. What the modellists (as I shall call them) think necessary is that the correlates, should be understood, and they may be understood as being theoretical concepts of a simpler theory which is already understood.5

This modellist (as Braithwaite calls it) school of thought is opposed by what Braithwaite calls the contextualist school. Rather than supposing that model theoretic bridge principles are essential for an understanding of some theoretic explanans, contextualists suppose that satisfaction of the information condition follows upon one's understanding of the role(s) played by some theoretic terms in deriving or deducing empirically testable generalizations, that is, in deriving or deducing antecedently understood explananda.

Such an account (which I will call the contextualist account) has been given by many recent writers: Quine in his recent book (Word and Object) refers to works by Carnap, Einstein, Frank, Hempel and myself, and this list could be expanded. Briefly, the contextualists hold that the way in which theoretical concepts function in a scientific theory is given by an interpretation of the calculus expressing the theory which works from the bottom upwards. The final theorems of the calculus are interpreted as expressing empirically testable generalizations, the axioms of the calculus are interpreted as propositions from which these generalizations logically follow, and the theoretical terms occurring in the calculus are given a meaning implicitly by their context, i.e., by their place within the calculus. So an understanding of a theoretical concept in a scientific theory is an understanding of the role which the theoretical term representing it plays in the calculus expressing the theory, and the empirical nature of the theoretical concept is based upon the empirical interpretation of the final theorems of the calculus. (MES, p. 231)

In a more current article, Feyerabend characterized the modellist view thusly:
According to Carnap, Feigl, Nagel and others the terms of a theory receive their interpretation, in an indirect fashion, by being related to a different conceptual system which is either an older theory or an observation language. Older theories, or observation languages are adopted not because of their theoretical excellence (they cannot possibly be: the older theories are usually refuted). They are adopted because they are 'used by a certain language community as a means of communication'.

Feyerabend takes the guiding idea behind this view to be "...that new and abstract languages cannot be introduced in a direct way but must be first connected with an already existing and presumably... understood...idiom." (CS, p. 223) He thinks that this guiding idea "...is refuted at once by pointing to the way in which children learn to speak and in which anthropologists and linguists learn the unknown language of a newly discovered tribe." (CS, p. 223) He thinks that we can come to understand the language of new theories without model theoretic bridge principles.

What is overlooked is that an initially ununderstood alternative (language) may be learned in the way in which one learns a new unfamiliar language, not by translation, but by living with the members of the community where the language is spoken. (CS, p. 223)

What Braithwaite seems to have overlooked is that his account of how we may come to have an understanding of the use of some theoretical terms is an account dependent upon our having some antecedent appreciation or understanding of the workings of an uninterpreted logical calculus. Feyerabend goes beyond him here by supposing that learning how to speak and understand a new theoretic language is like learning how to speak and understand any new language. It remains to be seen whether learning to speak and understand a new language is like learning to use, and understand the use of, for example, the set theoretic notion of class inclusion through an
understanding of its use in deriving some set theoretic theorems.

We do come to understand the language of new theories, and we do learn to speak and understand new natural languages (whether as children or as bilinguals); Feyerabend seems to think that, however we are to go about explaining how it is possible for a child to learn to speak and understand a language, mutis mutandis, that explanatory strategy will explain how it is possible for us to learn to speak and understand a new theoretic language. He recommends that we "...study the language of new theories not in the definition factories of the double language model, but in the company of those metaphysicians, experimenters, theoreticians, playwrights, courtesans, who have constructed new world views." (CS, p. 225)

This point would seem to be a rather appropriate one with which to conclude this chapter. In the next chapter, save one, we shall be concerned to discuss just how such theorists as Chomsky, Davidson, Searle and Grice, who are principally concerned to explain how it is possible to speak and understand a natural language, recommend (either explicitly or in their practice) that we go about doing such explaining. The point of this chapter has been to provide a characterization of the role played by theories in scientific explanation that is sufficiently detailed to enable us to discuss without too much misunderstanding what I take to be the explanatory strategy adopted by the above four theorists, namely, scientific explanation. What, then, have we learned to this point?

We have learned that the explanatory power of any explanation (whether theoretic or not) stems (in part) from its satisfaction of, what I have called, the information condition, on the one hand, and
the ignorance condition on the other. A putative explanation satisfies the ignorance condition just in case its explanans conveys information of which the agent wanting the explanation was unaware prior to the statement of the explanans. The information condition is satisfied just in case the agent does understand the information conveyed by the explanans once it is stated. Furthermore, we have learned that the relation between explanans and explanandum must be like the relation between premisses and conclusion of a valid deductive argument. That is, this relation must be such that from the information conveyed by the explanans, and from that information alone, an agent must be able to tell if the explanandum is true.

We have learned that a scientific explanation (whether empiric or theoretic) must involve in the statement of its explanans, some lawlike statement - "...a statement to the effect that whenever and wherever conditions of a specified kind F occur, then so will always and without exception, certain conditions of another kind, G." (NS, p. 54) It is the generality of such lawlike statements that, together with satisfaction of the information conditions, insure not only the deducibility of explanandum from explanans, but also the testability of such lawlike statements. It is their generality - that they relate kinds of phenomena - which insures their use in predicting the truth of explananda otherwise not known to be true.

We have learned that a scientific theory is a set of lawlike statements distinguished from empiric laws, not only in their increased generality - they are to be used to explain the truth of empiric laws - but also in that their statement satisfies the ignorance condition in a way that the statement of empiric laws does not. The
use of empiric laws in explanation satisfies the ignorance condition just to the extent that prior to the statement of such explanans we were not aware of the information conveyed by the explanans. The use of theoretic laws in explanation satisfies the ignorance condition not only in the way the statement of empiric laws satisfies it, but also because, at least initially, the statement of theoretic laws involves the use of terms with which we are not antecedently familiar, which we just do not understand.

This guaranteed satisfaction of the ignorance condition by theoretic laws prima facie seems to spell frustration for the information condition, and consequently, to defeat the purpose of introducing such laws in the first place. But this is only a prima facia result, for the fact of the matter is that we have come to understand the statement of various and sundry theoretic laws, both those of the past and those of today.

A major debate in the philosophy of science has centered around explaining how it is possible for us to come to understand theoretic laws when the non-logical vocabulary of such laws apparently guarantees satisfaction of the ignorance condition. In the immediately preceding pages we have (admittedly in a rather oversimplified way) discussed some of the proposals that have been offered - reductionism, instrumentalism, modellism, and contextualism (or realism). Each of these may be taken to be alternative accounts of the role played in theoretic explanation by what I have called (following Hempel) bridge principles - principles (rules) relating (in some way) the antecedently understood terms in which the explananda of theoretic explanation are stated to the non-logical vocabulary of the theoretic laws which are, at least,
Initially, not understood at all.

Some criticism of both reductionism and instrumentalism (at least of the more extreme variety) was offered. Both views seem to be founded on the mistaken assumption that one may be said to understand the use of some non-logical term only if one is (or has been) acquainted with the phenomenon (or kind of phenomenon) denoted, or referred to, by (the use of) that term. No decision was reached regarding whether bridge principles are to be construed as alternative (analogical) interpretations of the non-logical terms of the calculus underlying the theory in question (modellism), or whether bridge principles, even if model theoretic, are unnecessary heuristic devices (contextualism or realism).

In general, any decision in favor of one or the other of these last two mentioned alternatives must wait upon a decision concerning how we may best explain how we do learn to speak and understand a natural language. We know that we do come to have an understanding of theoretic languages, so it must be possible to explain how this is possible. And our relation (so to speak) to a theoretic language is, at least initially and apparently, not any different from that of the child to a natural language. Of course, on the face of it, learning a theoretic language must seem more like learning a second language, than a first. But learning any language is apparently, and perhaps metaphorically, a matter of gradually increasing one's expertise. Going from English to German seems to be no more of a dramatic increase in one's linguistic expertise than going from baby talk to some version of Standard English.
The fact of the matter is that we do learn to speak and understand a natural language. We want to know just how this is possible. We want an explanation of the truth of the generalization: people do learn to speak and understand natural languages. The question is should that explanation be framed in terms that describe the means (the kind of strategy or technique) used (by ourselves and others) to teach us how to speak and understand a language; or should that explanation be framed in terms that describe an ability to learn to speak and understand a language, which ability we possess regardless of the (kind of) means used to teach us to speak and understand a language?

If the latter, then model theoretic bridge principles are, as the contextualists (or realists) suppose, unnecessary heuristic devices. They are unnecessary in the sense that our learning to speak and understand a (theoretic) language does not depend upon any particular set of such principles, though it may be hastened (but also impeded) by some such set - hence, their heuristic value. We learn to speak and understand a language in virtue of an ability we possess (apparently because we are people and not apes).

It is my contention that contemporary theorists of language as prima facia diverse as Chomsky and Davidson, on the one hand, and Searle and Grice, on the other, are of the opinion that this latter view regarding how we are to go about explaining how it is possible for us to learn to speak and understand a language (natural or theoretic) is correct. It is my contention that these four theorists (and their cousins) believe that a theory of language should be composed of a set of statements which are descriptive of some state of mind or body of any person who may be correctly said to possess the ability to speak
and understand a language. That is, a theory of language is supposed by these four theorists to be a set of statements descriptive of some state of a person if and only if it is true that that person can speak and understand a language.

Before going on I want to emphasize that the above remarks should not be taken as an attempt to describe the actual motives behind the theorizing of any of the above mentioned theorists. I have no idea whether any of them actually are interested in substantiating the contextualist (realist) account of scientific explanation sketched earlier. Nevertheless, it does seem to me that, speaking broadly, the explanatory strategy adopted by all of them will result in a theory of language that may be used to support such a view of explanation.

In the next chapter I shall briefly outline the main points of a scientific explanatory strategy as it applies to the theory of language. I shall leave it to the succeeding chapter to substantiate my claim that each of these four theorists does, each in his own way, believe that a theory of language must be constructed along those lines.
FOOTNOTES FOR CHAPTER I

1 C. G. Hempel, Philosophy of Natural Science, Prentice Hall, 1966, p. 54. Hereafter referred to as NS.


In this chapter I shall briefly discuss just how a scientific theory of language should be constructed. That is, I shall utilize what we have learned from the last chapter about scientific explanation in general in order to sketch what such an explanatory strategy applied to the study of language ought to look like. Along the way we shall occasionally digress from the main theme to discuss some of the current controversies concerning the application of such strategy to the explanation of linguistic phenomena.

As I indicated at the close of the last chapter, a scientific theory of language is, strictly speaking, a set of statements (hopefully) descriptive of some state of mind or body of a person who can speak and understand a language. As I have also been at pains to point out, theoretic statements (properly so called) supply us with new information about the world, satisfy the ignorance condition on explanation not simply because we are unaware of the information supplied by such statements prior to their formulation, but also, and more dramatically, because such statements are framed in terms (theoretic terms) the use of which we just do not, at least initially, understand.

In its present state of development, we can only say that the statements composing a theory of language, that putatively describe
some state of mind or body of a person who can speak and understand a language, represent the structure of such a state. The relations alleged to hold among the non-logical terms of the theory are alleged to represent the structure of this state of mind or body in much the same way that the physical processes of a computer programmed to model the brain are alleged to represent in their temporal sequence the temporal sequence of processes occurring in the brain.

For example, we find Davidson talking about "...recovering the structure of a very complicated ability - the ability to speak and understand a language."¹ as a result of the successful construction of a theory of language. We find Chomsky and Searle (and a wealth of others) speaking of a speaker's having internalized, or having tacit, or intuitive knowledge of, or having developed an internal representation of, the rules or statements putatively describing his ability to speak and understand a language.

As one might have expected this way of speaking about the relation between the statements composing a theory of language and the state of mind or body they are alleged to represent has occasioned a good deal of controversy. Chomsky is of the opinion that "...linguistic theory may be regarded as an abstract theory of language acquisition: it postulates certain "innate predispositions" that enable the child to develop competence on the basis of primary linguistic data."² He believes that the ability to speak and understand a language is innate, and the ability to speak and understand a particular language, e.g., English, is acquired.

...[theories]...of individual languages thus provide the empirical basis for construction of a theory that deals with the common structure of natural languages. This theory leads to specific hypotheses about the innate principles underlying language acquisition. (LT, p. 349)
To the extent that theories of different particular languages will differ, Chomsky seems to be following a time honored scientific procedure of supposing that there is some higher, more general theory that may be used to explain each of these particular theories. The statements composing this higher theory will describe the relations obtaining among linguistic universals, and will represent some innate schematism or framework which enables the child to learn to speak and understand a particular language.

The speaker-hearer does not acquire competence by inductive generalization from regularities in a corpus of primary linguistic data; rather a rich framework of linguistic universals defines a set of..."humanly possible grammars"...Language learning is a process of determining which of these is the actual grammar of the language to which the child is exposed. The procedure, we suggest, is akin to theory construction. Learning a language is a matter of selecting among humanly possible grammars, filling in details in a fixed framework or schematism. (LT, p. 349)

Ranged against Chomsky's innateness hypothesis are those theorists who believe that we may explain how a child may acquire the ability to speak and understand a language without having to posit the existence of anything innate. How a child learns a language will occasion a theory of how one learns anything. It will be a study on the one hand of the strategies and techniques that may be used most efficiently to teach something, and on the other hand, a study of the biological and psychological mechanisms involved in acquiring from scratch, so to speak, the appropriate state of mind or body necessary and sufficient for the truth of the attribution of any ability. There is no need to posit a unique and innate language acquisition device, the innate capacity we have to learn anything is, according to Chomsky's opponents on this issue, more than sufficient to do the job.
Many of the fiercest partisans of empiricist and behaviorist learning theories are willing to concede that the child has innate learning capacities in the sense that he has innate dispositions, inclinations and natural potentialities.... W. V. Quine, for example, in his response to Chomsky's innateness hypothesis argues, "The behaviorist is knowingly and cheerfully up his neck in innate mechanisms of learning readiness" Indeed, claims Quine, "Innate biases and dispositions are the cornerstone of behaviorism." 3

One way of looking at this debate is to see it as centering around the question of whether language acquisition is indeed so surprising and so complex that a special unique innate mechanism must be posited in order to explain it. Searle puts the case in behalf of such a unique mechanism in the form of a dilemma.

The behaviorist and empiricist learning theorist who concedes the complexity of grammar (which complexity and the surprise it is supposed to engender upon realizing that a child can master it is used by Chomsky as evidence for his innateness hypothesis) is faced with a dilemma: either he relies solely on stimulus-response mechanisms, in which case he cannot account for the acquisition of the grammar or he concedes a la Quine, that there are innate mechanisms which enable the child to learn the language. But as soon as the mechanisms are rich enough to account for the complexity and specificity of the grammar then the stimulus-response part of the theory, which was supposed to be at its core, becomes uninteresting; for such interest as it still has now derives entirely from its ability to trigger the innate mechanisms that are now the crucial element of the learning theory. Either way, the behaviorist has no effective reply to Chomsky's arguments. (CR, p. 23)

The reply to this dilemma is to take it by one of its horns and argue that the surprise engendered by language learning is a function of the complexity of the theory constructed to explain our (acquired) ability to speak the language in question. If theorists were to come up with less complex theories, this surprise would be proportionally diminished until it perhaps does not exist. Of course, as Harman notes,
If someone wishes to argue that language learning is not so surprising, it is not enough that they point out that there may be alternatives to Chomsky's view. What they need to do is to develop an alternative account that makes language learning look less surprising.

Now at this point one might naively suppose that this controversy can be resolved by first taking a look inside the head of a person we know can speak a language and then comparing what we find there to what we find upon opening the head of a newborn babe. Unfortunately, even if the present level of neuro-physiological research were able to make such a detailed inspection, there seems good reason to believe that such a procedure could not, even in principle, resolve the controversy. At least if one is inclined to accept Quine's arguments for indeterminancy there is good reason.

Without going into detail, Quine's thesis is this:

...there is no correct (or incorrect) answer to the question of whether a sentence \( S \) is an accurate translation of another sentence \( S' \), except (trivially) relative to some pre-chosen translation manual.\(^5\)

The effect this thesis has in this context may be stated thusly: appealing to the actual neuro-physiological state of a person who can speak a language, even if it were practically possible, will not help to settle the controversy over the innateness of any set of truths supposedly representative of some neuro-physiological state, since any neuro-physiological description of such a state will have to be interpreted in, or translated into, English or whatever language the theorist has chosen to state his set of truths. And, given alternative sets, there seems to be no non-question begging way of pulling off this translation, short of knowing in advance just how to translate neurological states (or their descriptions in the language of biology) into English (the pre-chosen translation manual,
which since pre-chosen, would be question begging); if we have such
a translation manual, then all good philosophers would be, or should
become, neuro-physiologists.

If theories A and B account equally well for the facts,
does it make any real sense to suppose that the language
learner internalizes the rules of grammar A rather than
those of grammar B?...Even if we open up the brain and discover
a representation there of the rules of grammar, we cannot
expect the representation to be in English; it will require
interpretation, and will receive different interpretations
from proponents of different theories. (RLM, p. 217)

Now to this point it should be clear that which ever side one
takes on Chomsky's innateness hypothesis, the fact remains that a theory
of language is to be a set of statements putatively describing some
state of mind or body of a person who can speak and understand a
particular language, e.g., English. (Parenthetically, I must note that
to this point I have been trading on an ambiguity involved in the
phrase, 'speaking a language'. I have left it an open question whether
it is language in general, so to speak, or some particular language,
for example, English, that has been under discussion. In so far as
I am not interested in defending or attacking Chomsky's innateness
hypothesis, nothing I have so far said hinges on this ambiguity.
But for clarities sake, I want to note that from here on, whenever I
use this (potentially) ambiguous phrase, I shall be talking only about
speaking a particular language like English.) Whether innate mechanisms
specific for language learning are necessary to explain this fact or not,
the ability to speak and understand a language, possessed by a person
who can speak and understand a language, is presumed by all concerned
to be a state of mind or body of that person, however it is acquired.

Quine's indeterminancy thesis only underscores the fact, already
mentioned, that the statements composing a theory of anything, let
alone language, cannot be directly verified. We must decide upon their truth by deciding upon their efficacy in producing explanations of the truth of generalizations about the range of phenomena which is their subject matter. In case we are dealing with a theory of language, its test rests in its capacity to produce explanations of the truth of generalizations about linguistic phenomena.

A linguistic generalization is the analogue in a theory of language of such empirical generalizations as that water boils at 212 F. Like empirical generalizations they are statements to the effect that a certain kind of phenomenon is always accompanied by certain kinds of conditions. And like empirical generalizations linguistic generalizations must be framed in terms the use of which we must understand prior to any understanding we may acquire of the use of theoretic terms.

Linguistic generalizations are statements about the kind of phenomena that is the subject matter of a theory of language. Searle describes this subject matter briefly and well, I think.

"...we do know that people communicate, that they do say things and sometimes mean what they say, that they are, on occasion at least, understood, that they ask questions, issue orders, make promises, and give apologies, that people's utterances do relate to the world in ways we can describe by characterizing the utterances as being true or false or meaningful, stupid, exaggerated or what not. And it these things do happen it follows that it is possible for them to happen, it ought to be possible to pose and answer the questions that examine that possibility."

We may say that the kind of phenomena which is the subject matter of a theory of language is the various and sundry utterances people produce in the course of communicating with one another. A linguistic generalization is, then, a statement to the effect that a certain kind or type of utterance is, e.g., true or meaningful or
exaggerated, or plausible, or a promise or a warning or a description or a question, or what not, under certain kinds or types of conditions. Such linguistic generalizations may be used to explain the truth of particular linguistic characterizations, e.g., that some utterance-token (some particular instance of the utterance-type mentioned in the generalization) is true or a promise or meaningful or what not. The explanation here will work in much the same way as any empiric explanation. One wants to know why some particular utterance (some utterance-token, some instance of an utterance type) is, for example, true. The explanans will contain a linguistic generalization to the effect that utterances of that type may be characterized as true under certain types of conditions; and then it will be pointed out that those conditions obtain in this case, hence, that particular utterance is correctly characterized as true. The relation between explanans and explanandum, here, is like that between the premises and conclusion of a deductive argument. From the information provided by the premises and only from that information one should be able to tell whether the explanandum is true - one should be able to retrodict the truth of the explanandum.

It is important to note here that the explanandum is a linguistic characterization of a particular utterance (an utterance token), a characterization of a particular utterance as true, or a promise or what not. It is the truth of such characterizations of utterance tokens that is to be explained by a linguistic generalization, not simply the truth of the particular utterance in question, but rather in this case, the truth of the characterization of the utterance as, e.g., true or a promise or what not.
Confirmation of a theory of language at this level depends not only on our ability to characterize particular utterances in the various ways that we do, but also on our ability to tell whether any particular characterization is correct. We may not know exactly why it is correct to characterize some utterance as a promise, but we must, at least initially, be able to tell whether some utterance is a promise. Linguistic generalizations provide us with an explicit statement of what we may not have known (they satisfy the ignorance condition), but when stated they must provide us with information which is sufficient to tell whether we would characterize an utterance of the type in question as, e.g., a promise, under the conditions specified (they must satisfy the information condition).

This point underscores a rather major difference between explanation in a theory of language and, say, in a theory of physics. Linguistic generalizations are not statistical generalizations. The information recorded in a linguistic generalization is not information garnered from a succession of past observations. We need not take a survey to see if some linguistic generalization is correct or not. If one knows how to characterize an utterance as true or meaningful or a promise, then one knows in some sense the conditions under which an utterance may be characterized as true, or meaningful or a promise. Whether a linguistic generalization is acceptable then, depends upon whether one would characterize an utterance-token of the type of utterance in question under the conditions in question. And if one can speak and understand a language then one must be able, at least, to characterize utterances as true, meaningful or a promise.
It is, as Searle notes, "...not always easy to characterize one's skills..." there is the general difficulty in correctly formulating knowledge that one has prior to and independent of any formulation; of converting knowing how to knowing that. We all know in one important sense what "cause", "intend", and "mean", mean, but it is not easy to state exactly what they mean." (SA, p. 14) But that is exactly what a linguistic generalization does - state, for example, what some utterance means. Nevertheless it is just because we do know how to characterize utterances as true or meaningful or as meaning that p, that we can confirm a linguistic generalization without an empirical survey of speakers.

The mistakes we...will make in...[formulating linguistic generalizations]...will be due to such things as not considering enough examples or misdescribing the examples considered, not to mention carelessness, insensitivity, and obtuseness; but, to repeat, they will not be due to over hasty generalization from insufficient empirical data concerning the verbal behavior of groups, for there will be no such generalization nor such data. (SA, p. 13)

Confirmation of a theory of language will come with a speaker's agreement that a particular linguistic generalization (deducable from the theory) may be used to explain the truth of some particular linguistic characterization. We must not lose sight of the fact that our assent to a particular linguistic generalization is founded on our realization that that generalization may be used to explain the truth of whatever particular linguistic characterization is in question.

It is our ability to speak and understand a language that enables us to decide whether some linguistic generalization is acceptable, and it is just this ability which a theory of language is to represent. Should it turn out that the information supplied by a linguistic generalization is not in principle sufficient to enable us to decide
whether we would under the conditions specified characterize some utterance token in the way in question, then we must reject that particular linguistic generalization. And if it can be shown that this result holds good for any linguistic generalization then we can only conclude that any theory of language founded on the idea that some set of statements, descriptive of some state of mind or body of a person who can speak and understand a language, may be used to explain the various expressions of that ability in our linguistic characterizations is simply mistaken.

In chapter five I shall argue that this result is indeed just the result a scientific theorist of language must face. In chapter four I shall present some considerations designed to show that this scientific approach was wrongheaded from the start; I shall there argue that the attribution of an ability is not descriptive, and so that it is simply a mistake to think that such an utterance as, "S can speak and understand his language", is used to describe some state of mind or body of the person in question.

But we are still a long way from chapter four let alone five. First I need to show that Chomsky, Davidson, Searle and Grice all do endorse this scientific explanatory strategy. But before getting into the details of each of these theories I want to quickly note two further points.

The major difference between Davidson and Chomsky, on the one hand, and Searle and Grice, on the other, rests in their different construals of the nature of the phenomena characterized in some linguistic characterization. This difference is reflected in the apparent ambiguity involved in our use of 'utterance'. It is not
clear whether, in speaking of an utterance token, one is speaking of the act of uttering something, what was done by a speaker; or of the result of such an act, what was uttered by a speaker. Let us call the former an utterance act and the latter, an utterance object.

In characterizing an utterance act we are characterizing what was done by a speaker on an occasion, what the speaker did. In characterizing an utterance object, we are characterizing what was uttered by a speaker on an occasion, what the speaker uttered. The importance of this difference may best be appreciated by considering the difference it will make in the kind of linguistic generalization to be used in explaining linguistic characterizations of either sort.

If we want an explanation of the truth of some particular linguistic characterization of an utterance act, the generalization used in the explanans will have to specify the type of conditions under which what the speaker did may be characterized in the relevant way; alternatively, in case what is characterized is some utterance object, the generalization in question will have to specify the type of conditions under which what a speaker uttered may be characterized.

In the former case, the conditions specified will be conditions which the behavior of the agent in question must satisfy. In the latter case, the conditions will be conditions which the utterance object must satisfy. In stating a linguistic generalization usable to explain some characterization of an utterance object, we shall have to treat what was uttered as an instance of some type of utterance object. And we shall have to treat the conditions provided by such a generalization as a specification of the properties possession of which by an
instance of the type of utterance object in question explains (or retrodicts) the truth of the linguistic characterization in question.

In explaining some characterization of an utterance act, we shall first have to decide just how best to distinguish linguistic behavior from non-linguistic behavior; i.e., we shall have to decide how best to distinguish the kind of behavior we are willing to characterize linguistically (as a promise, or as meaningful, for example) from the kind of behavior we would not so readily characterize linguistically. This decision will determine just how we are to go about specifying the conditions alleged in a linguistic generalization to be those under which an instance of some kind of behavior may be characterized linguistically.

Now Chomsky and Davidson are clearly of the opinion that what stands in need of explanation (at the very bottom of a theory of language, so to speak) is the truth of our linguistic characterizations of particular utterance objects. They both agree that, insofar as particular utterance objects may be considered instances of what we may call sentence types, one principle condition on the adequacy of a theory of language is that it contain the wherewithall to generate a potentially infinite number of sentence types. A person who can speak and understand a language seems able to produce and understand a potentially infinite number of sentence tokens (utterance objects); hence, a theory of language composed of statements putatively representing some state of mind or body of a speaker, must as well represent this ability to produce and understand a potentially infinite number of sentence tokens given only finite resources.
Searle and Grice, on the other hand, are clearly of the opinion that what stands in need of explanation is the truth of our characterizations of instances of utterance acts. As we shall see, Grice is of the opinion that what distinguishes an utterance act from other kinds of behavior is that it is produced with certain intentions. Searle, however, takes the view that instances of utterance acts may best be viewed as instances of rule following behavior.

In the next chapter we shall first consider Davidson, then Chomsky, Searle and Grice. Of the four, only Davidson and Searle will be given any further consideration (in chapter five). My reasons for omitting Chomsky and Grice will become apparent in the next chapter.


CHAPTER III

FOUR SCIENTIFIC THEORISTS OF LANGUAGE

In this chapter I intend to make good on my promise to substantiate my claim that Davidson, Chomsky, Searle and Grice all practice (or preach) the philosophy of language in, what I have called, the scientific manner. Each in his own way (with the exception of Chomsky, as we shall see) proposes a type of linguistic generalization usable in explaining the type of linguistic characterization each considers of principle importance - "'s' is true", for Davidson, "'s' is a promise", (or some other type of speech act) for Searle, and "U meant something by uttering x", for Grice.

No doubt I shall not do full justice to the intricacies of the views of any of these four theorists, but then my aim is only to provide just enough information to make my point concerning the contemporary practice of the philosophy of language, and of course enough to allow my readers to make sense of my reasons (which will be presented in chapters four and five) for disparaging such practice.

Davidson

It is not difficult to find in Davidson's articles on the subject a clear and concise statement of the adequacy conditions he believes must be satisfied by a theory of a language. He seems to
spend time in almost all of his articles making, in one way or
another the following points:

An acceptable theory should...account for the meaning...of
every sentence by analysing it as composed...of elements
drawn from a finite stock. A second natural demand is that
the theory provide a method for deciding, given an arbitrary
sentence, what its meaning is. (By satisfying these two
conditions a theory may be said to show that the language
it describes is learnable and scrutinable.]

We do learn to speak and understand a language. A theory designed
to explain how this is possible by representing the structure of
a very complicated ability must be so constructed that it shows
how, from a finite stock of words and rules, a potentially infinite
number of meaningful sentences can be produced.

...[A]...satisfactory theory of meaning must give an account
of how the meanings of sentences depend upon the meanings of
words. Unless such an account could be supplied for a
particular language, it is argued, there would be no explaining
the fact that we can learn the language; no explaining the
fact that, on mastering a finite vocabulary and a finitely
stated set of rules, we are prepared to produce and to
understand any of a potential infinitude of sentences.]

Furthermore, someone who can speak and understand a language
can distinguish meaningful from meaningless utterances just because,
for the most part, he knows what an utterance means, if it means
anything at all. Consequently, an adequate theory of a language
must not only,

(A1) ...define a predicate of expressions, based solely
on their formal properties, that picks out the class
of meaningful expressions (sentences)... (TM, p. 387)

but it must also,

(A2) ...specify in a way that depends effectively and solely
on formal considerations, what every sentence means.
(TM, p. 387, cf. also SNL, p. 184 and TM, p. 454)

In terms of our earlier discussion, Davidson seems to be
placing particular importance in the following points. People who
can speak and understand a language can produce and understand a potentially infinite number of utterance objects (tokens of utterance, i.e., sentence, types). A theory of language must somehow reflect this ability.

Secondly, and perhaps more interestingly, Davidson has decided that, of all the ways available for characterizing utterance objects (for example, as a promise, a warning, as exaggerated, and so on) the most important way is not only as meaningful but also as meaning that p. That is, Davidson has decided that an adequate theory of a language must be composed of a set of statements that may be used to explain the truth of a potentially infinite number of linguistic generalizations to the effect that a certain type of utterance object is, not only meaningful, but also means that p. A good deal of his time in most of his articles on the subject is devoted to describing just how a theory of a language may be so constructed as to meet these two demands. We shall spend most of the rest of this section summarizing just what his recommendations are.

In the first place, we have noted, the ways provided by such a theory for picking out the meaningful sentences and for giving their meaning(s) must be recursive in character. That is, an adequate theory of meaning must explain how the meaning of a sentence depends upon the meanings of its parts (words). And this because, "...there seems to be no clear limit to the number of meaningful expressions...[so]... a workable theory must account for the meaning of each expression on the basis of the patterned exhibition of a finite number of features." (SML, p. 177)
In the second place, we must not suppose that in constructing an adequate theory of a language we are faced with two distinct, autonomous chores, recursively characterizing sentencehood and giving the meaning(s) of each member of the fixed finite stock of elements (words) out of which a meaningful sentence may be composed. That is, "...recursive syntax with dictionary added is not necessarily recursive semantics..." (TM, p. 454)

The point is easily illustrated by belief sentences. Their syntax is relatively unproblematic. Yet, adding a dictionary does not touch the standard semantic problem, which is that we cannot account for even as much as the truth conditions of such sentences on the basis of what we know of the meanings of the words in them. (TM, p. 454)

I think the point may also more elaborately be illustrated by the failure of (what W. P. Alston has called) entitative theories of meaning. Assigning an entity as meaning to each significant syntactic element of a meaningful sentence gives one a concatenated series of entities, not the meaning of the sentence.

Assigning an entity to only some of the significant syntactic elements of a sentence, e.g., just those expressions, however complex, which can serve, or be made to serve, as the subject term of a meaningful sentence (ruling out, of course, the so-called syncategorematic terms), will not help. Nor will it do to suppose the sentence is a truth function of other sentences all of whose elements are Names of the chosen entities. The problem remains; the meaning of a sentence (even if a special atomic sentence) is not given by producing (if one only could) a concatenated series of entities.

Assigning an entity to the sentence as a whole (as a last resort) has one advantage but (at least) two crucial drawbacks. The advantage is that such a move, "...behooves us...to rephrase our demand on a
satisfactory theory of meaning so as not to suggest that individual words must have meanings at all, in any sense that transcends the fact that they have a systematic effect on the meanings of the sentences in which they occur." (TM, p. 451)

The disadvantages are these: having already noted that the class of meaningful sentences is potentially infinitely large, positing such entities makes, "...talk of the structure of the sentence and of the meanings of words...idle, for it played no role in producing...the meaning of the sentence." (TM, p. 453)

The Fregean answer to the above disadvantage brings with it the second drawback I mentioned. Frege would have us distinguish between sense and reference, with the result that there is no infinitude of entities referred to by the (potential) infinity of meaningful sentences, but only two such entities, the True and the False. And to avoid the result that all sentences alike in truth value must be synonymous, we make meaning a function of sense rather than reference. "If we want a theory that gives the meaning (as distinct from reference) of each sentence, we must start with the meaning (as distinct from the reference) of the parts." (TM, p. 452)

Unfortunately, treating the meaning of the sentence as something other than the reference of the sentence, "...leads to no useful account of how the meanings of sentences depend upon the meanings of the words...that compose them." (TM, p. 452) It would seem such an account only replace one kind of entity with another, more mysterious, one.

Ask, for example, for the meaning of 'Theaetetus flies'. A Fregean answer might go something like this: given the meaning of 'Theaetetus' as argument, the meaning of 'flies' yields the meaning of 'Theaetetus flies' as value. The vacuity of this
answer is obvious. We wanted to know the meaning of 'Theatetus flies'; it is no progress to be told it is the meaning of 'Theatetus flies'. (TM, p. 453)

All of this should be familiar ground. I have covered it again only to give some bite to my remark earlier that constructing a theory of a language that satisfies conditions (A1) and (A2) must not be supposed to require two autonomous tasks. In recursively defining the predicate which is to pick out the class of meaningful sentences in a language, (A1), we must at the same time, so to speak, be specifying a way to give the meaning of every meaningful sentence in the language, (A2).

One consequence of so conflating (A1) and (A2) is, in Davidson's words, "a certain holistic view of meaning."

If sentences depend for their meaning on their structure and we understand the meaning of each item in the structure only as an abstraction from the totality of sentences in which it features, then we can give the meaning of any sentence (and word) in the language only by giving the meaning of every sentence (and word) in the language. Frege said that only in the context of a sentence does a word have meaning; in the same vein he might have added that only in the context of the language does a sentence (and therefore a word) have meaning. (TM, p. 454)

Simply put, an adequate theory of a language is one which can account for the workings of a natural language; "...the task of a theory of meaning...is not to change, improve or reform a language, but to describe and understand it." (TM, p. 460)

Obviously, if we have a recursive definition of a predicate which does give the meaning of every meaningful sentence in the language under study then we have in one good sense accounted for the workings of that language; so "...nothing stands in the way of putting what I am calling a theory of meaning into the form of an explicit definition of a predicate 'is T'." (TM, p. 455) Such a theory will have done its
work, it will have accounted for the workings of a language,
"...if it provides for every sentence in the language under study, a
matching sentence...that in some sense yet to be clear, 'gives the
meaning' of ...[that sentence]...." (TM, p. 455) Providing we can
get clear on how one sentence can "give the meaning" of another, suc-
cess in defining the predicate 'is T' will show how the meanings of
sentences depend upon the meanings of words by demonstrating, step
by step how the meaning of any sentence depends upon a recursively given
structure. (TM, p. 184)

This is to say, a satisfactory theory of a language is one
which provides an effective method for arriving at the meaning of
an arbitrary sentence structurally described. Clinging to an entitative
theory of meaning this demand requires, "...a theory that has as
consequences all sentences of the 's means m' where 's' is replaced
by a structural description of a sentence and 'm' by a singular term
that refers to the meaning of that sentence...." (TM, p. 453)

But as we have seen an entitative theory will not do. And
it is important to remember why. Davidson does not argue that the
phrase "the meaning of E" is not a referring phrase. Rather, his
objection to treating meanings as entities stems from the failure
of such theories to satisfy the adequacy conditions (A1) and (A2).
"My objection to meanings in the theory of meaning is not that they
are abstract or that their identity conditions are obscure, but
that they have no demonstrated use." (TM, p. 453)

Consequently, if we are to retain the demand that a theory of
a language provide an effective method for arriving at the meaning
of an arbitrary sentence structurally described, then the problem
is to, "...get rid of the troublesome singular terms supposed to replace 'm' and to refer to meanings." (TM, p. 455) So rather than refer to meanings, what replaces 'm' in such a linguistic generalization as 's means m', must be a sentence that in some way gives the meaning of 's'. Davidson's solution is to "...sweep away the obscure 'means that', provide the sentence that replaces...['m']...with a proper sentential connective, and supply the description that replaces 's' with its own predicate." (TM, p. 455) the result is the following schema:

\[(T) s \text{ is } T \text{ if and only if } p.\]

We are now in a position to say how a theory of meaning can meet the demand that it provide an effective method for arriving at the meaning of an arbitrarily chosen sentence structurally described. That is to say, we now have (the beginnings of) a way to test the adequacy of a theory of some as yet unspecified language.

What we require of a theory of meaning for a language L is that without appeal to any (further) semantical notions it place enough restrictions on the predicate 'is T' to entail all sentences got from schema (T) when 's' is replaced by a structural description of a sentence of L and 'p' by...[a sentence that gives the meaning of that sentence].... (TM, p. 455)

How does what replaces 'p' in an instance of (T) give the meaning of the sentence structurally described by what replaces 's'? Davidson's answer relies on our noticing that schema (T), "...the condition we have placed on satisfactory theories of meaning, is in essence Tarski's Convention (T) that tests the adequacy of a formal semantical definition of truth." (TM, p. 455)

Given this not so amazing coincidence, the heretofore rather obscure demand that the sentence replacing 'p' in an instance of schema (T) give the meaning of the sentence structurally described...
by what replaces 's' can be clarified. What replaces 'p', gives the meaning of a sentence by giving the truth conditions for that sentence. Indeed, the mysterious predicate, 'is T', demanded by (A1) and (A2), is now no longer mysterious. It is just the predicate Tarski has shown us how to define in constructing his theory of truth for formalized languages.

...a theory of meaning for a language L shows how the meanings of sentences depend upon the meanings of words if it contains a (recursive) definition of truth-in-L. And, so far at least we have no other idea how to turn the trick. It is worth emphasizing that the concept of truth played no ostensible role in stating our original problem. That problem, upon refinement, led to the view that an adequate theory of meaning must characterize a predicate meeting certain conditions. (TM, p. 455)

And, as luck would have it, Tarski has already shown us how to construct a predicate satisfying just those conditions. It is Davidson's contention that we must not let "dust from futile and confused battles" over whether Tarski's semantical concept of truth has "cast any light on the ordinary use of such words as 'true' and 'truth', prevent those with a theoretical interest in language - philosophers, logicians, psychologists, and linguists alike - from recognizing in the semantical concept of truth (under whatever name) the sophisticated and powerful foundation of a competent theory of meaning." (TM, p. 456)

The definition of truth-in-L (of the predicate required by (A1) and (A2)),

...works by giving necessary and sufficient conditions for the truth of every sentence in L and to give the truth conditions is a way of giving the meaning of a sentence. To know the semantic concept of truth for a language is to know what it is for a sentence, any sentence, to be true, and this amounts, in one good sense we can give to the phrase, to understanding the language. (TM, p. 456)
What remains to be done is to see how Davidson supposes a Tarski style theory of truth can be made to apply to a natural language like English. The difficulty Davidson's program faces at this point may be summed up thusly. He began by supposing that a sine qua non of an adequate theory of language is that it should produce generalizations usable in explaining the truth of particular characterizations of utterances as meaningful and as meaning that \( p \), e.g., 'Snow is white' means that snow is white. He decided (he never really argued for it) that if a theory of language could produce generalizations usable in explaining the truth of particular characterizations of utterances as true, e.g., 'Snow is white' is true, then it would, in doing so, explain the truth of characterizations of utterances as meaningful and as meaning that \( p \), all in one fell swoop, as it were. Part of his reason for this decision lies in the fact that Tarski has already provided us with the machinery capable of producing generalizations usable in explaining the truth of characterizations of utterances in a formal language as possessing a certain property. The difficulty lies in the fact that the truth of a characterization of an utterance in a natural language as true is often a function of when the utterance was produced, by whom and in what context.

In lieu of such information, an instance of schema \( (T) \) could not provide us with a sufficient amount of information to tell, from the explanans containing such an instance of \( (T) \) whether a particular characterization of an utterance is true. The definition of the predicate Tarski has presented us with must be modified in such a way that one can tell whether the natural language sentence
structurally described on the left of an instance of (T) may be
correctly characterized as true under the conditions specified on
the right hand side.

Demonstratives present the most dramatic illustration of
the need for such modification. Consider, for example, such a sentence
as, 'I am wise'; plugging it into an unmodified instance of (T)
yields, "'I am wise' is T if and only if I am wise". Such an
instance may be assented to only under the unlikely assumption that
that speaker of the structurally described sentence on the left is
the speaker considering whether to assent to the instance of (T).
That is, if it is John Doe who has claimed to be wise in uttering
the sentence structurally described on the left, the fact that I
am wise just has noting to do with the truth value of the sentence
structurally described on the left. Indeed, unless the reference of
the demonstrative in any case can be fixed there can be no telling
whether the corresponding instance of (T) is true or not.

Not surprisingly then, Davidson admits "...these complaints
can be met...though only by a fairly far reaching revision of the
theory of truth." (TM, p. 464) What is required to adopt a Tarski
style theory of truth-in-L to natural language is a revision in our
notion of truth, and a consequent modification of our test of such
a theory, schema (T). Schema (T) "...must be revised to make truth
sensitive to context." (SNL, p. 180) and Davidson proposes we do
so by "...making truth a relation that holds between a sentence
a speaker, and a time." (SNL, p. 180)
The theory of meaning undergoes a systematic but not puzzling change: corresponding to each expression with a demonstrative element there must in the theory be a phrase that relates the truth conditions of sentences in which the expression occurs to changing times and speakers. Thus, the theory will entail sentences like the following:

'I am tired' is true as (potentially) spoken by p at t if and only if p is tired at t.

'That book was stolen' is true as (potentially) spoken by p at t if and only if the book demonstrated by p at t is stolen prior to t. (TM, p. 464)

But this is not the end of the changes we shall have to make in schema (T). There is at least one more, and it has to do with the problems presented by ambiguous terms. The problem is again that no matter how we fiddle with the theory's axiomatic structure to accommodate ambiguity, schema (T) seems to need further modification just to enable a speaker to tell whether the truth conditions given on the right are the truth conditions of the sentence structurally described on the left spoken by whomsoever, whenssoever. For example,

Bar Hillel points out that in most contexts someone who knows English will have no trouble resolving the ambiguity in an utterance of "The box was in the pen." Bar Hillel maintains that machine translation can never be perfected to handle such cases (assuming that the language into which we are translating does not have a word that reflects the ambiguity of 'pen'). (SNL, p. 182)

Davidson suggests a theory might handle the troublesome sentence along these lines:

"The box was in the pen" is true for an English speaker x at time t if and only if either the box was in the playpen before t and the circumstances surrounding x at t meet condition c or the box was in the writing pen before t and the circumstances surrounding x at t meet condition c'. (SNL, p. 182)

So if someone should wish to have it explained to them why the instance of the utterance type, "The box was in the pen" uttered by English speaker x at time t is correctly characterized as true, the
answer may be supplied by citing an instance of (T) as above, and noting that, e.g., the box was in the playpen before t and the circumstances surrounding x at t meet condition c. The alternation on the right hand side allows for the use of a number of alternative statements of antecedent conditions, any one of which, together with the instance of (T) in question, may be used to deduce (retrodict) the characterization of the utterance object in question as true.

This is the last revision of schema (T) of which I am aware that Davidson suggests. To discuss whether or not further modifications are necessary would only belabor the point I have been trying to make. Davidson does believe that a theory of language is (or ought to be) composed of a set of statements representative of some state of mind or body of a person who can speak and understand a language. The test of any such set of statements rests partially in their capacity to produce a potential infinity of linguistic generalizations in the form of schema (T), however modified. The further test of such a theory rests in a speaker's agreement that a suitably modified instance of (T) may be used to explain the truth of a characterization of some particular utterance object as true.

Chomsky

This section will be rather brief. The reason, as we shall see, is that I find a Chomskyan theory of language seriously flawed just where it should be the most sound - in its testing procedure. We have earlier discussed, albeit briefly, the current debate over Chomsky's innateness hypothesis. We shall not here be concerned with this question. Whether or not it makes sense to suppose that there is a theory of language, over and above the theories of
particular languages, which "...deals with the common structure of natural languages..." and leads to specific hypotheses about the innate principles underlying language acquisition, will not be at issue here. Suffice it to say that so far as particular languages are concerned, "...the grammar of the language generates a set of pairs (s, l) where s is the phonetic representation of a certain signal and l is the semantic interpretation assigned to this signal by the rules of the language. To discover this grammar is the primary goal of the linguistic investigation of a particular language." (LM, p. 116) It is supposed by Chomsky that a person who can speak and understand a language "...has in some way internalized the system of rules that determine both the phonetic shape of the sentence and its intrinsic semantic content..." (LM, p. 115)

The grammar of a language must be capable of generating "...an infinite set of 'structural descriptions', each structural description being an abstract object of some sort that determines a particular sound, a particular meaning, and whatever formal properties and configurations serve to mediate the relation between sound and meaning." (LM, p. 104) In more detail, this structural description consists of a semantic component, and a phonological component related by the syntactic component.

The syntactic component defines a certain (infinite) class of abstract objects (D, S), where D is a deep structure and S a surface structure. The deep structure contains all information relevant to phonetic interpretation. The semantic and phonological components are purely interpretive. The former assigns semantic interpretations to deep structures; the latter assigns phonetic interpretations to surface structures. Thus the grammar as a whole relates semantic and phonetic interpretations the associations being mediated by the rules of the syntactic component that define paired deep and surface structures. (LM, p. 125)
Clearly, Chomsky shares with Davidson a fascination with (or perhaps appreciation of) the fact that a person who can speak and understand a language seems capable of producing and understanding a potentially infinite number of meaningful sentences given only finite resources.

Having mastered a language, one is able to understand an indefinite number of expressions that are new to one's experience; that bear no simple physical resemblance and are in no simple way analogous to the expressions that constitute one's linguistic experience; and one is able, with greater or less facility, to produce such expressions on an appropriate occasion, despite their novelty and independently of detectable stimulus configurations, and to be understood by others who share this still mysterious ability. (LM, p. 100)

However Chomsky, as aware as he obviously is of the importance of providing a way to test empirically the results of his grammar, seems to have lost sight of the fact that a theory/grammar of a language is to provide explanations of particular linguistic characterizations of utterance tokens by means of particular linguistic generalizations recording the type of conditions under which one (of an infinite number of possible) utterance type(s), tokens of which may be correctly characterized in the way in question. Davidson (for reasons I have mentioned in the preceding section) has decided that the basic characterization that must be accounted for is the characterization of an utterance token as true. Instances of schema (T) record the type of conditions under which an instance of a particular utterance type may be characterized as true, and instances of (T) are deducable from the statements composing the theory of language as Davidson envisages it. The test of such a theory rests in a speaker's decision whether an instance of (T) may be used to explain why a particular utterance may be characterized as true.
The question I wish to raise is just how a structural description of an utterance type may be used by a speaker to determine the truth of some particular linguistic characterization. It is not at all clear just what characterization is at issue. In order to predict or retrodict the truth of some characterization of a particular utterance token, it seems clear enough that some mention must be made of that characterization in the generalization used to predict or retrodict it. But a Chomskyan style structural description seems to make use of no characterization at all.

Of course to the extent that a computer may be set up to model the workings of a brain, a description of the computer set up provides us with no description of the brain. The computer provides us with certain outputs given certain inputs, but it does so entirely in computerese, so to speak. We must translate physical stimuli into computerese and then translate computerese into observable responses, in order to decide if the computer's set up does indeed model the workings of a brain.

A Chomsky style grammar generates particular structural descriptions of utterance types. But we must translate those structural descriptions into descriptions of particular utterance tokens in a language in order to test Chomsky's grammar. But we are not after translation here, but explanation. We want to know why some particular utterance token means what it does, it is no help to be told that that utterance token may be described in thus and such a Chomskyan vocabulary. There are no doubt innumerable ways in which some utterance token may be described, and if some one of these descriptions is supposed to be a complete "...account of its grammatical (i.e., semantic, syntactic
and phonological) properties." (LM, p. 348) one seems well within one's rights to ask just how such a description does account for, i.e., explain, those properties - one does not even know just what those properties are unless one understands the description in the same way one understands the utterance in question. But then, how has the description helped?

One can still ask the same questions about the description - replacing one language with another just does not help explain the properties exhibited by instances of either language. Whatever advantage a Chomskyan style structural description has over our ordinary ways of describing or characterizing utterance tokens must be dependent on the same intimations of structure, intuitions of meaning and nuance, that led us to characterize the utterance token in our ordinary terms in the first place.

As Davidson notes concerning his own theoretic recommendations as opposed to those of Chomsky:

...the evidence to which we are appealing is of much the same sort as Chomsky uses: mainly questions of the loss or preservation of truth value under transformations. Such considerations will no doubt continue to guide the constructive analytic labors of linguists as they long have those of philosophers. The beauty of a theory of the sort we have been discussing is that these intimations of structure, however useful or essential they may be to the discovery of a suitable theory, need play no direct role in testing the final product. (SNL, p. 188)

Searle

As I mentioned at the end of chapter two, a major difference between theorists like Searle and theorists like Davidson, lies in their construal of the kind of phenomena characterized in characterizing an utterance as, e.g., a promise or ture. This difference is reflected
in the apparent ambiguity involved in our use of the term, 'utterance'.
It is not clear whether in speaking about an utterance, we are speaking about the act of uttering, or about the result of such an act. It is Searle's opinion, contrary to Davidson's that the

...unit of linguistic communication is not, as has generally supposed, the symbol, word, or sentence, but rather the production or issuance of the symbol or word or sentence in the performance of the speech act.

Construing the kind of phenomena characterized in linguistic characterizations as an utterance act rather than an utterance object, will obviously occasion some changes in our attitude toward the form which an explanation of the truth of such a characterization must take. Briefly put, Searle would have us believe that "...a theory of language is part of a theory of action..." (SA, p. 16) If "...speaking a language is performing speech acts..." (SA, p. 16) then what stands in need of explanation is a certain sort of behavior. A theory of language ought to explain how it is possible for a person to know how to do something, namely speak his language. And, "The agent's knowing how to do something may be only adequately explicable on the hypothesis that he knows (has acquired, internalized, learned) a rule to the effect that such and such, even though in an important sense he may not know that he knows the rule or that he does what he does in part because of the rule." (SA, p. 42)

In so far as speaking a language just is a form of behavior, and in so far as certain forms of behavior (the possibility of certain kinds of behavior) may be explained only on the assumption that the agent in so behaving is following a rule to the effect that such and such, Searle hypothesizes the central thesis of his book: that "...speaking
a language is engaging in a (highly complex) rule-governed form of behavior. To learn and master a language is (inter alia) to learn and to have mastered these rules." (SA, p. 12) Stated a bit more clearly:

The form this hypothesis will take is that speaking a language is performing speech acts, acts such as making statements, giving commands, asking questions, making promises, and so on; and more abstractly, acts such as referring and predicating; and, secondly, that these acts are in general made possible by and are performed in accordance with certain rules for the use of linguistic elements. (SA, p. 16)

The test of this hypothesis will come in actually trying to state the rules for the performance of some particular speech act. "If we are unable to give any satisfactory rule formulations, our failure could be construed as partially disconfirming evidence against the hypothesis." (SA, p. 37)

Where Davidson seems to believe that the primary linguistic characterization the truth of which a theory of language must finally account for, is one in which a particular utterance is characterized as true, Searle seems to think that an adequate theory must explain the truth of a host of different characterizations. It must explain the truth of characterizations of particular utterances as a promise, a statement, a question, a command and so on. Searle does not believe that these two approaches (his and Davidson's) are entirely incompatible; "...it is important to realize that these two approaches, construed not as theories but as approaches to investigation, are complementary and not competing." (SA, p. 18)

Indeed, Searle goes so far as to suppose the information both approaches can supply is necessary for a complete philosophy of language, i.e., in the terms in which I have been putting it, information from both sources is necessary for a complete answer
to the question, How is it possible for a speaker to speak and understand his language?

A typical question in the second approach is, "How do the meanings of the elements of a sentence determine the meaning of the whole sentence?" A typical question in the first approach is, "What are the different kinds of speech acts speakers perform when they utter expressions?" Answers to both questions are necessary to a complete philosophy of language, and more importantly, the two questions are necessarily related. (SA, p. 19)

It will be instructive to see why Searle claims that "...the study of the meanings of sentences and the study of speech acts are not two independent studies but one study from two different points of view." (SA, p. 18) Searle takes it as "...an analytic truth about language that whatever can be meant can be said." (SA, p. 17)

For just as it is part of our notion of the meaning of a sentence that a literal utterance of that sentence with that meaning in a certain context would be the performance of a particular speech act, so it is part of our notion of a speech act that there is a possible sentence (or sentences) the utterance of which in a certain context would in virtue of its (or their) meaning constitute a performance of that speech act. (SA, p. 18)

Searle calls this analytic truth of his, the Principle of Expressibility. He gives us a more perspicuous statement of the Principle in the following passage.

We might express this principle be saying that for any meaning X and any speaker S whenever S means (intends to convey, wishes to communicate in an utterance, etc.) X then it is possible that there is some expression E such that E is an exact expression of or formulation of X. (SA, p. 20)

For our purposes the most important consequence of the Principle is that "...it enables us to equate rules for performing speech acts with rules for uttering certain linguistic elements, since for any possible speech act there is a possible linguistic element the meaning of which (given the context of the utterance) is sufficient to determine that its literal utterance is a performance of precisely that speech act." (SA, p. 21)
Why does Searle think that "...a study of the meaning of sentences is not in principle distinct from a study of speech acts..."?

(SA, p. 18) I have already noted just what Davidson thinks a study of the meaning of sentences must be like in order to be an adequate study, i.e., a search for the rules governing the manipulation of certain theoretic devices which are such that following those rules will result in the production of a proof of the statement of the truth conditions for an arbitrarily chosen sentence (an instance of schema (T)). Now we need to get clear on just what Searle takes a study of speech acts to be.

Searle claims (roughly following Austin's breakdown of the speech act in How To Do Things With Words) that in performing a speech act one is characteristically (though not always) performing at least three subsidiary acts:

(a) Uttering words (morphemes, sentences) - performing utterance acts.
(b) Refering and predicating - performing propositional acts.
(c) Stating, questioning, commanding, promising, etc. - performing illocutionary acts.

The point of abstracting each of these kinds is that the 'identity criteria' are different in each case....the same propositional acts can be common to different illocutionary acts, and it is obvious that one can perform an utterance act without performing a propositional or illocutionary act at all. (One can utter words without saying anything). (SA, p. 24)

Utterance acts consist simply in uttering strings of words. Illocutionary and propositional acts consist characteristically in uttering words in sentences in certain contexts, under certain conditions and with certain intentions... (SA, p. 25)

Now the identity criteria for utterance acts is apparently not problematic. At least Searle devotes almost no time to discussing them. Propositional and illocutionary acts are a different story however. As a start Searle offers this:
Correlative with the notion of propositional acts and illocutionary acts, respectively, are certain kinds of expressions uttered in their performance: the characteristic grammatical form of the illocutionary act is the complete sentence (it can be a one word sentence); and the characteristic grammatical form of the propositional act are parts of sentences: grammatical predicates for the act of predication and proper names, pronouns, and certain other sorts of noun phrases for reference. (SA, p. 25)

To keep our terminology clear we must note that Searle has introduced an alternative way to refer to any speech act, as an illocutionary act. In the first place he claims that "...utterance acts stand to propositional and illocutionary acts in the way in which, e.g., making an 'X' on a ballot paper stands to voting." (SA, p. 24) And in the second place he says that "...propositional acts cannot occur alone; that is, one cannot just refer and predicate without making an assertion or asking a question or performing some other illocutionary act."

(SA, p. 25)

Now having semantically divided the speech act (illocutionary act) into these three constitutive acts (and having apparently dismissed the utterance act as unproblematic) he goes on to claim that:

We can distinguish two (not necessarily separate) elements in the syntactic structure of the sentence, which we might call the propositional indicator and the illocutionary force indicator. The illocutionary force indicator shows how the proposition is to be taken, or to put it another way, what illocutionary force the utterance is to have; that is, what illocutionary act the speaker is performing in the utterance of the sentence. Illocutionary force indicating devices in English include at least: word order, stress, intonation contour, punctuation, the mood of the verb, and the so called performative verbs. (SA, p. 30)

We are now almost in a position to compare Davidson's approach with Searle's. One thing more needs mentioning. Searle supposes that a speaker's ability to speak and understand his language may be
explained by construing speech as a form of behavior and then citing rules such that following them just is behaving in the way we may characterize as engaging in meaningful speech. The rules Searle is searching for are rules for the use, not only of the illocutionary force indicating device, but also for the, as it were, predicating device and refering device.

...we can separate our analysis of the proposition from our analysis of kinds of illocutionary acts. There are rules for expressing propositions, rules for such things as reference and predication, but I think that those rules can be discussed independently of the rules for illocutionary force indicating... (SA, p. 31)

Davidson, as I mentioned, supposes that giving the meaning of a sentence requires saying or showing how the meanings of the parts of the sentence contribute to the meaning of the whole. Searle claims that giving the speaker's meaning in uttering a sentence requires citing the rules following which counts as the performance of a certain speech act. Searle's rules are rules for the use of illocutionary force indicating devices, predicating devices and refering devices.

In Davidson's terms, if we include the illocutionary force indicating devices as part of the complete sentence (which Davidson would be loath to do) then giving the meaning of the complete sentence must involve (or should involve) saying or showing how the illocutionary force indicating device as well as the predicating and refering devices contribute to the meaning of the complete sentence. In Searle's terms we should say instead that giving the speaker's meaning in uttering the complete sentence must involve saying or showing how the illocutionary force indicating devices as well as the predicating and refering devices may be used such
that that use counts as the performance of a particular speech act.

Davidson's explanation has the form of producing a set of rules such that following them will result in the construction of a Tarski style proof of a statement of the truth conditions of the sentence in question. Searle's explanation has the form of producing a set of rules such that following them will bring about the conditions necessary and sufficient for the performance of the speech act performed in uttering the sentence in question.

In summary we may say the following about Searle's explanatory program. A person who can speak and understand a language has internalized or tacit knowledge of various sets of rules, one set each for each of the various ways in which utterance acts may be characterized. The statement of these various rule sets compose a theory of language in just the same way the statements recursively characterizing the predicate 'is T' compose a theory of language for Davidson. The test of Davidson's thesis rests in a speaker's ability to tell from the information provided by an instance of (T) whether an instance of the utterance type described on the left of an instance of (T) is true or not under the conditions described on the right. The test of Searles's thesis rests in a speaker's ability to tell from the information provided by the statement of some rule set whether behavior in accordance with those rules may be correctly characterized as an instance of the type of speech act in question.

Grice

Grice, like Searle, takes the primary subject matter of a theory of language to be particular utterance acts, rather than particular utterance objects. He is concerned to explain the truth of our
characterizations of what a speaker did by uttering something on an occasion. He is not, however, primarily concerned to explain as large a range of linguistic characterizations as Searle seems to be. He restricts his attention, for the most part, to our characterizations of an utterance act as meaningful (as meaning something), and as meaning that p.

He wants to discover a general schema for a linguistic generalization that may be used to explain (to retrodict) the truth of linguistic characterizations to the effect that

a) U meant something by uttering x on an occasion, and

b) U meant that p by uttering x on that occasion.

This end requires that he come up with a type of condition, a specification of which is both necessary and sufficient for the truth of (a) and (b) above. Grice proposes that a specification of a speaker's intentions upon uttering something will, if carefully done, constitute a specification of such a type of condition. If a speaker may be said to have the proper (set of) intentions(s) then his act of uttering x may be characterized as meaningful; and a specification of the effect or response he intends to induce in his audience will be a specification of what the speaker means by uttering x.

A simplified version of the schema for the type of linguistic generalization Grice thinks will suffice to explain the truth of characterizations like (a) and (b) is this:

(M) "U meant something by uttering x" is true if and only if for some audience, A, U uttered x intending

1) A to produce a particular response r,

2) A to think (recognize) that U intends (1), and

3) A to fulfill (1) on the basis of his fulfillment of (2).
A specification of the particular response, $r$, will be a specification of what $U$ meant by uttering $x$.

It seems clear enough that Grice intends the schema $(M)$, suitablely filled out of course, to be used as one of the two premisses in an explanans paralleling the sort of scientific explanation sketched in chapter one. In abbreviated form, a Gricean scientific explanation would look like this:

"$U$ meant something" is true if and only if $U$ intends (1), (2) and (3).

$U$ does intend (1), (2) and (3).

"$U$ meant something" is true.

The explanandum is a particular characterization of what a speaker did on an occasion by uttering something as meaning something (or as meaning that $p$). The second premiss is a characterization of the speaker as having or exhibiting in his behavior, the requisite set of intentions mentioned in the first premiss, $(M)$.

Hence, the explanatory adequacy of a linguistic generalization of the form of $(M)$ depends, not only upon our ability to tell whether the explanandum is true, but also upon our ability to recognize what a speaker did on an occasion as the expression of the type of intention mentioned in $(M)$. Consequently, if we are to prefer a Gricean schema for linguistic generalization over those proposed for example by Searle and Davidson, prima facia, the notions of recognition and intention must be supposed clearer, less liable to be misunderstood and misapplied on an occasion, then those of truth or rule. On the face of it this requirement just does not seem to be satisfied. Grice, however, does not seem to be too concerned with this point.
Now some question may be raised about my use, fairly free, of such words as 'intention' and 'recognition'. I must disclaim any intention of peopling all our talking life with armies of complicated psychological occurrences. I do not hope to solve any philosophical puzzles about intending, but I do want briefly to argue that no special difficulties are raised by my use of the word 'intention' in connection with meaning. ... Surely to show that the criteria for judging linguistic intentions are very like the criteria for judging nonlinguistic intentions is to show that linguistic intentions are very like nonlinguistic intentions.

Grice's point here is not so much that the notion of intention is a more perspicuous one than that of, e.g., truth, but rather that the notion of intention has a wide and important range of uses apart from its use in connection with meaning. Sooner or later, we are going to have to come up with a theory of intention adequate to explain our characterization of behavior, whether linguistic or not, as intentional. So if we can show that our characterizations of linguistic phenomena as, e.g., meaningful, may be explained in terms of generalizations involving the notion of intention then, we will have shown it unnecessary to construct both a theory of language (or of meaning) and a theory of intention. The theory of intention (or of psychological characterizations in general) will provide the means for explaining the truth of the second premiss in the explanans explaining the truth of the type of linguistic characterization with which Grice is primarily concerned. No special theory of language will be necessary beyond the provision of generalizations of the form (M).

Grice's program has met with a good deal of resistance, principally on two fronts. It has been argued, not surprisingly by Chomsky and Davidson, that Grice's approach must fail to explain the truth of our characterizations of utterance objects as, e.g., meaningful. It is their contention that one just cannot explain the truth of our
characterizations of utterance objects in terms of the truth of

our characterizations of what a speaker did by uttering something on

an occasion. Davidson has this to say.

There can be no objection to detailing the complicated and

important relations between what a speaker's words mean and

his non-linguistic intentions and beliefs....My claim is

only that making detailed sense of a person's intentions and

beliefs cannot be independent of making sense of his

utterances.

Chomsky says this.

Though considerations of intended effects avoids some problems

it will at best provide an analysis of successful communication,

but not of meaning or of the use of language, which need not

involve communication, or even the attempt to communicate. If

I use language to express or clarify my thoughts, with the intent

to deceive, to avoid an embarrassing silence, or in a dozen other

g ways, my words have a strict meaning and I can very well mean

what I say, but the fullest understanding of what I intend my

audience (if any) to believe or do might give little or no

indication of the meaning of my discourse.

Other theorists have criticized Grice on the ground that a

specification of intention is either not sufficient or not necessary

for the truth of a characterization of what a speaker did on an

occasion by uttering something as meaningful or as meaning that p.

(In replying to these criticisms in his article, "Utterer's Meaning

and Intention", Grice cites Stampe, Strawson and Shiffer as arguing

for insufficiency and he considers himself a number of cases

designed to show the analysis is not necessary.)

Broadly speaking, the arguments for insufficiency turn on

our ability to tell that what a speaker did on an occasion by

uttering something is not correctly characterized as meaningful.

The arguments for non-necessity turn on our ability to tell that

a specification of what the speaker meant on an occasion by uttering

something is not the same as a specification of the response or

effect he intended (however complex the intention may be) - such
arguments do not show that what the speaker did may not be characterized as meaningful, indeed they presume that it may; just as the arguments for insufficiency presume that there is no difficulty in telling that a speaker's behavior is indeed an expression of some (however complex) intention.

Now I do not intend to defend Grice against critics like Davidson and Chomsky (nor do I intend to defend their attacks upon him). My overriding purpose in this essay is not to defend one or another of the various scientific theories of language. My purpose is to present considerations which go some way towards showing that the scientific approach to explanation, per se, is mistakenly applied to our characterizations of linguistic phenomena.

I have devoted these few words to Grice because he does represent a serious scientific alternative to such approaches as those taken by Davidson and Searle, for example. And because I believe Grice has recognized, perhaps without realizing it, what I take to be a major flaw in any scientific attempt at explaining linguistic characterizations. One which I shall use in chapter five to argue for the overthrow of the explanatory strategies recommended by Davidson on the one hand, and Searle on the other. I am picking on these two theorists rather than Chomsky and Grice for a number of reasons. I am most familiar with their work. The two of them seem (to me) to most clearly represent the two ends of the spectrum of scientific explanation in the philosophy of language. And because I do think their respective views are the most viable, given, of course, the correctness of the pre-theoretic prejudice that the attribution of ability is descriptive of some state of a person who
has that ability, a prejudice I intend to strike down in the next chapter.

Now just what fly in the scientific ointment has Grice noticed? The difficulty becomes apparent in considering Grice's defense against those critics who argue for the insufficiency and/or the non-necessity of his analysis. As we have seen, Grice is committed to the view that a speaker, U, means something by uttering something if and only if U intends a certain complex effect, E₁, to be induced in an audience, A. Criticism has taken the form of counterexamples designed to show either a) that while it is prima facia correct to characterize what U did as mean something, E₁ was not, in that case, intended by U, but rather E₂ was; or b) that E₁ was intended but it is prima facia wrong to characterize what U did as mean something on that occasion.

Grice's strategy for meeting these objections involves either a') modifying the statement of requisite intentions to include E₂; or b') including in the statement of intentions some factor which will disqualify the case in question as one in which U meant something by uttering what he did. Counterexamples of the sort (b) are designed to establish insufficiency, and of the sort (a), non-necessity. Now I think a moments reflection will show that only lack of ingenuity stands in the way of meeting counterexamples of the sort (b) in the way that Grice proposes. For a full and detailed statement of the result of adding disqualifying conditions to the requisite intentions one can take a look at page 164 of "Utterer's Meaning and Intention". I shall not reproduce Grice's modifications here for my interest lies in his reply to those examples designed to establish non-necessity. Let us, for convenience sake, abbreviate a statement of
the intentions resulting from his attempts to meet arguments for
insufficiency with the phrase, 'M-intends'. So the schema (M) will
now read:

\[(M)\text{ U meant something by uttering x on an occasion if and only if for some audience, A U uttered x M-intending to induce in A effect or response r.}\]

As earlier mentioned, arguments for non-necessity turn on
cases in which a specification of what U meant by uttering x is
just not the same as a specification of the response, r, intended
by U on that occasion. For example, in the context of taking an
oral exam, U may have meant by uttering, "The battle of Waterloo was
fought in 1815", that the battle of Waterloo was fought in 1815, but
he could not have M-intended his examiner to think that the battle of
Waterloo was fought in 1815. The examiner would not be the
examiner if he did not already know that and U must be aware that he
does. The M-intended effect in this case would probably be that the
examiner think that U thinks that the battle of Waterloo was fought
in 1815, and it does not seem attractive to say that U meant by
uttering, "The battle of Waterloo was fought in 1815", that he
thinks that the battle of Waterloo was fought in 1815. (cf. p. 169 of UM&I)

Grice concludes from such examples as the above that "...we
need to be able to say on occasion that U meant that so and so
without committing ourselves to the proposition that U M-intended
to produce a belief that so and so." (UM&I, p. 166) Grice wants to
meet the counterexamples for non-necessity by introducing an M-intended
effect that will in some (perhaps most) cases be different from a
specification of what U meant, but which will nevertheless allow one
to pass, if necessary, from the M-intended effect to the effect a
specification of which will be a specification of what U meant on
the occasion in question.

The following quote summarizes his reply here.

Since, when U does intend, by uttering x, to promote in
A the belief that p, it is standardly requisite that A
should (and should be intended to) think that U thinks that p
(otherwise A will not think that p), why not make the direct
intended effect not that A should think that p, but that S
should think that U thinks that p? In many but not all
cases, U will intend A to pass, from thinking that U thinks
that p, to thinking that p himself ("informing" cases).
But such an effect is to be thought of as indirect (even
if often of prime interest). (UM&I, p. 171)

Clearly, what is problematic here is the notion of passing, i.e.,
of an audience being expected to pass from a realization of the
M-intended effect to the effect, a specification of which will
be a specification of what the speaker meant by uttering x. Only
if the audience does pass from the M-intended effect to that
effect a specification of which will be a specification of what the
speaker meant on the occasion in question will that audience be
able to tell whether a Gricean explanation of the truth of a character-
ization of a speaker as meaning that p by uttering x is adequate or
satisfying. That is, the information supplied by the explanans of a
Gricean explanation (composed of an instance of schema (M) and a
characterization of the speaker as M-intending some effect) will
enable an audience (testee) to decide upon the truth of the
appropriate explanandum (a characterization of what the speaker did
by uttering x as meaning that p) only if that audience brings to the
explanation information, extra-theoretic information, not provided
by the explanans.
What that information must be will be made clear once it is made clear just what makes it possible for a speaker to expect his audience to pass from thinking that \( U \) thinks that \( p \) to thinking that \( p \) himself. And if \( U \) does not have this expectation then he can hardly be accredited with even an indirect intention that \( A \) think that \( p \).

I think that what makes it possible for \( U \) to have this expectation is, speaking broadly, \( U \)'s belief that he and his audience speak the same language and are both aware that they are. To say of someone that he speaks the same language as you, is at least to say that the two of you can converse, communicate, with some degree of confidence that each will understand the other. So to some extent, the extra-theoretic information which \( U \) expects \( A \) to possess and in virtue of which expectation \( U \) intends \( A \) to pass from thinking that \( U \) thinks that \( p \) to thinking that \( p \) himself, may be codified in a statement of (what we might call) the rules of conversation. That is, the means whereby \( U \) expects \( A \) to pass from thinking that \( U \) thinks that \( p \) to thinking that \( p \) himself, may be codified in the statement of a set of rules or conditions governing conversation.

I think that Grice has made a start at providing such a set of rules in his article "Logic and Conversation". He notes there that,

Our talk exchanges do not normally consist of a succession of disconnected remarks, and would not be rational if they did. They are characteristically, to some degree at least, cooperative efforts: and each participant recognizes in them, to some extent, a common purpose or set of purposes, or at least a mutually accepted direction....We might then formulate a rough general principle which participants will be expected (ceteris paribus) to observe, namely: Make your conversational contribution such as is required, at the stage
at which it occurs, by the accepted purpose or direction of
the talk exchange in which you are engaged. One might label
this the Cooperative Principle.\textsuperscript{10}

So we might suppose Grice to have made a start at explicating
the notion of speaking the same language by supposing him to be
attributing to speakers of the same language mutual knowledge of
the Cooperative Principle (and so, of course, also of the various rules
of conversation, which Grice calls conversational maxims, flowing from
this principle). The point here being that this mutual knowledge or
rather the statement of such knowledge must constitute a statement
of the extra-theoretic information which an audience brings with him
to a talk exchange, a conversation, and which knowledge enables him
to pass, if necessary, from thinking that U thinks that p to thinking
that p himself.

Without involving ourselves in a discussion of the Cooperative
Principle (and its implications) we should realize at this point
that the fly in the ointment of scientific explanation of the
truth of our characterizations of linguistic phenomena which
Grice has noted, but which Davidson and Searle have not, is this.
To the extent that a scientific explanation of the truth of a linguistic
characterization depends for its adequacy upon the fact that the
explanans of such explanation must provide a sufficient amount of
information for a speaker of the language to tell whether the
characterization in question is true, no such explanation can be
counted adequate. The test of any such explanation will fail on the
grounds that the testee must always utilize some extra-theoretic
information in order to decide if the explanandum is true given the
information supplied by the explanans.
Conclusion

The chart on the next page will provide a graphic (if simplified) representation of the similarities I have alleged obtain among Chomsky, Davidson, Searle and Grice and scientific explanation in general. Each level, starting with (IV) and working upwards, is (supposed to be) deducable from the level above it. There may very well be theoretic laws higher than those on level (I), e.g., some general physical theory, but I have not bothered to go beyond (I).

For the purposes of introducing the next two chapters we must note the following two points. Of these four theorists only Grice does not clearly maintain that the statements composing a theory of language (level (II) statements) are representative of some state of mind or body of a person who can speak and understand a language. A theory of language is at least to explain our ability to speak and understand a language. The statements composing such a theory may be supposed to be statements descriptive of some state of mind or body of a speaker such that if such statements are true of a person then that person may be attributed with the ability to speak and understand a language, i.e., then it is true to say that that person can speak and understand a language. (It is not clear that Grice holds such a position because it is not clear just how Grice supposes that our characterizations of a person’s behavior as intentional are to be explained. Indeed it is not in general clear how we are to go about explaining the truth of our use of any psychological predicate. In the concluding chapter of this essay I shall have a few suggestions to make in this connection.)
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In the next chapter I shall argue that this prejudice is mistaken. I shall argue that it is a mistake to think that in attributing any ability to a person we must be describing some state of that person. I shall draw the conclusion as a result of these arguments that a scientific theory of language (one founded on this prejudice) must be incapable of test.

The test of any scientific theory depends (in part) upon the deductive nature of the relation between explanans and explanandum. From the information supplied by the explanans and only from that information we are supposed to be in a position to tell whether the explanandum is true. As this point applies to a scientific theory of language, any such theory, were it practically possible to do so (or even philosophically possible, if one accepts Quine's indeterminacy thesis), should supply one who understands the statements composing such a theory, with sufficient information to tell whether a person possesses the ability to speak and understand a language.

Since it is not now and may never by possible to directly tell whether the statements composing a scientific theory of language are true of a speaker, the test of such a theory must lie in deciding whether it supplies the requisite apparatus to deduce the truth of particular linguistic characterizations of linguistic phenomena (whether utterance objects or acts). A person who can speak and understand a language can tell whether particular utterances are meaningful, a promise, a question, and so on. So a theory of language must supply a sufficient amount of information to tell whether linguistic characterizations of utterances are in particular cases, true.

Now if the attribution of an ability is not descriptive of an agent then the information on the basis of which a speaker attributes
an ability cannot be wholly specified in terms of a set of statements descriptive of some state of the agent in question. Consequently, if the test of a scientific theory lies in our ability to tell whether we would characterize particular utterances under the conditions specified by a linguistic generalization, no such theory may be confirmed without the testee's relying on some extra-theoretic information.

In chapter five I shall argue that the explanatory strategies of both Davidson and Searle are subject to the inadequacy outlined above. Neither of their respective explanatory proposals can avoid the fact that the explanans of a prospective scientific explanation of the truth of some linguistic characterization can provide a sufficient amount of information for a testee to tell whether the explanandum is true. In every case the testee must supply some information of his own, information which must be an integral part of his ability to speak and understand a language.
FOOTNOTES FOR CHAPTER III


6. A far more elaborate version may be found on page 172 of "Utterer's Meaning and Intention", Philosophical Review, 78, 1969. Hereafter referred to as UM&I.

7. H. Grice, "Meaning", in Steinberg and Jakobovits, Semantics, Cambridge University, 1974, p. 59. Hereafter referred to as M.

8. D. Davidson, "Meaning and Belief", Synthese, vol. 18, p. 32. Hereafter referred to as MB.


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At the conclusion of the last chapter, I announced my intention to argue that the sort of explanatory strategy which I have called scientific should not be followed in attempting to explain our ability to speak and understand a language. I shall develop my argument in two stages. In the first I shall argue that the attribution of the ability to speak and understand a language is not correctly characterized as a description of some state of the agent to whom the ability is attributed. I hope I have made it clear by now that any application of the scientific to the phenomena of speech (whether by Chomsky, Davidson, Searle or Grice) depends for any hope of success upon the presumption that ability attributions are descriptive. Davidson, for example, has told us that a theory of language is supposed to "...describe the skill and ability of a person who has learned to speak and understand a language."\(^1\) And Chomsky has told us that "...a grammar of a language purports to be a description of the ideal speaker-hearer's intrinsic competence..."\(^2\) And Searle has told us that the philosophy of language is concerned to "...pose and answer the questions which examine..." how it is possible for a speaker to speak and understand a language, e.g.,
How is it possible that when a speaker stands before a hearer and emits an acoustic blast such remarkable things occur as: the speaker means something; the sounds he emits mean something; the hearer understands what is meant; the speaker makes a statement, asks a question, or gives an order? 3

We should be equally as aware as these theorists apparently are that the principle aim or end of a scientific explanation of the multifarious range of phenomena that constitute speaking and understanding a particular language is confirmation (at least partial) of some particular hypothesis providing us with a description of the state of affairs which must obtain in the world for it to be true to say of someone that he can speak and understand his language. This is to say that we may take it to be the principle aim of a scientific explanation of our ability to speak and understand a language, to provide us (at least) with a (set of) statements of the form, 'S is F', true if and only if a statement of the form, 'S can speak and understand his language', is true.

This first stage is obviously directed against any attempt to concoct a scientific theory of language (or of a particular language). The second stage of my argument is a bit more specific. It will be directed against on the one hand a Davidsonian, and on the other, a Searlean implementation of the scientific strategy. I shall argue (in the next chapter) that neither of these two ways of carrying out the scientific strategy can be confirmed without violating the information condition on the adequacy of any explanation. That is, I shall argue that the explanans of a linguistic explanation where the explanandum is some linguistic characterization on an occasion does not provide a testee with sufficient information to determine the correctness of the characterization. In every case in which a
testee agrees that the characterization is correct he must do so on the basis of information not incorporated in the relevant explanans.

These two stages are not unrelated, of course, though I think that neither depends upon the conclusions of the other. The prejudice that the scientific is the only way to go about explaining anything is so well entrenched that I can easily imagine someone replying to my first stage that if confirmation of a scientific theory of language (whether Davidsonian or Searlean) is at least possible then, my arguments to the contrary notwithstanding, the attribution of an ability (at least the ability to speak and understand a language) must be descriptive. My second stage is obviously designed to forestall this sort of reply.

As I have noted, scientific theories of language are founded on the presumption that it is possible to confirm an hypothesis alleging that some (set of) statement(s) of the form, 'S is F', are true if and only if a statement of the form, 'S can speak and understand his language', is true. The relevant 'S is F' statements are descriptive of some aspect of some state of the world (in particular the physiological or psychological state of the agent indicated by 'S').

However, as I noted in chapter two, no scientific theorist should (at any rate) suppose that one can directly confirm any such hypothesis. Neither inspection nor introspection will suffice. Any attempt to record the result of a direct inspection of a speaker's physiological state would (good Quinean's that we are) be subject to a charge (at least) of question begging. And any attempt by a speaker to report the result of an introspection, as it were, of his own mental state would not only be equally question begging, but also probably impossible,
since the state in question is no doubt well below the level of consciousness.

Confirmation must be indirect; and the way is open for such confirmation once we take note of the fact that we paradigmatically (in the most obvious cases) attribute the ability to speak and understand a (particular) language on the basis of an agent's demonstrated ability to recognize utterances (act or object) as true or a promise or a warning or as meaning or referring to something, etc. And so, if we can explain the correctness on an occasion of one or some of these linguistic characterizations in terms of linguistic generalizations and then explain the generalizations in terms of (some of) the statements putatively representative of the state of an agent who can speak and understand a (particular) language, we shall have (at least partially) confirmed the theoretic hypothesis in question. A linguistic generalization will be a statement relating a type of characterization (e.g., 'is true', or 'is a promise') to a set of conditions (or condition types) putatively necessary and sufficient for the correctness of a token of the type of characterization in question. If we can get a speaker to agree that such a generalization does explain the correctness on an occasion of the relevant characterization then we can count that generalization at least partially confirmed. And if we can deduce that generalization from the set of statements putatively descriptive of the state of any agent who can speak and understand the language in question, then we can count the corresponding theory of language (at least partially) confirmed.

In stage two (chapter five) I am going to argue that linguistic generalizations cannot be confirmed scientifically, i.e., I shall argue
that such generalizations (whether of the Davidsonian or Searlean sort) just do not even together with a statement of the relevant conditions at the time of characterization, provide a testee (one who can speak and understand the language in question) with sufficient information to determine whether the characterization is (or would be) correct. In this stage, in this chapter, I am going to argue that utterances of the form, 'S can speak and understand his language', (i.e., utterances of the sort, 'S can V') are not properly characterized as utterances descriptive of some state of the agent in question, that such utterances do not mean the same as (some) utterances of the sort, 'S is F', utterances I take to be paradigmatic descriptive utterances.

In the concluding section of this chapter I shall suggest that such utterances, i.e., 'S can V' utterances, should be viewed as what J. L. Austin has called verdictives. The difference between a description and a verdiction is simply this. Utterances are correctly characterized as descriptive only if there is (in principle at least) a specification of one (or a set of) conditions which is necessary and sufficient for the correctness of the utterance in question. An utterance of the sort, 'The chair is red', is correctly characterized as descriptive just in case there is (in principle) a specification of the state of the world (an alternative way to specify the state of the world) which if the world is in that state at the time of utterance is neccessary and sufficient for the truth of the utterance at that time (for the correctness of the characterization of that utterance (act or object) as true).

This is not to say that the characterizer must actually know how to characterize the relevant state of the world, but only that he
must take it on faith, so to speak, that there is such a specification. Should it turn out that it is not even in principle possible to exhaustively specify a state of the world which is necessary and sufficient for the correctness of an utterance of the sort in question, then the characterizer's faith is misplaced and it is a mistake to characterize that sort of utterance as descriptive.

The salient point as regards descriptions contrasted with verdicts (the one I am interested in, at any rate) is that they are not essentially at least, expressions of an opinion (i.e., expressions of a verdict or judgement). We might say (I shall say) that descriptions are not essentially discretionary. What marks off a description from a verdict is the element of discretion involved in a verdict (or judgement). There just is no set of conditions even in principle a specification of which is necessary and sufficient for the correctness of a verdict. If an agent were knowledgable enough, if he knew enough about the subject matter and took the time to investigate the state of the world sufficiently, then there would be nothing for him to exercise any discretion about in deciding whether, e.g., the chair is red, or more, obviously, the painting is beautiful.

Now Austin lists description as a species of verdict and we may take this in one of two ways. We may suppose that Austin espoused the view that there are no discrptive utterances in the ideal sense, i.e., that all putatively descriptive utterances are really discretionary, though some seem to require less discretion than others, e.g., 'The chair is brown', as opposed to, 'The chair is puce.' Or we may suppose Austin to have held the weaker view that in our everyday, ordinary lives, given the rather limited knowledge we
possess of the state of the world at any given time, we ordinarily do exercise some discretion in describing, but we needn't have. The chair is puce because it appears to us to be a bit purplish, to have a hint of purple in its otherwise brown coloring. Whether there is, for example, some spectoscopical analysis of the chair's color which we could perform (and could appreciate if we could perform it) which would decide for us, which would eliminate the element of discretion involved in characterizing the chair as puce, is beside the point, unnecessary for our ordinary purposes (most of the time, anyway).

Though I must admit to a good deal of sympathy with the first, the more radical, of these two interpretations of Austin's classificatory propensities (even if Austin would not himself have gone so far), it is not my purpose here to argue for it. It will be enough just to assume that there are descriptive utterances, i.e., utterances paradigmatically of the form, 'S is F', which are not discretionary, at least not essentially so. My intent in this chapter is merely to argue that utterances of the sort, 'S can V', are not descriptive, and to suggest that they are best viewed as verdictives.

In order to properly introduce my arguments here I want to take a moment to make it clear (in my own fashion) just how important it is to the success of any scientific attempt to explain our ability to speak and understand a language that we understand just how we do use utterances of the form, 'S can V'. Unlike theories of, e.g., physics, confirmation of a (scientific) theory of a language requires an exercise of the very ability to be explained by the theory. Whether any proposed set of statements putatively representative (at least)
of the ability to speak a language does provide us with acceptable explanations of the correctness of some linguistic characterization on an occasion depends (as I have noted) essentially on whether a speaker of the language accents the explanans in question. And a speaker of the language is an agent who can speak and understand the language.

Unfortunately (for a scientific theorist) not everyone who may be so characterized, i.e., as an agent who can speak and understand the language, may, or should be considered an acceptable testee, as it were. There are, to be sure, some relatively clear and unproblematic cases. I, for example, and anyone reading this essay, I imagine, would be, no doubt, acceptable testees. The difficulty is to say just why we are acceptable, while, for example, new born babies, five year olds, people in comas or who have amnesia, even Frenchmen who have never spoken nor understood a word of English, are not. We must have some pre-theoretic understanding of the meaning of utterances of the sort 'S can speak and understand his language', which allows us to rule out of consideration as acceptable testees, babies, five year olds, and people in comas, while ruling into consideration you and I. The issue is not the rather silly one concerning a theorist's right, as it were, to stipulatively define a familiar term. It is our familiar and ordinary understanding of the use of the term 'can' that the adequacy, indeed the very feasibility of stipulatively defining such a term rests upon.

Prior to any theorizing on the subject we should note that 'can' as it is used in utterances of the sort, 'S can V', where 'S' holds a place for the name or description of a human agent, is used in one
or another of the following three ways (however they may overlap in practice).

1) It may be used to indicate that the agent has (in the opinion of the speaker, at least) the opportunity to V at the time of utterance. We may say that what a speaker means on an occasion by uttering an instance of 'S can V', is that S's V'ing at the time is consistent with some subset of the set of conditions, the failure of anyone of which to obtain, would prevent S from V'ing. Which conditions of them all the speaker has in mind, so to speak, will be just those that are the most salient in the context, the ones most worthy of note.

2) It may be used to indicate that the agent has (in the opinion of the speaker) the skill or expertise required to V. 'S can V' in the skill sense differs from 'S can V' in the opportunity sense in that S can V in the skill sense may be true on the occasion even though S cannot V in the opportunity sense on that occasion. If we count having the skill to V as one of the conditions which must obtain in order for S to have the opportunity to V on an occasion (whether or not it is on that occasion a salient condition) then we may say that S can V in the opportunity sense only if S can V in the skill sense.

3) It may be used to indicate that the agent has the capacity to V, i.e., is in a position to learn to V, to acquire the skill or expertise to V. We may say that S can V in the capacity sense only if 1) S can Y in the skill sense, where Y'ing is an ingredient activity, so to speak, in the activity of V'ing, and 2) S can learn to V. For example, we may say that S can put in the capacity sense only if S
can grip a club and swing it in the skill sense of 'can', and only if S possesses sufficient intelligence to learn to putt. If an agent's intellectual equipment, so to speak, is so impaired that there is no hope of his ever reaching a point at which we may say that he can putt in the skill sense, then it is surely doubtful, if not just false, that he can putt in the capacity sense (even though he can grip and swing a club in the skill sense).

Now obviously babies won't do as acceptable testees for a scientific theory of language. Though we might say that a (normal) baby can speak English, we are clearly using 'can' in the capacity sense. Babies just can't speak English in the skill sense. And though we might say that a person in a coma can speak English in the skill sense, he won't do as an acceptable testee either. He can't speak English in the opportunity sense. The hard question is why the five year old won't do, or, at least, won't do as well as your or I. We may suppose that he can speak English in both the opportunity sense and the skill sense, its just that he is not as skilled as your or I.

The question is whether our use of 'can' (particularly in the skill sense, but derivatively in the opportunity sense as well) may be treated as analogous to the 'can' that is used in, e.g., attributing solubility to sugar. Just as we suppose that sugar can dissolve only if it is possessed of a certain microstructure, should a scientific theorist of language suppose that a person can speak and understand a language only if he is possessed of a certain neuro-physiological structure?
It should be clear that I am principally interested in the use of 'can' to attribute some skill or expertise, rather than any opportunity or capacity. In the first place, confirmation of a scientific theory of a language is to be achieved (if at all) by agents who can speak and understand the language in (at least) the skill sense. And in the second place, it does seem that so long as 'S' in any 'S can V' utterance holds a place for the name or description of a person, baby, five year old, or even amnesiac, the skill sense of 'can' is at least implied. We may say that the correct use of 'can' in either the opportunity sense or the capacity sense implies that the agent in question can V in the skill sense on that occasion. 'S can V' in the opportunity sense on an occasion may be correct only if S can V in the skill sense on that occasion. 'S can V' in the capacity sense may be correct only if S can Y in the skill sense on that occasion (and we might add, if learning to do something is as much of a skill as it seems to be, only if S can learn to V.)

Obviously the intent of this chapter is to argue in effect that attributing the skill to speak and understand a language is not at all like attributing solubility to sugar. If it were then of course, 'S can V' utterances would be, after all, descriptive. They would be unproblematically replaceable with utterances of the sort, 'S is F'. And this is just the point of view that I am challenging. One of the presuppositions behind my even wanting to argue for what I am is (my belief) that people are not like 'things', that our concept of a person, of personhood, is importantly different from our concept of a thing, of thinghood. And one effect of this difference is just that we cannot utilize the same explanatory strategy in attempting to explain the
correctness of our use of one or another of the concepts associated with personhood, e.g., skills of various sorts. If we attempt to explain physical dispositions, such as solubility, and abilities, skills, in the same way, the scientific way, then we shall be seriously misconstruing the nature of our concept of a person, or at least so I believe.

In behalf of my prejudice here, I am going to argue that the view of 'can' which would have us treat it as analogous to the attribution of solubility cannot adequately account for the following fact. Audiences who do understand 'S can V' utterances do come to hold beliefs that we may schematize as 'S is F', and count as descriptive beliefs. And furthermore, more often than not, such beliefs are just those which the speaker intended that audience to come to have. We may say that by producing an utterance of the sort, 'S can V', the speaker meant that S is F. And that on more occasions than not, the speaker's audience will understand just what the speaker meant, i.e., the audience will come to have the appropriate belief.

If we ask just how this is possible and are intent on maintaining the view that utterances of the sort 'S can V' mean (timelessly as Grice would say, or even mean on an occasion) the same as some 'S is F' utterance (no matter how complex the predicate is for which 'F' holds a place), we may expect this sort of reply. The 'S is F' utterance specifying the belief which we may say the speaker intended his audience to come to have specifies one (or a subset of) the conditions necessary and sufficient for V'ing, i.e., for the agent's successful V'ing. That the speaker may be characterized as meaning that this condition (or subset of conditions) and not some other, obtains, is the result of
the salience or noteworthiness of such conditions at the time of utterance. So accounting for an audience's 'S is F' belief on an occasion requires accounting for the salience of the condition (or subset of conditions) noted by the speaker (and expected by him to be noted by his audience.)

With this reply a scientific theorist will have (he thinks) happily married his prejudice that the 'can' of skill is indeed analogous to the 'can' of solubility, with the fact that what speaker's mean by 'S can V' utterances (in the skill sense, at least) may be specified in terms of an 'S is F' utterance where 'F' holds a place for a specification of only a subset of the all the conditions necessary and sufficient for S's V'ing. Now I am going to argue that one cannot satisfactorily account for this fact by concocting a theory of salience to go along with the assumption that 'S can V' utterances (even in the skill sense) are descriptive, i.e., mean the same as 'S is F' utterances, where 'F' holds a place for a specification of all the conditions necessary and sufficient for S's V'ing. In order to accomplish this end I think I have to show two things. First, I shall have to show that it is a mistake to think that 'S can V' utterances mean the same as 'S is F' utterances (however complex a predicate 'F' holds a place for). I shall call this view the analysis view. And secondly I shall have to show that 'can' is not a device whose function it is to enable a speaker to mean something that may be schematized as 'S if F', even though he does not choose to produce the appropriate 'S is F' utterance. I shall call this view the ellipsis view. I shall conclude this chapter with a section elaborating upon the reasons why I think 'S can V' utterances are properly thought of as
verdictive. My suggestion here, that they are verdictive, discretionary, should be taken as an hypothesis, one that is intended to explain the discrepancy in what Wittgenstein would call the logical grammar between the 'can' of solubility (which is descriptive) and the 'can' of skill. This hypothesis of mine, besides accounting for the above mentioned fact concerning what speaker's mean by 'S can V' utterances, will be at least partially confirmed by the success of my arguments in chapter five. If the attribution of the ability to speak a language is not descriptive, then we should expect that our use of such characterizing terms as, 'promise' and 'true' are not descriptive either, for it is on the basis of our use of such terms to characterize the expression of this ability that we attribute this ability. And if linguistic characterizations are not descriptive, then we should not expect a scientific attempt to explain the correctness of such characterizations to be successful. In every case, the explanans just will not be able to convey sufficient information to tell whether the explanandum is correct. And this is to say that such explanations are not testable without violating the information condition.

The Analysis View

In this section and the next I shall draw heavily on the ideas presented in two different articles, "Meaning and Saying", by D. S. Shwayder, and "If's and Can's" by J. L. Austin. Though I do not entirely endorse the non-critical aspects of their articles, as we shall see I shall make extensive use of what I take to be their arguments contra both the analysis view and the ellipsis view of 'S can V' utterances.
Let us begin with a straightforward statement of the problem. Confronted with a speech performance involving an utterance of the 'S can V' sort, an audience will, if he understands such an utterance, come to hold a belief which we may schmatize as, 'S is F', where 'F' holds a place for some specific predicate, however complex, descriptive of some conditions(s) normally considered necessary for S's successful V'ing. And sometimes (perhaps more often then not) such a belief will be just that belief which the speaker intended that audience to come to have. For example, in uttering 'S can shoot', the speaker may mean that S is unguarded and so intend his audience to come to believe that S is unguarded. And, in this case we may suppose, the audience in question does indeed come to believe that S is unguarded upon being confronted with the speaker's utterance, i.e., we shall suppose that the audience understands the speaker in this case. Now the question is: How is this possible?

What justifies an audience in believing that the speaker meant on this occasion that S is unguarded? Those who expouse what I am calling the analysis view will answer this question by locating the warrant for the audience's belief in his knowledge of what the speaker's utterance meant.

There are, I think, three distinct ways to take this view. The first two reflect a (possible) ambiguity in our use of 'mean' (or 'meant'). The third presents (or exhausts) the classical attempts to analyze utterances into other utterances utilizing expressions supposedly clearer then those used in the original.

1) Utterances of the sort 'S can V' mean timelessly (as Grice puts it) the same as utterances of the sort 'S is F' mean timelessly, where 'F' holds a place for some specific
predicate, e.g., 'is unguarded'.

2) Utterances of the sort 'S can V' mean on an occasion just what an utterance of the sort 'S is F' would mean on that occasion, again where 'F' holds a place for some specific predicate.

3) Utterances of the sort 'S can V' mean on an occasion just the same as one of the following three sorts of utterances mean on that occasion,

   a) 'S M V if S is ...'
   b) 'S M V if S is F v G'
   c) 'S M V if S is F & G'

where 'M' in each case holds a place for some verb other than 'can' and in (a) the three dots hold a place for some specific but unspecified condition on S's V'ing, and in (b) and (c), 'F' and 'G' hold places for some specific and specified condition on S's V'ing, however complex.

I shall deal with each of these alternatives in turn. Analysis view (1) above can be quickly eliminated. The following quote from Shwayder should suffice.

I say 'He can shoot' meaning he is open. A man on the bench says 'He can shoot' meaning the coach has given his okay. The two of us mean different things, although both of us say that he can shoot. It seems evident from this that both of us are asserting specific facts, but different specific facts. I assert, in effect, that the player has the chance; and that is all. But, clearly, the sentence, 'He can shoot' does not mean the same as the sentence 'He has the chance to shoot'. (MS, p. 74)

I must note here some possible confusions in Shwayder's terminology; when he speaks of what an utterer says I understand him to be speaking of what the utterance means timelessly. And when he speaks of what an utterer asserts I take him to be speaking of what the utterer means on an occasion.

Analysis view (2) cannot be so quickly disposed of. Insofar as it is distinct from what I have called the ellipsis view, (as I believe a few moment's reflection will show), to deal with it we
must recall the explanatory strategies of both Davidson and Chomsky (and I am picking on these theorists since I take it that view (2) here would be one dear to their hearts). In order to establish view (2), briefly put, we would have to show either that a particular utterance of the form 'S can V' may be generated ala Chomsky from a surface structure of the sort 'S is F', or we must show how to derive ala Davidson an utterance of the sort 'S is F' on the right hand side of an instance of convention (T) having on the left hand side a description picking out an instance of an utterance of the 'S can V' sort.

What we are interested in is the contribution which 'can' makes to the meaning of utterances of the 'S can V' sort. If we are loathe to give up the idea that such utterances mean the same as 'S is F' utterances, then we have only to show how 'S can V' utterances may be generated or derived from 'S is F' utterances. Chomsky might introduce a can-transformation rule applicable very near the surface of those trees generating 'S is F' utterances. Davidson might introduce a non-logical axiom stipulating the logical role played by 'can'.

I have not gone into any great detail in describing either of these two theoretical possibilities because I think that both can be shown to be untenable. We may parse 'S can V' utterances into 'S is F' utterances only if we have at our disposal, as theorists, a finite list of all the conditions on successful V'ing whatever 'V' happens to stand for. Shwayder believes that there can be '...no completed list of condition sorts which can be spotted by saying 'can'...' (MS, p. 75)
Beginning such a list will make it more evident that it could not be completed: opportunity, permission, chance, luck, strength, coordination, smallness of fingers, sharpness of vision, wit, memory, composure, confidence, determination, courage, skill, experience, training, conditioning, state of the atmosphere...availability of equipment...logical possibility. (MS, p. 75)

I am inclined to agree with Shwayder on this point, but I want to suggest a more telling point. Notice that each of the above listed condition-sorts (and however many more one can think of) specify a class of specific predicates. (For example, 'availability of equipment' might include, 'having a basketball (or baseball, or football, or soccer ball, or golf ball)', 'having a hammer (or a saw or a screwdriver)', etc.) And recall that the 'S is F' utterance sort into which 'S can V' utterances are to be parsed (in one way or another are such that the predicate for which 'F' holds a place is to be (must be) a specific predicate. Now whether or not there are an infinite number of conditions sorts, it seems clear enough that there is no definite number of specific predicates falling under one or another of the available condition sorts. So even if we could complete the list of condition sorts we could not complete the list of particular conditions on successful V'ing whatever 'V' happens to hold a place for in 'S can V'. Hence, I take it that there can be no finite stock of predicates upon which theorists like Chomsky and Davidson could draw in order to derive or generate the appropriate 'S is F' utterance in giving the meaning on an occasion of an utterance of the 'S can V' sort.

We have now to consider analysis view (3). Both Shwayder and Austin oppose it, though I take Shwayder's arguments to be a bit more conclusive. The following quote from "If's and Can's" will help
refresh our memory regarding view (3).

There are two quite distinct and incompatible views that may be put forward concerning if's and can's, which are fatally easy to confuse with each other. One view is that wherever we have can or could have as our main verb, an if-clause must always be understood or supplied, if it is not actually present in order to complete the sense of the sentence. The other view is that the meaning of 'can' or 'could have' can be more clearly reproduced by some other verb (notably 'shall' or 'should have') with an if-clause appended to it. The first view is that an if is required to complete a can-sentence, the second view is that an if is required in the analysis of a can-sentence. (IC, p. 214)

Clearly, the first of Austin's fatally easy to confuse views is just what I have called the ellipsis view. We shall get to it in the next section. For the remainder of this section I shall concentrate on the second of the views Austin distinguishes (which I have called analysis view (3)).

Earlier in this section, in introducing the analysis view, (3), I distinguished three alternative utterance sorts into which 'S can V' utterances may be analyzed; in each some alternative verb (other than 'can') was to be used and the appended if-clause was to be completed in one of the following ways.

a) 'S M V if S is ...'
b) 'S M V if S is F v G'
c) 'S M V if S is F & G'

I shall not give Austin's arguments against view (3) since I believe Shwayder's to be stronger. Sustin's procedure in dealing with this view is to consider one at a time, first Moore's proposal that utterances of the sort 'S can V' mean the same as utterances of the sort 'S shall V if ...', and then Nowell-Smith's proposal that such utterances mean the same as utterances of the sort, 'S will V if ...'.
Austin musters a battery of syntactic and semantic information and concludes that both Moore and Nowell-Smith are simply mistaken.

Unfortunately that is all that he shows. He leaves it an open question whether there might not be some verb other than 'shall' or 'will' for which 'M' might hold a place in utterances of the sort 'S M V if S ...'; and Nowell-Smith in his article "If's and Can's" takes advantage of this lack of conclusiveness on Austin's part and there proposes that 'does' may indeed be such an alternative verb.

Shwayder's arguments against analyzing utterances of the sort, 'S can V', into utterances of one of the above three sorts, (a), (b) or (c), focuses not upon the particular verb 'M' is alleged to hold a place for but rather on the clause which is to follow the 'if' in each of (a), (b) and (c). To that extent Shwayder's arguments are stronger than Austin's since if successful they will count against any attempt to so analyze 'S can V' utterances regardless of the particular verb 'M' is to hold a place for.

Shwayder presents his arguments against (a), (b) and (c) above in a very abbreviated form in the following paragraph.

...saying...[that S can V]...is not equivalent to saying that a condition for...[S's]...doing...[V]...is satisfied, in the sense of at least one such condition, for singular...[S can V utterances]...always assert that specific conditions for...[S's doing V]...are satisfied.

For the same reason...['S can V']...should not be taken to assert a disjunction of conditions for doing...[V].

Saying that...[S can V]...is also not equivalent to saying that all conditions for...[S's doing V]...are satisfied, as is revealed in the consideration that a person can say, e.g., "We can't be late", meaning that one is not permitted to be late to this kind of function, and his companion, without contradiction can say, "We can be late", meaning that time is short. (MS, p. 74)
We must remember that the fact which any alleged analysis of utterances of the sort, 'S can V' must account for is that an audience upon being confronted with an utterance of the 'S can V' sort, if they understand it, do come to hold a belief which we may schematize as, 'S if F', where 'F' holds a place for some specific predicate, however complex. It is for this reason that the analyses (a) and (b) above will not do. In case (a), 'S M V if ...', the three dots hold a place for some predicate though none in particular; yet, as Shwayder notes, the descriptive belief warranted by an 'S can V' utterance is a specific one. In case (b), 'S M V if F v G', the disjunction of predicates has the same effect as the three dots. There is no telling which of the disjuncts is the one which the speaker has in mind, so to speak, in uttering 'S can V'.

The third case, (c), 'S M V if F & G', falls heir to a different kind of consideration than cases (a) and (b) above do. In this case, the if-clause allegedly specifies all of the conditions normally considered necessary for successful V'ing. As noted earlier it may very well be impossible to actually specify all the conditions on any of the activities for which 'V' may hold a place in 'S can V'. Furthermore, there are cases, as Shwayder notes, in which speakers may without contradiction, affirm and deny of someone that he can do something. If case (c) above were the case, then our two speakers should have contradicted one another.

I must note in passing here, that D. Lewis has offered a possible world semantics for counterfactuals of the sort, 'S would V if S were F', which, if it can be shown that the use of 'can' to attribute an ability means the same as (has at least the same truth conditions as) a counterfactual,
provides a very tempting version of the analysis view of 'S can V' utterances. (I say "tempting" here, since the notion of a possible world is not a notion that is universally held to be very clear or even ontologically respectable. And it would be an alternative version of such a schema as 'S M V if S is F' simply because the verb in the antecedent clause is, on Lewis's account, in the subjunctive, rather than the indicative, mood.)

I shall not review Lewis's analysis of counterfactuals in any detail here, since I think that it can be shown very quickly that 'S can V' utterances cannot be analyzed into 'S would V if S were F' utterances. We have only to note the following points. If someone utters 'S can shoot' meaning that S is unguarded, in most (if not all) cases in which we would count such an utterance true, it is true that S is unguarded but false that S has shot or is shooting. In Lewis's terms, this means that the counterfactual into which the true utterance 'S can shoot' would be analyzed would have to have a true antecedent and false consequent. Yet Lewis is quite clear that, in his system at least, such counterfactuals are false. "I have claimed that the counterfactuals with true antecedent and false consequent are false."?

I conclude, then, that at least until this matter of differing truth values is cleared up, Lewis's semantics, however fruitful they may be for counterfactuals, should not be supposed equally adequate for 'S can V' utterances. I also feel safe in concluding that the analysis view in any one of its three forms cannot explain how it is possible for an audience to come to hold a specific belief of the 'S is F' sort upon being confronted with a particular utterance of the 'S can V'
sort. And so we have to discuss what I have called the Ellipsis view.

The Ellipsis View

Briefly this view would have us believe that 'can' is a device whose function it is to signal to an audience that what the speaker means is other than what his utterance means. Shwayder seems to hold something like this view. He argues that

...there are regularized, conventional instrumentalities, often words, by which speakers systematically mean more than they say. It is a feature of the meanings of the words in question that those words enable speakers to mean what they do not yet say, although another person would understand what the speaker means. (MS, p. 70)

What I shall do to counter this view is assume that 'can' is such a device and then consider the two ways in which the use of 'can' simpliciter (as in 'S can V' utterances) may be avoided. I shall conclude that neither of these two ways will enable us to avoid the use (in every case) of 'can' simpliciter to get our point across, so to speak.

The first of these two ways, not necessarily the best is this:

a) 'S can V' utterances may always be produced with an if-clause added. The appended if-clause will specify just what condition(s) on S's successful V'ing the speaker has in mind. Rather than produce an utterance of the sort, 'S can shoot', it is always possible for the speaker to produce instead, e.g., an utterance of the sort, 'S can shoot if he is unguarded'.

b) 'S can V' utterances need never be produced (simpliciter) at all. In every case in which an 'S can V' utterance is produced simpliciter, it is always possible to produce instead just an 'S is F'
utterance. Rather than produce an utterance of the sort, 'S can shoot', it is always possible for a speaker to produce just an utterance of the sort, 'S is unguarded'.

Ellipsis view (a) attempts to explain the function of 'can' simpliciter by supposing it to be a grammatical device the use of which simpliciter always requires supplementation with an if-clause. Its grammar requires it, just as singular subjects require plural verbs. So confronted with an 'S can V' utterance simpliciter, an audience who understands the function of 'can', understand that the speaker has for whatever reason omitted the if-clause. That knowledge together with the audience's extra-linguistic knowledge (of the speaker, situation, activity and so on) will enable the audience to infer (sometimes, at least depending on the audiences rationality) just what 'S is F' utterance the speaker would have produced in the omitted if-clause.

Now Austin offers us two kinds of arguments against this way of accounting for our use of 'can' simpliciter. In the first place, Austin claims that only if 'can' (and its cognates) is always used in the subjunctive mood do we have any reason to suppose such uses demand supplementation with an if-clause. But, Austin notes, though "...it is quite true that 'could have' may be, and very often is, a past conditional...it is also true that 'could have' may be and often is the past (definite) indicative of the verb 'can'...Once it is realized that 'could have' can be a past indicative, the general temptation to supply if-clauses with it vanishes; at least there is no more temptation to supply them with 'can'." (IF, p. 215)
Since I am no grammarian I shall rest content here with
Austin's opinion. But we need not come to a full stop. Austin has
a second argument, which I shall quote in its entirety. It seems to
need no comment.

It might at this point be worth considering in general
whether it makes sense to suppose that a language could
contain any verb such as 'can' has been argued or implied
to be, namely one that can never occur without an if-clause
appended to it...let the verb in question be to X; then
we shall never say simply 'I X', but always 'I X if I Y'
but then also, according to the accepted rules, if it is
true that 'I X if I Y', and also true (which it must surely
sometimes be) that 'I do, in fact, Y', it must surely follow that
'I X', simpliciter, without any 'if' about it any longer.
(IF, p. 216)

Now I take this argument to be a conclusive refutation of
ellipsis view (a). It is simply a mistake to think that an audience's
'S is F' belief may be accounted for by supposing 'can' to be a
grammatical device whose use always requires an if-clause.

Consequently, if we had to contend only with (a) we should have
to conclude that the ellipsis view is mistaken, i.e., that 'can' is
not always an implicating device. But there is still (b) to consider,
and, indeed, it does look the more plausible of the two. In order to
show that (b) is mistaken I shall have to show that (sometimes at least)
it is correct to say of a speaker that he meant that, e.g., S can shoot
rather than that, on that same occasion, he meant that, e.g., S is un-
guarded. For if (b) is correct, then it must be a mistake to suppose
that any speaker might be correctly said to mean that S can V on an
occasion no matter what 'V' might hold a place for. For if 'can' is
a device the use of which simpliciter is always completely avoidable
in favor of the 'S is F' utterance expressing the belief the speaker
expects his audience to come to hold, then it must be a mistake to
think that a speaker can mean that S can V on an occasion. If 'can' is this kind of implicating device then no one could mean that S can V (no matter what 'V' might hold a place for).

Consider the following situation. Some friends and I are watching Arnold Palmer line up a five foot putt and I turn to one of my friends and remark, 'He's (only) got a five foot putt.' My friend replies, 'What do you mean by that?' And I answer, 'I mean that he can sink it.' Or a friend answers, 'He means that Palmer can sink it.'

Now surely if it makes as much sense as it seems to, to say of me (in this example) that I meant that Palmer can sink his putt, then it must make sense to suppose that, had I wanted to produce an utterance that meant on that occasion just what I meant on that occasion, then I should have produced an utterance of the 'S can V' sort, namely, 'Palmer can sink it.'

How might one reply to this kind of example (and I am sure with a little imagination one could produce many more such examples)? The only reply I can think of would be to claim that there are things which a speaker can mean on an occasion which cannot be specified in or by specifying the meaning on an occasion of some utterance. That is, in the example, though its clearly correct to say that I meant that Palmer can sink it, it must be a mistake to think that one can specify what I meant by specifying the meaning of 'Palmer can sink it.' Clearly, one cannot specify what I meant by specifying the meaning of 'He's (Palmer) only got a five foot putt.'

Now I do not think this reply is acceptable and for two reasons. In the first place, were it acceptable then it must follow that there
are ineffable thoughts, feelings, beliefs, intentions, etc., (at least one). Such a view is to me repugnant, for it means that there are aspects of human nature, as it were, about which no one can have anything to say. In the second place, it would mean that the Principle of Expressibility and so the linguistic turn would be mistaken. And this too, I am unwilling to accept and more importantly neither would Chomsky, Davidson, Searle and Grice. At least, I think that all other alternatives should be exhausted and shown unacceptable before we should lapse into making claims concerning the nature of the ineffable.

I conclude that neither ellipsis view (a) nor (b) can satisfactorily account for the fact that we do use 'S can V' utterances simpliciter to attribute abilities. That is, I conclude that these last two sections successfully accomplish their end. In the introduction to this chapter I pointed out that a scientific explanation of our ability to speak and understand a language depends upon the pre-theoretic prejudice that 'S can V' utterances are correctly characterized as descriptive. I pointed out that part, at least, of the impetus behind such a prejudice lies in the very nature of a scientific explanatory strategy. I remarked that it would be very surprising result if this were all that lies behind such a prejudice. Theorists of the caliber of Chomsky, Davidson, Searle and Grice just cannot be assumed to have erected their theoretic edifices on such shaky grounds.

I then noted a second, less pejorative reason for this prejudice. Audiences who understand 'S can V' utterances do come to hold beliefs that we may schematize as 'S is F', descriptive beliefs. And more often than not, such beliefs just are the beliefs which the speakers
in question intend them to come to have. In lieu of any critical
examination of this fact, nothing could be more natural than to assume
that 'S can V' utterances may be characterized as descriptive.
But no utterance (type) comes with a tag attached labeling it as
descriptive or whatever. The characterization of an utterance (type)
as descriptive stands in need of some defense. I then stated my
intention to launch an investigation of the above mentioned prima
facia reason for the pre-theoretic prejudice that 'S can V' utterances
may be (correctly) characterized as descriptive. I have now concluded
that an audience's 'S id F' belief may not be accounted for by either
the analysis view or the ellipsis view of 'S can V' utterances.
Supposing that 'S can V' utterances are descriptive requires at least
supposing either

1) that such utterances mean the same as (some) utterances
which may be correctly characterized as descriptive, i.e., 'S if F'
utterances; or, failing that,

2) that 'can' (or 'can V') is a device whose simpliciter use,
together with an audience's extra-linguistic knowledge, warrants an
audience in coming to hold the relevant 'S is F' belief.

I think that the last two sections establish that neither
(1) nor (2) above will satisfactorily account for the non-pejorative
reason behind the prejudice that 'S can V' utterances are descriptive.
Hence we seem to be left with two alternative conclusions. There is
no good reason for such a prejudice save the fact that a scientific
explanatory strategy demands it. Or, if this is too harsh a judgement,
then there must be some third way (other than the analysis or ellipsis
view) of accounting for an audience's descriptive belief upon
understanding an 'S can V' utterance. And this third way must justify the characterization of 'S can V' utterances as descriptive.

Now I do not believe there is any such third way. In the next chapter I shall argue that under the assumption that 'S can V' utterances are descriptive (however that assumption/prejudice is to be justified, if it can be) no scientific explanation of our ability to speak and understand a language is testable. As representative of scientific theories constructed under this assumption/prejudice, I have chosen Davidson's and Searle's to investigate. If I am correct then the question whether there is a third way to justify this assumption/prejudice is a moot point. Even if there were, a scientific theory could not adequately explain our ability to speak and understand a language.

Now I want to conclude this chapter with a suggestion as to the proper characterization of 'S can V' utterances. If they are not descriptive, what are they? As we shall see in a moment I believe they are, what Austin has called, verdictives. If so, then what stands in need of explanation is not what makes it possible to describe someone as having the ability to speak and understand a language, but rather what makes it possible to pass the verdict that someone has the ability to speak and understand a language. In the concluding chapter of this essay I shall consider just what kind of explanation has any hope of accomplishing this end, since the scientific presents us with no hope at all.
Verdictives

To begin to see why I think that 'S can V' utterances should be considered verdictive let us consider the constraints any characterization of 'S can V' utterances must meet. Whatever characterization we decide upon, it must be such that it will enable us to account for these facts.

a) An audience who understands an 'S can V' utterance does come to have a belief of the 'S is F' sort, where 'F' holds a place for some specific predicate descriptive of some state of the world;

b) no utterance of the 'S is F' sort, no matter how complex the predicate might be for which 'F' holds a place, may be supposed to be, in all cases, a meaning preserving alternative to an utterance of the 'S can V' sort.

I take constraint (b) to rule out of consideration any attempt to characterize 'S can V' utterances as descriptive. That is to say, I take constraint (b), which I have in effect spent this entire chapter arguing for, to be such that we just cannot suppose the characterization of utterances of the 'S can V' sort to be the same as our characterization of utterances of the 'S is F' sort - where such 'S is F' utterances are to be counted true if and only if the world is such that whatever is picked out by whatever phrase 'S' holds a place for, falls within the extension of whatever predicate 'F' holds a place for.

Now supposing that in characterizing an utterance as a description or whatever, we are specifying what Austin has called the illocutionary force of that utterance, constraint (b) may be taken to have this rather far-reaching consequence. In so far as a theory or
grammar of a natural language is to describe the state of the world which makes utterances of the sort, *'S can speak and understand a language*', true, any such theory or grammar must be just wrongheaded. Utterances of the sort, *'S can V'*, of which *'S can speak and understand a language'*, is one, just are not descriptions, i.e., such utterances just cannot be supposed to have the illocutionary force possessed by an utterance describing the world.

What then is a theory of language supposed to do if not describe the world in some terms? We shall answer this when we have decided just what illocutionary force *'S can V'* utterances do have. So here and now I want to propose or suggest that such utterances are, what Austin has called, Verdictives, i.e., they have the illocutionary force of a verdict.

...verdictives are typified by the giving of a verdict, as the name implies, by a jury, arbitrator, or umpire. But they need not be final; they may be, for example, an estimate, reckoning, or appraisal. It is essentially giving a finding as to something - fact or value - which is for different reasons hard to be certain about.

Verdictives consist in the delivering of a finding, official or unofficial, upon evidence or reasons as to value or fact, so far as these are distinguishable...Verdictives have obvious connexions with truth and falsity as regards soundness and unsoundness or fairness and unfairness. (HT, p. 152)

To sum up, we may say that the verdictive is an exercise of judgement. (HT, p. 162)

Now Austin has provided us with a list of some of what he took to be species of the genus, Verdictive. He includes on his list such well know act-types as: grade, assess, characterize, interpret as, value, diagnose, find (as a matter of fact), understand, describe and analyze. (HT, p. 152) He concludes his list with this, to my mind, quite interesting comment.
Further examples are found in appraisals or assessments of character, such as 'I should call him industrious'. (HT, p. 152)

In so far as what people can and cannot do may be counted part of their character, I take this comment to be quite consistent with my view of the nature of the speech act performed in producing an utterance of the 'S can V' sort, that it is an assessment, an exercise of judgment, a delivering of a finding upon evidence or reasons as to value or fact.

Now I have put this claim as a proposal or suggestion simply because I have no arguments to give in its behalf other than that it seems to me that so construing 'S can V' satisfactorily accounts for both constraints (a) and (b) above. (Interestingly enough, Austin lists 'describe' as a species of verdictive. Nevertheless, it should be clear that he is not thereby classing descriptive utterances as verdictives. It is, rather, that taking an utterance as a description is a verdictive act. Whether it follows from this that there are no utterances which are naturally, i.e., literally, descriptive, regardless of how they are taken, I am not now prepared to say, though I suspect that this is the case.)

Clearly, taking 'S can V' utterances as verdictives meets constraint (b) above. If we are to blur the distinction between assessment (appraisal or evaluation) and description it shall have to be on the side of verdiction, not that of description. So that we may count all so called descriptions, verdictions, but not conversely. But this is not a point I shall argue for here, though I think it could be very convincingly stated.
In regard to constraint (a), will counting an 'S can V' utterance a verdiction, account for the fact that audiences who understand such utterances do come to have specific beliefs, an utterance of which would have the form, 'S is F'? The answer seems clearly, yes. If an audience understands such utterances then it must be correct to suppose that that audience is aware that the 'S can V' utterance in question is a '...delivering of a finding, official or unofficial, upon evidence or reasons as to value or fact...' And what could the evidence or reason in question for such a finding be than that the speaker believes that some descriptive predicate, 'F', is true of the subject. And so, depending upon the audience's familiarity with the speaker, his knowledge of the activity in question, and his rationality, it seems no small wonder that occasionally an audience will come to believe just what the speaker intends him to believe as a result of the speaker's 'S can V' utterance. We may place the warrant for an audience's specific descriptive belief upon being confronted with an 'S can V' utterance, in the verdictive nature of the speech performance engaged in by a speaker who produces an utterance of the 'S can V' sort.

In order to introduce the next chapter let me briefly review this chapter. I have to this point considered two ways in which a theorist might try to establish the initial plausibility of scientifically explaining how it is possible for a person to speak and understand a language. I have argued that this initial plausibility depends upon one's establishing that utterances of the sort 'S can V' are descriptive by showing that it is possible to specify the meaning of such utterances by specifying the meaning of utterances of the sort, 'S is F'. What
I have called the analysis view maintains (in general) that
'S can V' utterances mean the same as 'S is F' utterances; what I have
called the ellipsis view maintains that predicates of the 'can V' sort
are devices enabling a speaker to mean something other than what he
says.

I have in this chapter considered both of these alternatives and
declared each a failure. I conclude that it is just not plausible to
expect a scientific explanatory strategy to produce an adequate explana-
tion of how it is possible for a person to speak and understand a
language. Nevertheless, though my arguments may be beyond reproach
my conclusion is not. Scientific theorists like Davidson and Searle
may well be inclined to overlook my arguments on the grounds that even
partial confirmation of their respective theories would be enough to
confirm the pre-theoretic prejudice that 'S can V' utterances do indeed
describe some state of the agent in question, my arguments to the
contrary notwithstanding. It must seem easier to find fault with my
few arguments than to face the prospect of having to revise, if not
abandon, an explanatory strategy as well entrenched as the scientific.

Consequently, if my conclusion is to have the persuasive force
I want it to have, it is incumbent upon me to show that there is some-
thing amiss with the confirmation procedures advocated by both
Davidson and Searle. Just exactly what we can expect to find wrong
with these testing procedures is this. In both cases confirmation will
occur upon a testee's agreement that he would attribute to an agent
meeting the conditions specified in the explanans the ability to speak
and understand a language. If the attribution of such an ability
were descriptive then a testee ought to be able to tell solely from
from the information supplied by the explanas whether he would attribute to an agent meeting those conditions the ability in question. Should it prove to be the case that some additional, extra-theoretic information must be utilized by a speaker/testee to confirm such (scientific) explanations as Davidson's or Searle's then such theories must be inadequate as they stand.

Hence, we may conclude either that such theories require supplementation or alteration or that the attribution of the ability to speak and understand a language is not correctly characterized as a description of the agent in question. Clearly, the results of this chapter cast some substantial doubts on the latter alternative. In the next chapter I hope to substantiate such doubt.
FOOTNOTES FOR CHAPTER IV

1. D. Davidson, "Theories of Meaning and Learnable Languages", in Proceedings of the 1964 congress for Logic, Methodology, and philosophy of Science, 1964. Hereafter referred to as TMLL.


4. Taken from "Meaning and Saying", D. S. Shwayder, 1971. Hereafter referred to as MS.

5. J. L. Austin, "If's and Can's", in Philosophical Papers, ed. Urmson, and Warnock, Oxford Press, 1970. Hereafter referred to as IC.


CHAPTER V

TESTING A SCIENTIFIC THEORY OF LANGUAGE

In chapter four I argued that it is a mistake to presume that utterances of the sort 'S can V' are descriptive. To the extent that 'S can V' utterances are paradigmatic attributions of ability, it follows that it is a mistake to think that in attributing an ability we are describing the agent in question. I concluded chapter four with the observation that one major consequence of this conclusion is that scientific attempts to explain the attribution of such an ability must fail on the grounds that they are not in principle testable. If the attribution of an ability is not descriptive of an agent then the information on the basis of which a speaker/testee attributes an ability cannot be wholly specified in terms of a set of statements descriptive of some state of an agent.

In chapter one I went to some lengths to establish that satisfaction of (what I called) the information condition on (scientific) explanation demands that the explanans provide sufficient information to enable a speaker/testee to tell if the explanandum in question is true. Should this prove not to be the case, i.e., should it prove necessary for a testee to rely on extra-theoretic information (information not provided by the explanans) in order to confirm some putative explanation, then we must count that attempt at explanation a failure. I also
pointed out that the test of any (scientific) explanation depends upon our having some antecedent understanding of the explanandum in question, i.e., our understanding of the explanandum must be prior to our understanding of the explanans. Satisfaction of (what I have called) the ignorance condition on explanation requires that there must have been a time (at least in principle) at which one is unaware of the information conveyed by a potential explanans, but at which time one is quite aware of what one wants explained, i.e., there must have been a time when one understood the explanandum but without understanding the explanans (whether because the explanans had yet to be formulated or because it was formulated in theoretic terms with which one is unfamiliar).

In chapter two I pointed out that the explanandum of a (scientific) explanation as applied to linguistic phenomena is what I called a linguistic characterization. That is, a characterization of some utterance token (act or object) as, e.g., meaningful, a promise, true and so on. I further noted that the test of any (scientific) explanation resides in a speaker/testee's ability to tell from the information, and only from the information supplied by the explanans whether he would characterize the utterance token in question in the way in question.

In this chapter I shall examine the testing procedures of both Searle and Davidson. My purpose shall be to establish that supposing the attribution of ability to be descriptive does indeed have as a consequence that scientific attempts (at least Davidson's and Searle's) to explain the possibility of such attribution just are not testable. We shall begin with Davidson.
Davidson's Testing Procedure

I noted in chapter three that Davidson supposes that the sort of theory he is proposing may be tested by deciding whether the instances of Convention (T),

\[ \text{T)} \ a \text{ is true if and only if } p, \]

(suitably relativized to time, speaker, context and whatever else seems necessary) which are deducable from the theory are true.

A theory of meaning (in my mildly perverse sense) is an empirical theory, and its ambition is to account for the workings of a natural language. Like any theory, it may be tested by comparing some of its consequences with the facts. In the present case this is easy, for the theory has been characterized as issuing in an infinite floor of sentences each giving the truth conditions of a sentence; we only need to ask, in selected cases, whether what the theory avers to be the truth conditions for a sentence really are. A typical test case might involve deciding whether the sentence 'Snow is white' is true if and only if snow is white.¹

I further noted that this decision on the part of a speaker/testee amounts to a decision whether some instance of (T) may be used to explain the truth of a characterization of some particular utterance object as true. My criticism of this test is simple enough to state. I do not believe that a speaker can decide whether an instance of (T) is true without relying on (or surreptitiously introducing) extra-theoretic information to do so. I am going to argue that an explanans having the form,

\[ \text{T)} \ a \text{ is true if and only if } p. \]

\[ \text{a)} \ p \]

does not provide sufficient information for a speaker to tell whether the explanandum,

\[ \text{b)} \ a \text{ is true.} \]

is true.
We may begin by getting clear on just what sort of information such an explanans does provide. In the first place, as I noted in chapter three (section two), what is to replace 's' in an instance of (T) is to be a description or representation of a sentence type. We may adopt any convention for doing so that seems appropriate, concatenation or Gödel numbering or some system of phonetic representation. I shall merely use single quotes.

In the second place, Davidson is clear that it is sentence tokens that are properly characterized as true or false. Tokens of sentence types have truth values, sentence types do not. This point may be seen most clearly in considering the role demonstratives play in determining truth value.

I turn now to one more, and very large, fly in the ointment: the fact that the same sentence may at one time or in one mouth be true and at another time or in another mouth be false. Both logicians and those critical of formal methods here seem largely (though by no means universally) agreed that formal semantics and logic are incompetent to deal with the disturbances caused by demonstratives....No logical errors result if we simply treat demonstratives as constants; neither do any problems arise for giving a semantic truth definition...What suffers in this treatment is not the definition of a truth predicate, but the plausibility of the claim that what has been defined is truth. (TM, p. 463)

Davidson proposes that the truth predicate be relativized to time and speaker to avoid the mistake of characterizing types as true.

We could take truth to be a property, not of sentences, but of utterances, or speech acts, or ordered triples of sentences, times, and persons; but it is simplest to view truth as a relation between a sentence, a person and a time. (TM, p. 464)

I shall go Davidson one better here, and simply specify in the statement of an instance of T that it is a sentence token that we are talking about. Whatever constraints must be placed on the truth predicate (and however they are to be applied) in order to make truth sensitive
to changing times and speaker, may all be captured by simply mentioning the fact that it is, after all, a token of a sentence type that is to be characterized as true. So we may rewrite (T) as:

\[ T' \] Any sentence token of the type, 'Snow is white' is true if and only if snow is white.

What is to replace 'p' in an instance of (T) (as (T') illustrates) is a specification of the type of condition under which a token of the type represented by what replaces 's' is true. It is to be a specification of the truth conditions for any token of the type of sentence in question. So the second premiss (a) in a Davidsonian explanans is to be a statement or description of the prevailing (existing) conditions at the time of utterance; when the token of the sentence type in question was produced by the speaker in question. So filling out our earlier schema premiss (a) might read (leaving the temporal indicator implicit in the tense of the verb):

\[ a' \] Snow is white.

For future reference, we must note that the decision to describe the prevailing (and relevant) conditions at the time of utterance in such a way that it is clear that those conditions are of a type with the type of conditions mentioned in the (relevant) instance of (T) (or (T')) is no simple matter of observation. The decision whether to describe a table on a particular occasion as red or scarlet or even pink, is a decision not only relative to one's own expertise in matters of color, but also relative to the purposes and aims of the conversation in progress at the time, as well as the perceived level of sophistication in matters of color of one's audience. We shall see that such difficulties play no small role in deciding whether or not to characterize some sentence token as true; and this latter)
decision just is a decision whether the explanandum of a Davidsonian explanation is true.

Now we are going to run into some trouble regarding this explanandum. As I have schematized it, it must appear that the description or representation of the sentence type (what is to replace 's' in an instance of (T)) must reappear in the explanandum. And this would be a mistake. Again, sentence types are neither true nor false, they do not have truth values, sentence tokens do. If I had relativized the truth predicate as Davidson suggests, I should have to specify the particular speaker and time in question by instantiating the variables that would have to occur in the relevant instance of (T). For clarities sake I propose to add a third premiss to the explanans.

c) U uttered a token of the type s.

And this will become in our filled out example,

c') U uttered a token of the type 'Snow is white';

'U' here should be counted an abbreviation for the name of the particular speaker in question. Our explanandum now becomes simply

b') What U uttered is true.

The clarity which I hope to achieve with this formulation of a Davidsonian explanation is this. (b') as stated makes it clear and obvious (which (c) even with a relativized truth predicate does not) that it is the characterization of what a speaker utterers as true that is to be explained. One of the reasons behind the resistance on the part of a number of contemporary philosophers to the Davidsonian program centers around the belief that what is uttered by a speaker on an occasion is just not the sort of thing that may
properly said to have a truth value.

Cartwright has argued, for example, that it is instead what is asserted by a speaker on an occasion that is properly characterized as true (or false).\(^3\) Prima facia, what is uttered may not be identified with what is asserted. What is uttered is a set of sounds, what is asserted is not. What is uttered may have been produced with haste or loudly, what is asserted may not. The same set of sounds may be characterized in one context as an assertion but in another as a question. Different assertions may be made with tokens of the same set of sounds.

To the extent that the test of an instance of \((T)\) (or \((T')\)) rests on a speaker's decision to the effect that such an instance may be used in explaining the truth of an explanandum like \((b')\) above, the testee must be able to tell whether he would characterize what U uttered (a token of the type 'Snow is white', in our example) as true given the information supplied by the explanans. And if speakers of a natural language do not in fact characterize what is uttered as true, but rather what is asserted, then knowing what is uttered on an occasion, but not what was asserted on an occasion, just is not enough information to tell whether what is uttered is true.

Davidson is not unreceptive to this sort of criticism but he apparently does not consider it very damning. He says in,

"True to the Facts", that it

...will not matter to the theory whether we read this predicate ...
...[the truth predicate]..., "Sentence 's' is true (as English) for speaker 'u' at time 't'", or, "The statement expressed by sentence 's' (as English) by speaker 'u' at 't' is true." Those who believe we must for further reasons retain statements as truth vehicles will find the second formulation with its complex singular term ("the statement...") and one place predicate ('is true') more perspicuous."
Making such a change in schema \( (T') \) will result in this:

\[ T'' \] Any statement made by an utterance of a token of the type 'Snow is white' is true if and only if snow is white.

We shall also have to add to the explanans these two premisses.
In addition to

\( a' \) Snow is white.

and

\( e' \) U uttered a token of the type 'Snow is white'.

we shall have to include the information

\( d' \) U made a statement when he uttered a token of the type 'Snow is white'.

and

\( e' \) U stated that snow is white.

So now our explanandum will read,

\( b'' \) What U stated is true.

Premises \( (d') \) and \( (e') \) each present different problems for Davidson. First, it is no simple matter of observation to determine whether U made a statement when he uttered a token of the type 'Snow is white'. And second, including a specification of what U stated \( (e') \) appears to defeat part, at least of the purpose behind Davidson's proposal to construct a theory of (a particular) language on the foundation provided by replacing the (apparently intentional) connective, 'means that', with the (apparently extensional) connective, 'is true if and only if'.

Including \( (d') \) seems to lend credence to Searle's claim that a theory of speech acts is not only complementary (rather than inconsistent) with a theory of language of Davidson's sort, but also is a necessary adjunct of such a theory. We need a theory of speech
acts in order to test an instance of schema (T''), at least so long as a premiss like (d') must be included in the explanans of a Davidsonian explanation.

Now while Davidson might have no great objection to including a Searlean theory of speech acts in a complete theory of language, he certainly would object to including a premiss like (e') above in one of his explanans. One of the more attractive features of Davidson's program is that it promises to provide an effective way to determine the meaning of any meaningful sentence in a natural language by giving its truth conditions. But (e') apparently reintroduces a problem at least as difficult, namely, that of specifying what a speaker said (or stated, or asserted) on an occasion. If recall from my earlier discussion of Davidson's program, (in chapter three) it is the 'that' in the formula, "a means that p", with its suggestion of intensional entities, which causes such problems for a theory of meaning. There is no reason not to expect these same problems to overtake our present version of Davidson's theory of truth, so long as we must include a premiss having the form, 'U stated that p', in an explanation of the sort needed to test any instance of (T) like (T'')

In his article, "On Saying That", Davidson takes a stab at eliminating the problems posed by that-clauses in indirect discourse. He puts the problem nicely.

Since Frege, philosophers have become hardened to the idea that content sentences in talk about propositional attitudes may strangely refer to such entities as intensions, propositions, sentences, utterances and inscriptions; what is strange is not the entities, which are all right in their place (if they have one), but the notion that ordinary words for planets, people, tables and hippopotami in indirect discourse may give up these pedestrian references for the exotica. (ST, p. 172)
Since it is the apparent failure of the laws of extensional substitution that gives rise to speculations about intensional entities, Davidson proposes that the content-sentence (the sentence following the 'that') be separated from the introducing phrase, 'U stated that' making the 'that' a demonstrative referring to an utterance.

The proposal then is this: sentences in indirect discourse as it happens, wear their logical form on their sleeves (except for one small point). They consist of an expression referring to a speaker, the two place predicate 'said' and a demonstrative referring to an utterance. Period. What follows gives the content of the subject's saying, but has no logical or semantic connection with the original attribution of a saying. This last point is no doubt the novel one, and upon it everything depends: from a semantic point of view the content sentence in indirect discourse is not contained in the sentence whose truth counts. (ST, p. 171)

This proposal will mean that in our example we must replace premiss (e') with

\[ e'' \] U stated that.
Snow is white.

So in our explanandum,

\[ b'' \] What U stated is true.

the phrase, 'What U stated' no longer threatens reference to an intensional entity. Unfortunately, it is not terribly clear just what 'What U stated' does refer to. Prima facia, it seems to refer to the second of the two utterances produced by someone producing a premiss like (e'') above. But this would be true only in the unlikely event that the speaker referred to by 'U' is the same speaker producing the (e'') premiss. In fact, what the phrase, 'What U stated', refers to is an utterance supposed to be synonymous (by the producer of the (e'') premiss) with the second of the two utterances produced
in producing the \(e''\) premiss in question. What an utterance of the first of the two utterances in \(e''\) does

...is announce a further utterance. Like any utterance, this first may be serious or silly, assertive or playful; but if it is true, it must be followed by an utterance synonymous with some other. The second utterance, the introduced act, may also be true or false, done in the mode of assertion or of play. But if it is as announced, it must serve at least the purpose of conveying the content of what someone said. (ST, p. 171)

Now unless I have missed something, introducing the notion of synonomy here is tantamount to introducing the troublesome notions indicated by the phrases, 'means that', and 'means the same as'. And it was just these notions that we had hoped to eliminate by means of Davidson's proposed analysis of that-clauses.

Of course, we can always take the notion of synonymy here to indicate sameness of truth value rather than sameness of meaning. But then what theory of truth shall we use to explain our belief that the two utterances (the one produced in producing \(e''\) and the one produced by \(U\)) do indeed have the same truth conditions? Surely, not Davidson's, for that would be to reintroduce just the difficulties I have been discussing. At least, we cannot utilize Davidson's theory as amended to deal with statements rather than sentences.

No doubt it is just such difficulties as these that lends intensity to the insistence (by some philosophers) that what is uttered by a speaker on an occasion may be, no matter what, properly characterized as true. Now I think that I can show that even if it is what is uttered that is properly characterized as true, the information supplied by a Davidsonian explanans cannot be sufficient for a speaker/testee to decide whether he would characterize what was uttered as true. It will be helpful to restate our earlier example of such an explanation.
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T') Any token of the sentence type, 'Snow is white' is true if and only if snow is white.

a') Snow is white.

c') U uttered a token of the type 'Snow is white'.

Thus, b') What U uttered is true.

What I shall do is state in summary just what my argument will be and then consider whether one or another of the premises leading up to my conclusion may be supposed mistaken.

1) An instance of (T) specifies the truth conditions for any token of a certain sentence type.

2) It does not specify the conditions under which it is correct to characterize a token of that type as true.

3) The test of an instance of (T) rests in a speaker's ability to tell whether a token of the type in question is correctly characterized as true under the conditions specified in the instance of (T) in question.

Thus, 4) No instance of (T) can provide a sufficient amount of information for a speaker to assent to it (to tell if it is correctly characterized as true.)

If the conclusion, (4) is false, then one of the premises must be false. (1) is clearly a statement of what an instance of (T) as Davidson conceives it, is supposed to do. It cannot be questioned without admitting that something is drastically wrong with Davidson's program. This means that my conclusion, (4), can only be avoided by supposing that either (2) or (3) above is wrong. If (2) is wrong then we may suppose that a specification of the truth conditions for an utterance token does specify the conditions under which it is correct to characterize that utterance token as true. If it is (3) that is mistaken then we may suppose that the test of an instance of (T) rests, not in a speaker's ability to tell if a token of the type in question is correctly characterized as true, but rather in a speaker's ability
to tell if a token of that type is (or would be) true should the truth conditions specified be realized in the world.

Let us begin our examination of (2) and (3) above by asking if it can be maintained that (3) is mistaken. Can a speaker decide whether an utterance token is true without thereby deciding whether that token may be correctly characterized as true? I think the answer is, No. This question should not be confused with another. Can a speaker decide whether an utterance token is true without thereby deciding whether to communicate that decision to an audience? The answer to this last question is clearly, Yes. We are here dealing with two distinct decisions. But in the former question we are dealing (I think) with only one decision. Having decided that an utterance token is true, we are not then faced with a second decision may that token be characterized as true? If we were then it would be conceivable that we might decide that an utterance token is true but then decide that it may not be characterized as true (which is not the same decision as deciding whether to communicate out loud, so to speak, our decision as to that token's truth value - that would be a second decision.) But the decision not to characterize an utterance token as true seems to take back the decision that the token is true. If we have decided that a table may not be described as red, we just cannot (consistently) maintain that it is red. In deciding that an utterance token is true we just have decided that it may be characterized as true. Deciding whether it may be correctly characterized as true is another decision - one we are supposed to be able to make on the basis of the information supplied by the explanans of a Davidsonian explanation. Whether we can or not is just what we are to decide here.
How I do not wish to rest my case in behalf of premiss (3) solely on the above argument, as obvious as it seems to be.

Even if my little argument is not convincing, I think its point can be alternatively made by considering how the Davidsonian program may be used to construct a translation manual for a language other than English. W. Lycan has given a concise description of this procedure in his article, "Reality and Semantic Representation."

I shall quote him at length here since what he has to say will not only help to make my point, but also because his remarks seem to echo my own in regard to the overall view of Davidson's program that I believe should be adopted. According to Lycan, Davidson's program would have us believe that

...to learn a foreigner's language is to learn the truth conditions of his sentences, which is simply to be able to construct a theory of logical form for his language using a regimented cognate of our own language as a metalanguage. We begin by learning his acceptance pattern (the set of stimulus meanings of his words), on the basis of normal inductive inferences (say, by Mill's Methods) for his behavior under controlled questioning. After investigating some simple sentences of his language, along with what we take to be projective devices such as truth functional connectives, we arrive at a set of instances of Convention (T) the left hand structural description naming a sentence of the foreigner's language and the right hand side of the biconditional consisting of a correlated sentence of our own. Each (T) sentence is for us an hypothesis, an hypothesized universal law, subject to further confirmation or refutation. And, just as before, these (T) sentences will serve as crucial data for our theory of logical form - we will demand that our truth definition for the foreigner's language entail them. (Consider the foreigner's sentence, "Stephen Schlaft." We would, after a brief period of ostensive teaching, naturally form the hypothesis that this sentence is true just in case Stephen is sleeping. In any situation, we can learn the truth of "'Stephen schlaft' is true." by taking the foreigner's assent behavior toward that sentence at its face value and assuming that the foreigner is accepting the sentence because it is true. (This involves assuming as well that we can antecedently recognize assent and dissent behavior on the foreigner's part)).
Lycan is here claiming that the test of an hypothesized instance of schema (T) (an hypothesized universal law analogous to the empiric law that water boils at 212 F.) rests in a native speaker's assent to, what I have been calling, the explanandum of a Davidsonian explanation; in this case, it is the characterization of what U uttered, namely, a token of the type, 'Stephen schlaff', as true (where 'U' may be supposed to hold a place for the name of some native speaker of the foreign language in question).

What the foreigner is assenting to is the characterization as true of a particular token of the type, 'Stephen schlaff'. He is not assenting to the utterance token in question, but rather to its characterization as true. Indeed, I fail to see how his assenting to the brute utterance token (uncharacterized) could be used to draw any conclusion at all about the utterance token in question. He might for example, be merely assenting to the fact that the utterance token is meaningful, or that it is odd, or that it is loud, or impolite, any number of things. I conclude then that not only does it not make sense in general to think that a speaker may decide that an utterance token is true without thereby deciding that that token maybe correctly characterized as true, but also, I conclude that Davidson's program demands that it be the characterization of an utterance token as true that is to be assented to by a speaker in testing an instance of schema (T).

Now let us turn to the first of the two alternative ways I earlier mentioned in which my argument against the possibility of testing Davidson's thesis might have gone wrong. Have I made a mistake in assuming premiss (2)? That is, can we say that a
specification of the truth conditions for a token of a sentence
type does specify the conditions (or type of conditions) under
which a token of that type may be correctly characterized as true?
Are the conditions for the correct characterization of an utterance
token as true the same as the conditions under which that token is true?

Naturally enough, I think the answer to this question is, No.
I have two reasons for this opinion, the second of which I consider
the more convincing, though others may think the first more persuasive.
I think that it is correct characterize a token of a type as true
if one's evidence justifies such a characterization, even if, unbel-
knownest to anyone but God perhaps, that token is false. So far as
our paradigm example is concerned, this means that I think premiss (a')
the claim that snow is white, may be accepted even though for all we
know in Afganistan (or Pittsburgh) snow is grey. I think it is correct
to characterize as true a token of the type, 'Geese fly south for the
winter.', even though (on one plausible interpretation) such an
utterance token may be thought to be false because there are several
goose who don't make it every year. And though one might think me pre-
sumptuous to give my own opinion on these matters, if one considers
that I am a native speaker of English (and that these opinions are
J. L. Austin's as well), then perhaps my point will not be dismissed
so lightly.

The second reason I have for thinking that conditions for
correct characterization as true and so called truth conditions are
not the same may be roughly stated thusly. I think that one must
know just how tight a fit is demanded between the way the world is
and the way the world is alleged to be by what is uttered on an occasion
by a speaker, in order to decide whether such an utterance token may be correctly characterized as true. For example, a token of the type, 'France is hexagonal', uttered in a fifth grade geography class seems to me correctly characterized as true; while a token of that same type uttered in a graduate seminar in geography seems to me correctly characterized, not as true, but perhaps as rough and ready or approximate. Interestingly enough to say that something is hexagonal is not to say that it is or has the shape of a hexagon, but only that its shape is like a hexagon. Just how similar to the geometric hexagon something must be to be hexagonal is a question I do not believe can be answered without considering the context of utterance, the purposes of the speaker and the perceived sophistication level of the audience. Austin makes this point nicely. He claims that 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 used in making it is correlated by the descriptive conventions....'Is of a type with which' means 'is sufficiently like those standard states of affairs with which'. Thus, for a statement to be true one state of affairs must be like certain others, which is a natural relation, but also sufficiently like to merit the same 'description' which is no longer a purely natural relation.

It is not too difficult to see that the description of the standard states of affairs with which a sentence used to make some statement is correlated by (what Austin calls) the descriptive conventions of the language, corresponds to the specification of truth conditions so called, on the right hand side of the biconditional in an instance of (T). Also a specification of the prevailing and relevant conditions at the time of utterance, the second premiss in a Davidsonian explanation corresponds to a description of what Austin called the historic state
of affairs, the one to which the statement made is correlated by the demonstrative conventions of the language.

Now my point (my understanding of Austin's point) is this. I mentioned earlier in this section that the decision to describe the prevailing and relevant conditions at the time of some utterance (at the time of the making of some statement) in such a way that it is clear that those conditions are of a type with the type of conditions mentioned in the relevant instance of (T) is no simple matter of observation. Observation might be enough to determine whether the prevailing conditions are naturally like the standard conditions with which the sentence used to make the statement in question is correlated. But the question of that statement's truth or falsity hinges on something more than a natural relation of similarity. The prevailing conditions must be sufficiently like the standard conditions to merit, as Austin says, being characterized as true. And to say that this relation is no natural (or no purely natural one) is to say that increased empirical study of the relevant conditions will not (in every case) help us decide whether for example, France is sufficiently like a hexagon for a token of the type 'France is hexagonal' to be correctly characterized as true. We must take into account the aims and purposes not only of the speaker but also of the characterizer. That is, we must take into account the point of characterizing the utterance as true, as well as the initial point of the utterance.

If a token of the type, 'France is hexagonal' is offered in the course of teaching a fifth grade class of geography but the characterization of the token as false is offered in the course of a
graduate course in geography, we may well conclude that in the
graduate course they were too harsh, they failed to consider the
context of the original utterance. And we have made this determination
as a result not only of knowing the original context but also the
context of the characterization.

The difficulty Davidson faces is this. A specification of
contextual conditions at the time of utterance may well be incorporatable
into an instance of (T). But it is not the circumstances of utterance
that matter here, it is the circumstances in which the utterance is
characterized as true that matter. In testing an instance of (T)
we are concerned with the question of whether utterances merit being
characterized as true, or as I have put it, with the question of
whether the utterance is correctly characterized as true. To make that
decision we need to know not only the point of the utterance but also
the point of characterizing it as true.

My objection to Davidson's testing procedure come to this.
Since the explanans of a Davidsonian explanation does not contain
information regarding the context in which the question of an utterances
truth has arisen (but only at best information regarding the context
of the utterance) then that explanans does not provide sufficient
information to deduce the correctness of the characterization of the
utterance as true. And this, I believe, marks the Davidsonian program
as inadequate, or at least not adequately testable, without our
surreptitiously introducing extra-theoretic information. At the
conclusion of this chapter I shall have a few more things to say
about the Davidsonian program in general, and about the characterization
of utterance tokens as true in particular. But to get to that conclusion
I must first make good on my promise to show that Searle falls victim to the same sort of difficulty Davidson does.

Searle's Testing Procedure

In this section we are to examine Searle's testing procedure to see if it falls heir to the same sort of difficulty Davidson's does. We want to see if it is possible to confirm Searle's theory entirely on its own merits, without requiring any extra-theoretic information on the part of the testee to do so. Unfortunately, Searle is nowhere as clear as Davidson on just how such a test is to be conducted. So our task will be twofold - first we shall have to decide just what sort of test would confirm Searle's thesis; and second, we shall have to decide whether this test does indeed require extra-theoretic information for its successful application.

Towards the former end a brief summary of Searle's views will be helpful. He takes the speech act to be the primary unit of language, rather than the word or sentence.

The unit of linguistic communication is not, as has generally been supposed, the symbol, word or sentence, or even the token of the symbol, word or sentence, but rather the production or issuance of the symbol or word or sentence in the performance of the speech act.

His working hypothesis is that speaking a language is engaging in a particular kind of intentional behavior.

Speaking a language is engaging in a (highly complex) rule-governed form of behavior. To learn and master a language is (inter alia) to learn and to have mastered these rules. (SA, p. 12)

More specifically then, Searle's hypothesis is that speaking a language is performing speech acts according to rules.
The form that this hypothesis will take is that speaking a language is performing speech acts, acts such as making statements, giving commands, asking questions, making promises, and so on; and more abstractly, acts such as referring and predicking and secondly, that these acts are in general made possible by and are performed in accordance with certain rules for the use of linguistic elements. (SA, p. 16)

Our ability to speak and understand a language is to be explained by explaining our ability to perform particular speech acts. And our ability to perform particular speech acts is to be explained by attributing to a speaker knowledge of the various sets of rules according to which various particular speech acts are performed. In effect then Searle is offering us a general hypothesis - that speaking a language is engaging in a rule governed form of behavior - which is to be partially confirmed by confirming a number of specific hypotheses. Instances of these specific hypotheses will each claim that a particular type of speech act, e.g., promising, is performed in accordance with some particular set of rules.

The general form of a specific (Searlean) hypothesis will be the matching of a particular set of rule formulations with, what Searle calls a linguistic characterization, e.g., that a speaker has made a promise. The rule formulations are offered as explanations of the truth of the linguistic characterization on the grounds that it is knowledge of the rules formulated that make it possible for us to recognize some linguistic behavior as, e.g., a promise, and so to correctly make the linguistic characterization in question.

We need to distinguish between (a) talking, (b) characterizing talk, and (c) explaining talk - the difference between e.g., (a) "That's an apple", (b) "'Apple' is a noun", and (c) "The rule for the indefinite article preceding a noun beginning with a vowel requires an 'n' as in 'an apple'". (b) and (c) are linguistic characterizations and explanations respectively. I have been
emphasizing that the ability to do (a) is what underlies and, indeed, what explains the possibility of knowledge of certain kinds of statements of kind (b). It is the data of kind (a) as recorded in statements of kind (b) which are explained by explanations of kind (c). (SA, p. 15)

Now a little needs to be said about the nature of this knowledge of rules upon which Searle's specific hypotheses depend. Clearly, it would be a mistake to suppose that a speaker knows the rules governing a speech act if knowing the rules implied that he knows a formulation of those rules or even if it implied that he knows that the behavior is rule following behavior. The question is, may we (with sense) attribute to someone knowledge of some (set of) rules even though that person knows of no formulation of those rules and may even be unaware that in behaving in such and such a way he is following any rules? The answer is, yes. At least it is in case we are talking about someone's game playing behavior. There seems to be a clear sense in which we may say of someone playing chess that he knows the rules of chess even though he may not be able to formulate any of the rules. (It is not so clear that we may also say of our chess player that he knows the rules of chess even if he is unaware that in making the moves he does he is moving according to (some) rules. That is, though it does make sense to say a person is playing a game even though he may not be able to state any of the rules defining that game, it may make no sense to persist in characterizing his behavior as playing a game if he is not aware that he is following some rules. We shall have occasion to return to this point in another connection later in this essay.) Searle agrees.
Sometimes in order to explain adequately a piece of human behavior we have to suppose that it was done in accordance with a rule, even though the agent himself may not be able to state the rule, and may not even be conscious of the fact that he is acting in accordance with the rule. The agent’s knowing how to do something may only be adequately explicable on the hypothesis that he knows (has acquired, internalized, learned) a rule to the effect that such and such, even though in an important sense he may not know that he knows the rule or that he does what he does in part because of the rule. (SA, p. 42)

Searle believes that our "...knowledge of linguistic characterizations is of a similar kind...", that is, that a speaker’s knowledge of linguistic characterizations may only be explained on the hypothesis that he knows (has acquired, internalized, learned) some (set of) rules.

Now the nature of a speaker’s knowledge of (what we may, for the moment, call) speech act rules suggests a distinction between rules and rule formulations. Clearly, if a speaker may be said to possess knowledge of speech act rules without knowing any formulation of such rules, it would be a mistake to identify a (set of) speech act rules with any (set of) speech act rule formulations. In general, so long as we are talking about rules governing behavior, this distinction should be obvious.

Rules may be followed, broken, enforced, or announced. Rule formulations may not. Rule formulations may be in French or English. Rules are not in any natural language. Rules may be expressed or formulated in some language in or by uttering some set of words. But there does not seem to be any particular form of words in which a rule must be formulated.
A request for an example of a rule can be satisfied by producing a full sentence in the indicative ("The dealer at bridge always bids first"), a "that" clause ("(The rule) that students enrolled in American colleges must have a high school diploma"), a verbal clause in the infinitive ("To show small capitals in proof, underline two times"), a full sentence in the imperative ("Do unto others as you would be done by"), an imperative phrase ("No smoking in classrooms") - and no doubt in many other ways. There is indeed no special form of words conventionally reserved for the formulation of rules.

And though it may be tempting to think so at this point, we may not identify a rule with some canonical formula defining a class of equivalent rule formulations. A class of rule formulations may no more be broken or obeyed than may a particular rule formulation.

Now since part, at least, of a specific (Searlean) hypothesis is presented to us in terms of some set of rule formulations, it will be helpful to note a couple of positive features of rule formulations. Whatever set of words a rule governing behavior is formulated in, that set will provide one who understands it with at least the following kinds of information. It will identify or describe a certain kind of human action, bidding at bridge, smoking, entering college, or what have you. And it will indicate whether the kind of action in question is forbidden, required or permitted. And in doing so it may further indicate the conditions under which the kind of action described is forbidden, required or permitted.

Hence, even before we come to inspect an example of a specific (Searlean) hypothesis we may expect to find that the rule formulations presented will provide one who understands them with sufficient information to identify an instance of the kind of action in question and tell whether and perhaps under what conditions, such instances are forbidden, required or permitted.
Now I have noted that the form of a specific (Searlean) hypothesis will be the matching of a particular set of rule formulations with a particular linguistic characterization. We have to this point a good idea of what to expect in the way of a set of rule formulations, but what of linguistic characterizations? We need to know a little more about them.

As I earlier noted, linguistic characterizations record facts about our linguistic behavior. According to Searle, such facts are, what he calls, institutional facts; "...the fact that a man performed a certain speech act, e.g., made a promise, is an institutional fact." (SA, p. 52) It is worth calling such facts as are recorded in linguistic characterizations, institutional facts, in order to distinguish them from another kind of fact, what Searle calls, brute facts. It will be worth our while to quote Searle at length on this distinction.

There is a certain picture we have of what constitutes the world and consequently of what constitutes knowledge about the world. The picture is easy to recognize but hard to describe. It is a picture of the world as consisting of brute facts, and of knowledge as really knowledge of brute facts...The model for systematic knowledge of this kind is the natural sciences, and the basis for all knowledge of this kind is generally supposed to be simple empirical observations recording sense experiences. It is obvious that large tracts of apparently fact stating language do not consist of concepts which are part of this picture...there are many kinds of facts, and facts which obviously are objective facts and not matters of opinion or sentiment or emotion at all, which are hard, if not impossible, to assimilate to this picture. Any newspaper records facts of the following sorts: Mr. Smith married Miss Jones; the Dodgers beat the Giants three to two in eleven innings; Green was convicted of larceny; and Congress passed the Appropriations Bill. There is certainly no easy way that the classical picture can account for facts such as these. That is, there is no simple set of statements about physical or psychological properties of states of affairs to which the statements of facts such as these are reducible; a marriage ceremony, a baseball game, a trial, and a legislative action involve a variety of physical
movements, states, and raw feels, but a specification of one of these events only in such terms is not so far a specification of it as a marriage ceremony, baseball game, a trial, or a legislative action...Such facts as are recorded in my above group of statements I propose to call institutional facts. (SA, p. 51)

Now I shall call those terms the use of which marks off a characterization of some fact as the characterization of an institutional fact, institutional terms, for example, 'married', 'convicted of larceny', 'touchdown', 'checkmate', and more to the point, 'refers', 'warns', and 'promises'. Clearly, a particular linguistic characterization (of the sort we are interested in) must involve the use of some institutional term. Consequently, we may say that the use of such an institutional term as, 'promise', records the facts to be explained by a specific (Searlean) hypothesis.

Now let us recall a few of the points made so far. Searle's general hypothesis is that speaking a language is performing speech acts according to rules. The ability to speak and understand a language (to perform and recognize the performance of speech acts) is to be explained by attributing to a speaker knowledge (albeit, tacit knowledge) of these rules. This general hypothesis is to be partially confirmed by confirming some specific hypotheses. A specific hypothesis will be that a particular kind of speech act, e.g., promising, is performed according to a particular set of rules. So the ability to promise may be explained (under the general hypothesis) by attributing (tacit) knowledge of these rules to a speaker/promisor.

I think we have now completed the first of the two tasks I set out to accomplish in this section. Confirmation of a specific (Searlean) hypothesis will depend upon actually formulating the rules according to which a particular kind of speech act is performed; and then seeing
if a speaker who understands the rules as formulated would agree that behavior in accordance with the rules would be correctly characterized as a performance of the kind of speech act in question. Searle's discovering procedure, as we might call it, involves first stating a set of conditions necessary and sufficient for the performance of the kind of speech act in question and then extracting from those conditions the rules required by his general hypothesis. It will be helpful to have something a little more concrete to work with. I think we may accept the following as a tentative formulation of (the form of) the first step in Searle's strategy.

P) In uttering x S promised something if and only if S's behavior satisfies the following conditions....

'S' here holds a place for the name of any speaker. I have left the conditions (to be specified in place of the three dots) unspecified here simply for the sake of convenience. We shall have occasion later to explicitly formulate the rules constitutive of promising (at least according to Searle) and there these conditions will receive as complete a statement as we shall need.

(P) is a generalization which may be used, together with the premiss,

1) U's behavior satisfies the conditions..., to explain the truth of such an explanandum as,

2) U promised something.

If we go on to ask why (P) is true, why those conditions and not some others, Searle's answer, as we shall discover in more detail later, will be to formulate a rule having the form, 'X counts as Y', where 'X' holds a place for a specification of (some at least of) the
conditions specified in (P), and 'Y' holds a place for some appropriate speech act term, in this case, 'promising'. Such a rule would look something like this:

(R) Satisfying conditions...counts as promising something.

According to Searle, (R) is the explicit statement (or rather something very like (R) in form) of the internalized rule, tacit knowledge of which on the part of a speaker, enables him to confirm an explanation of the explanandum, (2), given the explanans consisting of (P) and (l). That is, (R) is supposed to explain a speaker's recognition that (P) may be used to explain the truth of the characterization of any speaker's behavior as promising something.

Now insofar as (R) appears to be merely a paraphrase of (P), but with the connective, 'counts as', replacing 'if and only if', confirmation of (P) just will be confirmation of (R) provided we can show how to explain (P) on the basis of (R) and (R) just is an example of a specific Searlean hypothesis. The difficulty is of course that 'counts as' is not apparently a truth functional connective, so prima facia, the scientific explanatory strategy we have been concentrating on throughout this essay appears to be undercut somewhat. That is, the inference from (R) to (P) does not appear to be straightforwardly a truth functional one, and so (R) and (P) do not appear to be deductively related, at least they do not without a lot of fixing, as it were.

J. Ransdell in his article, "Constitutive Rules and Speech Act Analysis", has made a stab at providing his fixing, though I shall not saddle him with the intent I have in mentioning his proposals. After taking note of the fact that Searle considers game-rules to
be paradigm examples of the sort of rule he is proposing may be used to explain generalizations like (P), and so the truth of our linguistic characterizations, Ransdall, clear headedly enough, decided that a close look at game rules such as those governing baseball, will help us to understand better the nature of the rules Searle supposes will explain our ability to speak and understand our language.

Though I think Ransdall goes wrong, it will profit us to pay attention to what he has to say.

Ransdall distinguishes two kinds of game rules, what he calls first order rules and second order rules. First order rules specify the conditions under which certain game terms may be correctly applied. For example, 'Allowing four Balls to Pass while one is Batter counts as being Walked'; I shall observe Ransdall's practice of capitalizing obvious game terms wherever they occur. Second order rules specify the kind of behavior forbidden, permitted, or required (as the case may be) by anyone (or thing, we shall restrict ourselves to people, to Players) correctly characterized by the game term in question.

For example, 'Allowing four Balls to pass while one is Batter counts as being henceforth subject to the following rules: ____.', (CRA, p. 390)

He then notes that game terms are in principle eliminable in favor of a formula relating by means of the connective, 'counts as' the conditions under which a game term is applied and the kind of behavior forbidden, permitted or required of anyone satisfying those conditions. (CRA, p. 390) He then claims that the resulting formula, "...can be regarded as functionally equivalent to the conditional form, 'If X then Z', since the satisfaction of certain conditions is represented to be a sufficient condition for the application of certain (first order) rules." (CRA, p. 391)
For example,

(S) If one has allowed four Balls to pass while one is Batter then one is required to proceed to first Base.

In fairness to Ransdall I must note a couple of points of difference between my formulation of (S) and what his text might lead one to believe would be his formulation. In the first place, I have replaced his phrase, 'being henceforth subject to the following rules: ___', which occurs after the 'counts as' in his example of a second order rule, with my phrase, 'being required, permitted or forbidden to behave in the following ways: ___'. I have done so mainly because I wish to avoid a mistake Ransdall apparently makes in his article, a mistake taken note of by S. Boer in his article, "Speech Acts and Constitutive rules". As I noted a few pages back, a rule of whatever kind (constitutive or not) describes a kind of activity and in one way or another (implicitly or explicitly) indicates whether that activity is forbidden, required or permitted of a certain class of people under certain specified conditions. According to Boer, Ransdall has assumed that all rules (at least, game rules and so speech act rules) indicate only that the kind of activity in question is required (that under the conditions specified a member of the class of people in question has obligated himself to behave in the appropriate way). Clearly, if speech act rules, at least, are so narrowly construed most speech acts are not governed by such rules, since most speech acts do not obligate one to do anything and so the rules governing such speech acts cannot be so formulated that they indicate that one is required to do such and such. In formulating a form for speech act rules we must apparently allow for the fact that some speech acts (if not most)
do not impose requirements (obligations) but rather bestow permission, as it were, on the audience or the speaker or both.

Of course, Boer may have missed Ransdall point here. Ransdall may be arguing that regardless of whether a rule indicates that the kind of behavior in question is required, permitted or forbidden, if it is properly taken to be a constitutive rule, then the rule is obligatory. That is, if the rule is properly called constitutive, then the agent is required to observe the rule in his behavior even if the rule only indicates that the behavior is permitted not required. In Basketball there is a strategy rule to the effect that one is to press one's opponent if one is losing by less than, say, five points and there is only a minute or so to go in the game. Under the above, admittedly sympathetic reading of Ransdall, such a strategy rule should be counted constitutive of Basketball, since, other things being equal (as the phrase goes) a coach who does not instruct his players to press under the conditions specified is not a good coach, i.e., does not know the game of basketball as well as a coach is supposed to know it. He is required to observe such a strategy rule in order to deserve the reputation of being a good coach (or the judgement that he is a basketball coach and not just someone drawing a salary).

No doubt it will be objected that this sympathetic reading of Ransdall imposes on him an equivocation on the notion of constitutive. If strategy rules are constitutive rules (on my sympathetic reading) then the constitutive/regulative distinction (which I shall elaborate on in a moment) among rules is otiose. I agree, hence my sympathetic reading is really sympathetic. As we shall see I believe this distinction
is properly regarded as one obtaining between rule formulations not rules. But Ransdall is not as clear as I would like and Boer has caught what he says (at least) if not what he means, in a rather glaring mistake. I think we can illustrate this mistake by using my formulation of (S) in an attempt to explain the truth of someone's claim that U was just Walked to first Base. If someone were to ask why U just ran to first Base, we might attempt a scientific explanation, on Ransdall's unsympathetic reading, thusly;

   a) U just allowed four Balls to pass while he was Batter.
   (a) together with (S) implies,
   b) U is required to proceed to first Base.

The difficulty is of course inferring from (B), a specification of what U is required to do, the truth of a description of what U in fact did, run to first Base. Since people don't always do what they are required to do, (b) may be true, but U simply not have gone to first Base.

This problem becomes acute if we are to continue insisting upon the scientific nature of any adequate explanation and (S) and (a) are accepted as an explanation of the fact that U ran to first Base. We should be able to retrodict the truth of,

   c) U ran to first Base.

But all we seem able to retrodict from (S) and (a) is the truth of

   b) U is required to run to first base.

Now I think we can avoid this problem if we simply note that it is not the truth of (c) that we want explained, but rather the truth of something like,

   d) U Walked to first Base.
To the extent that the tiresome analogy between speech act rules and game rules is to hold up, what we must want explained is the truth of a certain kind of characterization of behavior, namely, that involving the use of what Searle has called, institutional terms. 'Walked' is such a term, 'ran' is not.

Ransdall is just wrong (or at least has not adequately represented Searle's intent) in supposing that the terms defined in first order rules are eliminable. Such a supposition is clearly in contrast to Searle's belief that institutional terms may not be wholly reduced to or defined in terms used to describe or refer to brute facts. Ransdall's failure here accounts for the failure of (S) and (a) to explain (scientifically) the truth of (c), simply because it is not (c) that we should be concerned with, but rather (d).

We wanted an explanation of why it is correct to characterize what U did, namely, proceed to first Base, as being Walked. The answer here, our explanans, will include a specification of what U did and a generalization specifying the conditions under which anyone doing what U did may be correctly characterized as being Walked. I think we can most profitably replace (S) above with,

\[
S') \text{ Any Batter has been Walked to first Base if he proceeds to first Base after letting four Balls pass while he was Batter.}
\]

In which case we can replace (a) with,

\[
a') \text{ U was a Batter and proceeded to first Base after letting four balls pass while he was Batter.}
\]

Now we can retrodict the conclusion we wanted,

\[
d) \text{ U Walked to first Base.}
\]
Obviously, (S') does not utilize the 'counts as' connective. But Searle does not hold that 'counts as' must occur in all formulations of speech act rules, but only that it may with some naturalness be used. (SA, p. 36) He does think that its use most clearly highlights some distinguishing features of speech act rules, or of any rule like speech act rules such as game rules, for example, (S'). We could have written (S') as,

(S'') Proceeding to first Base after allowing four Balls to pass while one is at Bat counts as being Walked. (S'') wears it s status as a rule of Baseball on its face, so to speak. (S') does not. But (S') may be obviously used to explain, in the scientific manner, the truth of the characterization of U's behavior as being Walked to first Base. (S'') may not be so obviously used. I think with Ransdall that (S') and (S'') are functionally equivalent, where the function is to explain the truth of Baseball characterizations like (d), the explanandum in our above example. If someone should persist in demanding an explanation of (S'), i.e., an explanation of why (S') may be used in explaining Baseball characterizations like (d), it would seem that we would have no further recourse save to refer him to (a copy) of the official Baseball rule book, wherein he will no doubt find (a version of) (S'). Of course, there is no corresponding speech act rule book to which a theorist like Searle can refer an interlocutor who persists in asking why some particular speech act rule formulation may be used in explaining the truth of some linguistic characterizations. But this point merely underscores the remark I earlier made in introducing this section - it is not as clear in Searle's case, as it was in Davidson's, just how a speaker/testee
is to go about confirming a specific Searlean hypothesis
(and so, at least partially confirming his general hypothesis). Now,
I think, we may have a glimmering of just what confirmation of a
specific Searlean hypothesis at least should be like. If we can
formulate linguistic generalization like the Baseball generalization,
(S'), then we shall have made a promising start.

In the last section I argued that the kind of theory Davidson
proposes cannot be confirmed solely on the basis of the information
provided by the theory. And since confirmation of the theory
requires an exercise of the ability which the theory is to explain
the theory must fail to provide an adequate explanation of that
ability. I have claimed that Searle's kind of theory must face the
same kind of difficulty. I shall argue that an understanding of the
kind of rule formulations Searle presents in formulating specific
hypotheses cannot provide a speaker/testee with a sufficient amount
of information to tell whether behavior in accordance with the rules
as formulated is correctly characterized as a performance of the kind
of speech act in question. In other words, I shall argue that knowing
how to give a linguistic characterization of some behavior requires
knowledge that cannot be specified in terms of, and so cannot be pro-
vided by an understanding of, the kind of rule formulations Searle
proposes.

I have already pointed out that knowing how to give linguistic
classifications of behavior amounts to knowing how to use institutional
terms, such as, 'promise', 'touchdown', 'Walked'. I have also pointed
out that the kind of information a (set of) rule formulations may provide
will amount to a description of some kind of action and an indication
of whether (and perhaps under what conditions) that kind of action is forbidden, required or permitted. Hence, I shall be arguing that the kind of action description presented in the kind of rule formulations Searle proposes cannot provide a sufficient amount of information about the kind of action in question to tell whether an institutional term may be used to characterize an instance of that kind of action.

The conclusion I shall finally draw from this argument is simply that in using institutional terms to characterize behavior we are not thereby describing that behavior. And this conclusion should come as no surprise to anyone convinced by my arguments in chapter three. If the attribution of the ability to speak and understand a language is not a description then we might have expected that our use of institutional terms to characterize the various expressions of that ability would not be descriptive either. Though I shall wait until later to go into the matter in any detail, it should be apparent that I believe that Davidson went wrong at just this same point. Our ordinary truth predicate may be considered an institutional term, and as such, supposing of course that I make good on my arguments, our use of 'is true' to characterize some instance of linguistic behavior cannot be accounted for in terms that presume its use to be a descriptive one. And this is one way to see the sort of theory Davidson proposes.

Before taking a look at the (set of) rule formulations Searle actually presents, it will be instructive to consider what he has to say about the kind of rule he believes speech act rules to be, what he calls, constitutive rules, rules supposedly like, (S').
I have said that the hypothesis of this book is that speaking a language is performing acts according to rules. The form this hypothesis will take is that the semantic structure of a language may be regarded as a conventional realization of a series of sets of underlying constitutive rules, and that speech acts are acts characteristically performed by uttering expressions in accordance with these sets of constitutive rules. (SA, p. 37)

He attempts to clarify the notion of a constitutive rule by contrasting such rules with another kind, what he calls, regulative rules.

I want to clarify a distinction between two different sorts of rules, which I shall call regulative and constitutive rules. I am fairly confident about the distinction but do not find it easy to clarify. As a start, we might say that regulative rules regulate antecedently or independently existing forms of behavior; for example, many rules of etiquette regulate inter personal relationships which exist independently of the rules. But constitutive rules do not merely regulate, they create or define new forms of behavior. The rules of football or chess, for example, do not merely regulate playing football or chess, but as it were they create the very possibility of playing such games. (SA, p. 33)

In elaborating upon this contrast he has this to say.

Where the rule is purely regulative behavior which is in accordance with the rule could be given the same description or specification (the same answer to the question "What did he do?") whether or not the rule existed, provided the description or specification makes no explicit reference to the rule. But where the rule, system of rules, is constitutive, behavior which is in accordance with the rule can receive specifications or descriptions which it could not receive if the rule or rules did not exist. (SA, p. 35)

At this point, I think we must be very careful to observe the distinction, made a few pages back, between a rule and a rule formulation. Searle is not at all careful about it and as a result confuses rules with their formulations in a way that I think marks the beginning of the end for his thesis.

Searle is simply mistaken if he thinks that it is the existence of a (set of) constitutive rules which makes it possible to describe or specify behavior in terms that would otherwise not be available
for use. Clearly, of rules and rule formulations, only rule formulations can introduce any terms at all. Yet Searle seems to completely overlook this point. Throughout the following passage he speaks of constitutive rules when he should speak instead of constitutive rule formulations.

Regulative rules characteristically take the form of or can be paraphrased as imperatives, e.g., "When cutting food, hold the knife in the right hand", or "Officers must wear ties at dinner". Some constitutive rules take quite a different form, e.g., "A checkmate is made when the king is attacked in such a way that no move will leave it unattacked", "A touchdown is scored when a player has possession of the ball in the opponent's end zone while a play is in progress". If our paradigms of rules are imperative regulative rules, such non-imperative constitutive rules are likely to strike us as extremely curious and hardly even as rules at all. Notice that they are almost tautological in character, for what the 'rule' seems to offer is part of a definition of "checkmate" or "touchdown". That, for example, a checkmate in chess is achieved in such and such a way can appear now as a rule, now as an analytic truth based on the meaning of "checkmate in chess". That such statements can be construed as analytic is a clue to the fact that the rule in question is a constitutive one. The rules for checkmate or touchdown must 'define' checkmate in chess or touchdown in American football in the same way that the rules of football define "football" or the rules of chess define "chess"... (SA, p. 34)

If the constitutive/regulative distinction is to be one founded on the fact that so-called constitutive rules make available for use terms otherwise (in the absence of such rules) not available, while regulative rules, so called, do not, then I think it rather obvious that this distinction, if it is to prove useful at all, must be viewed as a distinction between rule formulations not rules.

(Interestingly enough, in the above quote, Searle speaks of paraphrasing regulative rules. Clearly, only rule formulations may be paraphrased.) Prima facia, this point poses a problem for Searle, not so much over whether the actual rule formulations he presents are acceptable (a question we shall get to directly) but over his calling
such formulations constitutive. Only our knowledge of rules may be considered tacit or internalized knowledge. Our knowledge of rule formulations must be explicit. So if the knowledge we possess in order to speak and understand a language is indeed tacit or internalized knowledge, then it can only be knowledge of rules not of rule formulations. So it must seem that the constitutive/regulative distinction, so long as it only applies to rule formulations and not rules, is not a very useful one to use if our aim is to identify in advance of formulation just the kind of rule a formulation of which may be used to explain our ability to talk.

In what follows I want to take a moment to show that Searle is indeed mistaken insupposing that the rules he formulates are properly called constitutive. I shall then go on to argue the major point of this section - that the rule formulations Searle does present do not provide enough information to tell whether an instance of the kind of behavior in question is or may be properly characterized in the (institutional) way in question. So let us take a look (finally) at the actual (set of) rule formulations Searle supposes may be used to explain our ability to promise. (SA, p. 63)

1) $Pr$ is to be uttered only in the course of a sentence $T$, the utterance of which predicates some future act $A$ of the speaker $S$.

2) $Pr$ is to be uttered only if the hearer $H$ would prefer $S$'s doing $A$ to his not doing $A$ and $S$ believes $H$ would prefer $S$'s doing $A$ to his not doing $A$.

3) $Pr$ is to be uttered only if it is not obvious to both $S$ and $H$ that $S$ will do $A$ in the normal course of events.

4) $Pr$ is to be uttered only if $S$ intends to do $A$.

5) The utterance of $Pr$ counts as the undertaking of an obligation to do $A$. 
In each of these rule formulations Searle uses the variable \( Pr \) to hold a place for what he calls an illocutionary force indicating device.

The illocutionary force indicator shows how the proposition is to be taken, or to put it another way, what illocutionary force the utterance is to have; that is, what illocutionary act the speaker is performing in the utterance of the sentence. Illocutionary force indicating devices in English include at least; word order, stress, intonation contour, punctuation, the mood of the verb, and the so called performative verbs. (SA, p. 30)

It is worth noting that because of the use of the place holder, \( Pr \), what Searle has presented are not rule formulations, but forms for rule formulations, what Alston calls, 'rule functions'. Nevertheless, whatever is to replace \( Pr \) may be considered a term essential to the description of the activity permitted, required or forbidden by the rule formulations.

Hence, if the above rule formulations are to be considered constitutive, then we ought to consider whatever is to replace \( Pr \) an institutional term, a term like, 'touchdown' or 'chess' which may not be used to characterize any sort of activity. But if we recall some of the illocutionary force indicating device Searle mentions, this conclusion becomes just absurd. Stress, intonation contours, and the like, just may not be used to characterize any sort of activity.

If 'touchdown' is an institutional term then the promise indicating device is not. We may use 'touchdown' to characterize some football behavior, but we may not use a promise indicating device to characterize some linguistic behavior. And though we may use a promise indicating device to promise, we surely may not use 'touchdown' to score a touchdown.
Now it no doubt will be quickly noted that all the above argument establishes is that the (kind of) rule formulations presented by Searle are not constitutive rule formulations. It does not show that such rule formulations do not explain our ability to characterize some kind of behavior as a promise, whatever kind of rule formulations they should be.

Nevertheless, to be an explanation of the sort Searle is after, The type of rule formulations he presents must be supposed to be explicit specifications of what a speaker who knows how to promise (at least tacitly) knows. And, as we have seen, in every case such rule formulations all involve mention of some particular linguistic device. Such a device may be specific to a particular language, e.g., 'promise', or it may be language neutral, so to speak. But in either case the explanatory force of such rule formulations requires that we attribute to speakers who know how to promise, tacit, if not explicit knowledge of the linguistic significance of the use of some such device.

To simplify matters, let us suppose that a speaker/testee need know only that it is a first person, present tense, indicative utterance of the words, 'I promise', which may signify the occurrence of a promise. And let us suppose that he knows how to use 'promise' to characterize some behavior as a promise. Now the rule formulations presented in a specific (Searlean) hypothesis will specify the conditions under which the above described kind of behavior (the utterance of 'I promise') is forbidden, required or permitted. Under the general Searlean hypothesis a speaker's ability to use 'promise' to characterize some behavior as a promise, is to be explained by attributing to him
knowledge of the rules formulated. Confirmation of a specific (Searlean) hypothesis depends upon whether, from only the information specified in the rule formulations concerning the behavior involving the utterance of the device, 'I promise', a speaker/testee can tell if an instance of such behavior is a promise, i.e., can tell if he would use the institutional term, 'promise', to characterize such behavior.

Consequently, if I can show that a speaker/testee who knows how to use the institutional term, 'promise', cannot, only from the information provided by any (set of) Searlean rule formulations, tell whether instances of the behavior described in the formulation is a promise, then the purpose of this section will have been accomplished.

To use an illustration of Searle's (slightly altered to fit our situation) we are in the position of people who know how to use such terms as, 'checkmate', 'en passant', 'castling', and 'chess', to characterize behavior involving the moving of little plastic men about a checkered board and we want an explanation of our ability to do so. We form a general hypothesis that the plastic men are moved according to rules. We then set about confirming that general hypothesis by constructing specific hypotheses, one each for each of the characterizing terms we know how to use. Each specific hypothesis will putatively specify the conditions under which a particular kind of movement of a particular kind of piece (or pieces) may be characterized by the term in question, e.g., 'checkmate'.

Confirmation of a specific hypothesis will come with agreement that behavior in accordance with the (set of) rule formulations in question maybe characterized as, e.g., a checkmate. Now I do not
believe that any such agreement can occur solely on the basis of the information provided by such a specific hypothesis. In the first place, it seems clear enough that we would be willing to characterize that behavior as playing chess. Hence, the information provided by the specific hypothesis in question (the checkmate hypothesis) must include the information that the behavior is chess playing behavior. In fact, each of the specific hypotheses we are to construct must include this information. But 'chess' is one of the characterizing terms, to explain the use of which, we are to construct a specific hypothesis.

To avoid the obvious circularity here, we must suppose that it is our general hypothesis that will explain our use of 'chess'. So that the set of specific hypotheses will together provide the (set of) rule formulations knowledge of which will explain our ability to characterize, e.g., checkmate behavior, as chess playing behavior. (Analogously, we would only be willing to characterize someone's behavior as a promise if we are willing to characterize that behavior as speaking a language. Searle is not unaware of this point. He makes it in stipulating that normal input/output conditions must be assumed to obtain in considering any specific (Searlean) hypothesis. (SA, p. 57))

Hence, in order to tell whether some particular chess characterization may be applied to some behavior in accordance with some specific hypothesis solely on the basis of the information provided by that specific hypothesis, we must assume ourselves in possession of the information provided by all the other specific hypotheses, had they been stated. We must make such an assumption simply because it is only the set of specific hypotheses which provides the information necessary
to tell if the behavior in question is chess playing behavior. And without knowing that the behavior in question is chess playing behavior we cannot be in a position to tell if the chess characterization in question, e.g., 'checkmate', may be applied to any behavior in accordance with the specific hypothesis in question.

Now we can see our problem clearly. In assuming ourselves to be in possession of the information provided by the set of specific hypotheses regarding all chess characterizations, do we assume ourselves to be in possession of more or different information than would be provided by all the specific hypotheses were they all stated in conformity with Searle's prejudices? If so, then it is a mistake to suppose that a specific (Searlean) hypothesis may be confirmed solely on the basis of the information provided by that hypothesis, even under the assumption that since we know how to speak our language, we are in possession of the information that would be provided by the (set of) specific (Searlean) hypotheses (tacitly in possession of it, as it were).

The question we must answer is easy enough to formulate. Does a complete set of chess rule formulations specify all the information (tacitly) possessed by a person who knows how to characterize behavior as playing chess? The answer to this is clearly, no. A person who knows how to characterize behavior as playing chess knows that chess is a game, that it requires skill, that there are various strategies for moving the pieces, that some moves are brilliant, some stupid, that it is played for enjoyment, for recreation, for money, for something at any rate (if it were football we were talking about, then we could say that a person who knows how to characterize behavior as playing
football knows that some of the rules of football may be broken and the play still be characterized as playing football - two men on a side can play football, pass interference at the right time is good football, not non-play). None of the above bits of information will be found in any set of chess rule formulations, so long as that set contains only rule formulations governing the movements of plastic men on a checkered board. And despite a disclaimer by Searle, the rule formulations must indeed all involve mention of the use or movement of some specific plastic men or (in the case of speaking a language) some specific linguistic device. (SA, p. 34)

What has happened is this. 'Chess' is an institutional term used to characterize a kind of behavior which may be characterized by using other institutional terms, such as, 'checkmate'. Part of what is involved in characterizing behavior as playing chess is being able to characterize instances of such behavior as a checkmate (or as castling or capturing en passant). But what has been overlooked is that there are other institutional terms than those applied to moves on a board that may be applied to behavior which may be characterized as playing chess. While there may be no more to the game, 'chess', than just the rules governing (defining) the permissible moves, characterizing behavior as playing chess involves knowing how to use a good many other institutional terms than just those applicable to the moves of the game. Characterizing behavior as playing chess requires information (knowledge, tacit or otherwise) that cannot be provided by rule formulations specifying the behavior to which such terms as 'checkmate' and 'castling' may be applied.
We began our investigation of our ability to characterize some kind of behavior with terms like, 'checkmate', and 'chess', with the general hypothesis that the characterization in question may be explained by formulating (sets of) rules, one each for each of the institutional terms that may be used to characterize behavior known to be chess playing behavior. But we made a mistake in thinking that the set of institutional terms for which we needed such rule formulations was restricted to just those terms used to characterize the permissible movements of the pieces over a checkered board.

The information provided by the rule formulations explaining only those terms used to characterize the movement of the pieces just cannot be sufficient information to tell if behavior in accordance with those rules may be characterized as playing chess. What is missing are those rule formulations that would explain our characterization of chess playing behavior as, e.g., brilliant, stupid, enjoyable, exciting, dull, skillful, rash, pointless; namely, those rule formulations that would explain the strategy, point, purpose of the game of chess. And none of these rule formulations would involve or require explicit mention of the use of some particular (kind of) device (linguistic or chess).

I conclude that it is a mistake to think that the information provided by a specific (Searlean) hypothesis is sufficient to tell if behavior in accordance with the rule formulations in question may be characterized as, e.g., a promise. So long as the rule formulations presented govern only behavior describable as the use of some linguistic device (some illocutionary force indicating device) then the information provided cannot in principle be sufficient for deciding if behavior in
accordance with such (a set of) rule formulations may be characterized as a promise. What is missing are those rule formulations that would explain our characterizations of promising as being, e.g., a reason or a motive for other kinds of behavior, for rebuking or reproaching the promisor if he defaults, for holding men to account for and releasing them from their promises, for feeling releaved, reassured, exasperrated, or mad, or as being clever or stupid or honest or unnecessary or hasty, as being amended or emphasized or tentative; in other words, what is missing are those rule formulations that would explain the strategy, the point the role which promises have in our lives.

And none of these rule formulations could involve or require explicit mention of the use of some particular (kind of) illocutionary force indicating device. It remains to be seen whether at least part of an explanation of our ability to linguistically characterize behavior will involve the formulation of rules of the sort Searle proposes. I think this section suffices to show that any such explanation cannot be wholly composed of such rule formulations.

In the following chapter, my concluding chapter, I shall present some further considerations which I think will show that rule formulations of the sort Searle recommends, i.e., constitutive rule formulation, formulations requiring mention of a particular device in describing the activity governed by the rule, should not be considered even part of the explanation of our ability to speak and understand a language.
FOOTNOTES FOR CHAPTER V


2. Ibid., p. 546.

3. Ibid., p. 545.

4. Ibid., p. 545.

5. This is an example Cartwright uses in his article Propositions, we shall have occasion later to refer to this article.


7. Ibid., p. 85.


CHAPTER VI

THE EXPLANATION OF LINGUISTIC EXPLANATION

This chapter is to be my concluding chapter. Each of the preceding chapters may be thought of as a single piece of a larger whole. All of the pieces are now on the table, so to speak, and it is time to put them together. The result will be a picture of what is in my opinion the general inadequacy of a certain explanatory strategy as it is employed in the attempt to explain our ability to speak and understand a language. In the next few pages I shall try to describe this picture in a way that will focus attention on the inadequacies that I believe it portrays. I shall then conclude this conclusion with some suggestions regarding what I think is the proper explanatory strategy to be followed, a prolegomena, as it were, for any further theorizing in the philosophy of language.

The explanatory strategy which I believe to be inadequate is, of course, what I have called scientific. The first three chapters of this essay were devoted to a discussion of this strategy as it is employed in the work of four prominent theorists, Chomsky, Davidson, Searle and Grice. In the next two chapters (four and five) I presented some considerations designed to establish that this strategy is indeed inadequate. I concluded each of these two chapters with a suggestion as to why the arguments in each seem to work, that is, I suggested in
each case a reason behind the mistakes which my arguments seem to capitalize on. Simply put, that reason is this. Neither the attribution of an ability (let alone the ability to speak and understand a language) nor the linguistic characterization on an occasion of some speaker's utterance, should be presumed to be descriptions of anything. The presumption that utterances of the sort, 'S can V', and characterizations like, 'What he uttered is true', and 'He promised to do it', are descriptive of something is the mistake which I think leads to the sort of difficulties I have spent chapters four and five discussing.

At the end of chapter four I suggested that the attribution of an ability should be viewed as, what Austin called, a verdiction, the expression of a verdict or decision made for reasons of fact or value (to the extent that these can be distinguished as Austin says). A little later I shall suggest that this result regarding the attribution of an ability was to be expected on the grounds that all psychological characterizations should be viewed as verdictions, not just ability attributions.

At the end of chapter five I suggested that linguistic characterizations are made in terms (which I called institutional terms) that indicate the point or end of the behavior characterized. To that extent, linguistic characterizations should also be viewed as verdictions, i.e., as the expression of a verdict or decision to the effect that the behavior characterized succeeded in securing the end or point indicated by the characterizing (institutional) terms.

If I am right then obviously the notion of what constitutes an explanation, when the explanandum is either a linguistic or psychological
characterization is in for some serious revision. To the extent
that we do sometimes at least, succeed in explaining the correctness
of both linguistic and psychological characterizations, the notion of
explanation involved just cannot be that of scientific explanation.

What follows this introduction is divided into five sections.
In the immediately following section I shall summarize and try to
repaint the picture I have painted in the preceding five chapters.
In the next I shall go to some lengths to clarify what I see as the
principle mistake behind Searle's attempts to construct a theory of
speech acts along scientific lines. I shall then digress for a few
pages to argue that our characterization of human behavior in
psychological terms should no more be subjected to scientific
explanation than should our characterization of linguistic behavior.
In the next to last section I shall present my own account of just
how we should go about explaining the correctness of our linguistic
and psychological characterizations. And I shall finally conclude with
a few remarks concerning the consequences of my proposal for
theorizing in the philosophy of language in the future.

No doubt all this sounds a bit more ambitious than it really
is. All I am after is the outline of a program that can be successfully
followed, where success is to be measured in terms of understanding.
So let us begin with my picture of the last five chapters.

A Review of the Preceding Chapters

In chapter one I noted that a salient characteristic of any
explanation if that its explanans provides one with sufficient
information to determine whether the explanandum is correct. I
called this condition on successful explanation, the information
condition. I also pointed out that attempts (such as Hempel's) to formally specify the adequacy conditions on any successful scientific explanation may be viewed as attempts to specify a set of conditions which, if satisfied, will guarantee that the information condition is satisfied.

At one level of explanation the explanans of a scientific explanation must contain (or presume) some empirical generalization (an empirical law roughly like, 'Water boils at 212 F.') and a characterization of the prevailing and relevant conditions (roughly, 'The water on my stove is at 212 F.'). The explanandum will be a characterization of the specific phenomenon to be explained (again roughly, 'The water on my stove is boiling.'). The information condition will be certain of satisfaction provided the relation between explanandum and explanans is like that of (or formally the same as) the relation between premisses and conclusion of a valid deductive argument (and provided, of course, one understands the explanans).

In chapter two I indicated just how this explanatory strategy may be employed in explaining our ability to speak and understand a language. We may begin with what I called linguistic characterizations, i.e., statements characterizing some speaker's utterance on an occasion as, e.g., true, or meaningful or a promise or a warning. Such characterizations constitute the explananda of linguistic explanations. The explanans will consist at least of a specification of the prevailing conditions on the occasion of utterance. This specification is supposed to be a specification of the reasons on the occasion for the characterization constituting the explanandum. If those reasons are acceptable to a native speaker then we may say that those reasons explain the correctness of the linguistic characterization.
But we may not say that those reasons alone constitute a scientific explanation. What is missing from the explanans is what I called a linguistic generalization. If a native speaker does accept a specification of some (set of) conditions as explaining the correctness of some linguistic characterization then there must be a linguistic generalization in the off'ing, so to speak, one that provides the deductive warrant for the inference from the specification of conditions to the characterization. At least, there must be such a linguistic generalization if one is wedded to the scientific explanatory strategy. We may say that a speaker who accepts the specification of conditions as reason enough for the correctness of the characterization, does so on the basis of a linguistic generalization of which he is at least tacitly aware.

So we may say that a speaker who can speak and understand a language is either tacitly aware of a host of such generalizations or is possessed of the ability to tacitly formulate such generalizations as the occasion demands. The scientific explanation of a speaker's ability to speak a language consequently, must consist in a set of statements at least formally adequate to the task of deducing any and all of the linguistic generalizations discovered (roughly in the manner described above) to be correct. Such a set of statements will compose a theory of language (though not necessarily a theory of language acquisition). Such a set of statements will explain how it is possible for a person to speak and understand a language by being at least a formal representation of that state of mind or body in virtue of which a speaker can determine the correctness on an occasion of some linguistic characterization.
In very general terms, the above is the picture I painted in the first two chapters of the current state of mind of a large number of philosophers of language. After arguing in chapter three that four major theorists, Chomsky, Davidson, Grice and Searle, all do recommend overtly, or in their practice, that attempts to explain our ability to speak and understand a language proceed along these lines, I began my attack on this picture. I argued in chapter four that the attribution of any ability, let alone the ability to speak and understand a language, should not be presumed to be descriptive of any state of mind or body, because such attribution should not be presumed to be a description at all. I concluded that chapter with the suggestion that attributions of ability may be best viewed as, what Austin called, verdicts. Construing the attribution of an ability as a verdict accounts for the one salient feature in favor of counting them descriptions, namely that audiences, upon being confronted with such attributions, do, more often than not, come to hold beliefs the expressions of which would be descriptions of the agent or of his situation.

People who do understand attributions of ability do, as a matter of fact (I think), understand such attributions as verdicts (or judgments or decisions) made for reasons of fact or value, as Austin would say. And so, audiences who are competent speakers of the language in which the attribution is made and who are familiar with the speaker and the situation and even the agent to whom the ability is attributed, will, in casting about for the speaker's reasons for his decision, more often than not, discover it.
In chapter five I continued my attack on the possibility of adequately explaining (scientifically) our ability to speak and understand a language, by arguing that the recommended testing procedures of both Davidson and Searle fail to provide the kind of confirmation of their respective generalizations which a scientific explanatory strategy demands. I predicted at the end of chapter four that my thesis regarding the non-descriptive nature of attributions of ability would probably be resisted on the grounds that if linguistic generalizations produced under the assumption that such attribution is descriptive can be confirmed, then, despite my few arguments to the contrary, such attribution is best viewed as a description.

In Davidson’s case I argued that determining the correctness of a characterization of an utterance token as true depends upon a decision to describe the prevailing (and relevant) conditions at the time of utterance in such a way that it is clear that those conditions are of a type with the conditions specified in an instance of schema (T). I noted that this decision is one that must always be made on the basis of information that cannot be incorporated into a statement of an instance of (T).

The difficulty does not lie in schema (T), but rather in the statement of prevailing conditions that together with the relevant instance of (T) is supposed to allow a speaker/testee to deduce (or reproduce) the characterization of someone’s utterance at a particular time and place as true. The specification of the so called truth conditions, which is to occur on the right hand side of an instance of (T), must be a specification of truth conditions for any token of the relevant utterance type. And it is for this reason that the specification of truth conditions in an instance of (T) cannot include information
regarding the point of characterizing some particular utterance token as true. Yet without such information one is just not in a position to determine the correctness of any particular characterization of an utterance token as true.

Davidson might reply, of course, that the point of characterizing utterance tokens as true remains constant from speaker to speaker, context to context, audience to audience. But the fact is that it does not. And to stipulate that it does, (to idealize the data we have to work with in this way) is tantamount to tampering with the data. The fact remains, that in order to test a Davidsonian explanation the speaker/testee must in every case decide on the basis of information not included in a Davidsonian explanans, whether the specification of prevailing conditions in some particular case satisfies the point of characterizing the utterance token in question as true in that particular case.

In Searle's case, I argued that determining the correctness of a characterization of an utterance act as a certain type of speech act depends upon information that cannot be specified in terms of a (set of) rule formulations that describe some behavior and indicate whether it is forbidden, permitted or required under some conditions.

Taking Searle's analogy between game playing behavior and linguistic behavior seriously, I pointed out that to decide whether some chess play is correctly characterized as, e.g., castling, one must know that that play was perpetrated by an agent who was playing the game of chess. And knowing that an agent is playing a game, playing chess, requires more or a different kind of information than that which may be recorded by formulating the rules for the movement of chess pieces across a checkered board. Analogously, determining
the correctness of a linguistic characterization requires more or
different information than can be supplied by a (set of) rule
formulations specifying the conditions under which the use of certain
linguistic devices (i.e., words or illocutionary force indicating
devices) may be forbidden, permitted or required. One must know that
chess moves have points or ends, that there is a strategy (or several
possible strategies) that is being followed (for better or worse) by
the player in question in moving as he does. And appreciating or
recognizing this strategy requires knowing that chess is a game, having
the point or points of any game. So one has not specified what an
agent knows who knows how to play the game simply by specifying the
rules of the game. Analogously, in order to determine whether some
linguistic characterization is correct, one must know that the agent
in question was speaking a language; and this knowledge requires
knowing the point (or points) of the various speech acts supposedly
performed in speaking a language.

I have to this point been almost entirely critical. I think
we are now in a position to make a provisional diagnosis of the
underlying problem which seems to give my arguments the foothold
they have. The attribution of the ability to speak and understand
a language is made, not simply or merely on the basis of the behavior
of the agent in question, but on the basis of behavior we are willing
to characterize institutionally, i.e., with the use of what I have
called, following Searle, institutional terms. To the extent that
the predicate, 'can speak and understand a language', may be used to
attribute the ability to speak and understand a language, that
predicate is an institutional term on a par with, e.g., 'is a promise'.
As I mentioned in chapter two, no scientific theorist supposes (or should suppose, at any rate) that one can directly confirm a scientific explanation of the attribution of an ability. Such an explanation would supply information concerning the state of mind or brain of an agent (brute facts about the agent) which information could not be confirmed either by introspection or by physiological inspection. Introspection is clearly unreliable (the knowledge specified is, after all, supposed to be tacit knowledge) and there is no way to provide a non-question begging translation of the information provided by such an explanation into some physiological language.

Consequently, both Davidson and Searle (though Davidson not so clearly as Searle) maintain that the possibility of attributing the ability speak and understand a language is to be explained by explaining our ability to use institutional terms to characterize the linguistic behavior on the basis of which we do attribute the ability to speak and understand a language.

To this extent both Searle and Davidson may be supposed advocates of the same general hypothesis - that speaking a language is engaging in a rule governed intentional form of behavior. And both may be supposed to believe that this general hypothesis may be partially confirmed by confirming some specific hypothe - that our use of some particular institutional terms may be explained by attributing tacit or internalized knowledge of some particular set of rules to the speaker/testee who knows how to use the institutional term in question.

At this point, of course, Searle and Davidson part company somewhat. Davidson is of the opinion that our ability to speak a language may be explained by the set of rule formulations which will explain our ability to characterize instances of linguistic behavior as true
(or false). Clearly, 'is true', is an institutional term as I have preliminarily characterized such terms. It is used to characterize instances of linguistic behavior. Simply put, Davidson seems to believe that an account of the knowledge required to characterize instances of behavior as true (or false) will, ipso facto, account for the knowledge required to characterize linguistic behavior in other institutional terms. Searle apparently believes, without explicitly disagreeing with Davidson here, that our ability to speak a language may be explained by explaining our ability to sue a number of institutional terms, e.g., 'promise', 'warns', 'refers'.

We could consequently count Davidson's view as a special case of Searle's if we wanted to. But I am through talking about Davidson. I want now to concentrate on Searle, and in particular on Searle's apparent failure to appreciate the fact that institutional terms are not (primarily, at least) used to describe behavior, but rather to characterize behavior in terms that indicate the end or point of that behavior.

Indeed, this fact may be taken as the fundamentum differentiae of institutional terms, that which sets them apart from terms used to characterize what Searle has called brute facts. Parenthetically, we may speculate that Searle's insistence upon the kind of rule formulations he calls, constitutive, was motivated in part at least by a search for some reason behind the felt difference between these two kinds of terms. Some institutional terms, at least, though as I have tried to point out, not all, are indeed made available for use by the existence of non-natural social practices like the game of baseball. What my arguments against the possibility of testing a specific
Searlean hypothesis show is that the kind of rule a formulation of which may be used to explain our ability to characterize some chess playing behavior as, e.g., a checkmate, is just not the same kind of rule that may be considered defining for the move called, 'checkmate'.

What I am about to do (in the next section) is to provide a much more detailed diagnosis (than the above) of Searle's misplaced faith in the sort of rule formulations he proposes tacit knowledge of which will explain our ability to use institutional terms. I think this discussion will provide the beginnings of a proper account of the explanatory strategy that should be followed in attempting to explain our ability to speak and understand a language.

If we recall what was said in the first few pages of this chapter, we can fairly easily locate the point at which theorists with a penchant for the scientific have been led astray, so to speak. Explanations of the correctness of linguistic (or psychological) characterizations as they occur in our day to day lives, consist in a specification of one or a number of reasons for the characterization on the occasion in question. A scientific theorist will account for our acceptance of some reason as explaining on an occasion some characterization by positing the existence of a generalization of which we must be at least tacitly aware. He takes it to be part of his business, as it were, to discover and formulate such generalizations. A scientific theorist is wedded to the presumption that all explanations are explanations (properly so called) only so far as the explanans is deductively related via such generalizations to the explanandum. So it a speaker accepts some reason as explaining some characterization then there must be some discoverable generalization that will, so to speak, provide the authority behind that acceptance.
Now as I have noted, we do successfully explain to one another just why we are inclined to characterize each other's behavior on an occasion as having been an act of warning, or promising or referring. And any explanation is successful only if its explanans does provide one with enough information to decide whether the explanandum is correct. But it does not immediately follow from these two facts that in those cases of successful explanation in which there is no overtly stated generalization that there must be one nevertheless, one of which we must be tacitly aware. It may be that the relation between explanans and explanandum here, rather than being deductive, is more like that between reason and decision, where the reason does not logically force the decision, but rather supports it as a good one to make in the circumstances. Indeed, if I am right in taking linguistic characterizations to be verdictive in nature (rather than descriptive) then this latter sort of relation (the non-deductive) is just what we have to deal with.

If I am right then any question regarding the correctness of some linguistic characterization must be construed as a question regarding whether the point or end indicated by the characterization was secured or achieved. The reasons advanced in behalf of the characterization must be construed as specifications of the means used on the occasion in question to achieve (in the opinion of the characterizer) the end indicated. Deciding whether the characterization is correct will be a matter of deciding whether the reasons advanced are good ones, i.e., it will be a matter of deciding whether the means specified did result in, accomplish, or achieve, the end indicated by the characterization. Characterizing a linguistic characterization as correct is as much of
a verdict as the linguistic characterization in the first place. The decision expressed by characterizing a characterization as correct is one to the effect that the reasons advanced are good ones in the circumstances.

As should be obvious, it is my opinion that the relation between explanans and explanandum in a linguistic explanation should not be viewed as the same as that between premiss and conclusion in a valid deductive argument. The information supplied by such an explanans cannot be deductively sufficient for determining the correctness of the explanandum simply because the explanans can only (without begging the question) specify the means used to achieve the end which is specified in the explanandum. Whether that end was achieved by those means is in every case a matter of reasonable decision. Should we wish to formulate linguistic generalizations relating some particular characterization to some type of conditions (and behavior) we can only view those generalizations as, on the one hand, a recording of what has in the past in our experience been good, i.e., usually successful, strategy to follow in securing the end specified by the characterization, and on the other hand, as a recommendation for future behavior should one wish to achieve that end.

What needs to be done in order to provide at least the beginnings of an account of the proper explanatory strategy to follow in case we are dealing with characterizations of human behavior is this. We must come up with a way to specify the relation between explanans and explanandum in such an explanation. And we must account for the authority behind such explanans, that in virtue of which we do sometimes accept the explanans as a reasonable one, as explaining the explanandum. I believe the following section will take us one step closer to these ends.
Searle's Mistake

As I pointed out in the last chapter Searle would have us explain the correctness of a linguistic characterization in some particular case thusly. The explanans will consist of a linguistic generalization relating the appropriate characterization with a specification of the conditions under which some specific form of linguistic behavior must be taken in order for that characterization to be correct. The second premise of the explanans will be a specification of the prevailing conditions and of the speaker's behavior being characterized. For example,

a) Any speaker will have promised someone to do A if and only if that speaker behaved X'ly under conditions C.

b) U behaved X'ly under conditions C.

The explanandum will then be,

c) U promised someone to do A.

One of the actual rule formulations for promising that Searle proposes is this:

3) Pr is to be uttered only if it is not obvious to both S and his audience that S will do A in the normal course of events.

Pr here holds a place for the name of any speaker and A for a description of the action which the speaker has promised to do. Pr holds a place for a specification of any one of a number of promise indicating devices. The behavior which a speaker must take in order to perform an act of promising includes the uttering of one of the devices for a description of which Pr holds a place.

Now my objection to Searle's explanatory strategy does not lie, as it does in Davidson's case, in any difficulty a testee may have in deciding whether the specification (in premise (b)) of the speaker's
behavior and of the prevailing conditions are sufficiently like the behavior and the conditions specified in the generalizations (a) to warrant the conclusion that the speaker did indeed promise someone to do something. I argued instead that it is a mistake to think that a premiss like (b) does provide all the information a testee needs in order to tell whether a speaker's linguistic behavior on an occasion is correctly characterized as a promise. I indicated that the additional information which a speaker/testee does have and so which should be specified in an explanans if that explanans is to be adequate for telling if the characterization is correct, is the point of any act of promising, the role it plays in our lives. (Analogously, the knowledge which a chess player has in addition to his knowledge of the rules of the game, which enables him to characterize instances of chess play as, e.g., castling, is knowledge of the point of castling, the strategy such a move exemplifies as part of a game of chess.)

It is my opinion that linguistic characterizations are made in terms (institutional terms) that indicate ends or points of behavior. To characterize some behavior as a promise is to express one's belief that that behavior exemplifies a certain strategy, one apparently designed to achieve or secure the end or point indicated by the institutional term, 'promise', and that that strategy was successful - the speaker's behavior succeeded in being (in one's estimation) a promise.

To be as clear as possible (though it shall hopefully get clearer as we go through the next sections), I am not claiming that some behavior is correctly characterized as a promise only if that behavior is instrumental in bringing about some consequential effect. The point of marking a ballot with an x and placing it in the ballot box on election day at a polling place is to vote for a certain candidate.
To characterize someone's behavior as voting for so and so is to characterize that behavior in terms that indicate the end or point of that behavior. It is to express one's belief that that behavior exemplifies a certain strategy, one designed to achieve or secure the end or point indicated by using the expression, 'voting for so and so'.

There are, after all, other ways to go about voting for someone raising one's arm, saying 'aye', there needn't be any special day or special place. The behavior I have just described is not constitutive of voting for someone. But if one were asked to explain why one characterized this behavior as voting, one could (with some expectation of success) reply by noting that it is election day and the agent did mark his ballot with an x, and place it in the ballot box at the polling place. Such a reply is not an analysis of the concept of voting. It is an explanation of why one believes that the agent's behavior succeeded in securing the end or point one has indicated by characterizing it as voting for so and so.

One might be mistaken in one's characterization here for a variety of reasons. The ballot might have been improperly marked, it might not have been election day, the ballot might have been deposited in the wastebasket. In any of these cases one would have been mistaken in one's characterization because the behavior failed to achieve the end or point which one indicated in characterizing it as voting for so and so. The relation here between marking a ballot with an x, etc., and voting is not that between some behavior and some consequential effect of that behavior. Nor is the relation here that between two alternative descriptions, each necessary and sufficient for the other.
The relation is, I think, like that between means and end, where a specification of the means is no analysis of the end, and where the means is not the only one that may be followed to achieve the end. I hope to further elucidate this relation as we go along. Indeed, most of the remainder of this section and of the section after next should be considered attempts on my part to throw some more light on this relation and finally, to propose a way to specify this relation that will make clear just why a specification of means is on some occasion accepted as an explanation of the correctness of a characterization in institutional terms. I am not primarily concerned to analyze or elucidate the concepts marked by institutional terms. I am concerned to make the point that characterization in institutional terms is characterization in terms of ends or points (believed to be) secured or achieved by means of the behavior characterized in the circumstances in question.

Consequently, I shall not, in what follows, search out and use any alternative expression to characterize the end or point of behavior that is institutionally characterized. I shall speak of behavior that is characterized as a promise as behavior that has succeeded in being (in one's opinion) a promise. It is behavior that in one's estimation exemplifies a certain strategy, one that has succeeded in securing or achieving the point indicated by the characterizing term.

Now it is important to note that the behavior is to be understood as a strategy, a means, to an end. As such it is not (or should not be supposed to be) the only possible means. Indeed, one way to understand my objection to Searle's explanatory strategy is to see it as an objection against the necessity of incorporating in the
explain a specification of the speaker's linguistic behavior. I recognize that there is in English at least one promise indicating device (the words, "I promise", prefaced to what would otherwise be a statement, as Searle says, predicating some future action of the speaker) the use of which in the appropriate circumstances gives one extremely good reason to believe that the speaker has promised. But I believe that the use of such a device should be viewed as a means of insuring, as much as one ever can, that one's audience understands the point of one's audience understands the point of one's linguistic behavior on the occasion in question. The use of such a device should usually be viewed as good strategy if one's aim is to promise. But neither its use nor the use of any other so-called promise indicating device should be viewed as necessary (nor should a disjunctive set of such devices be viewed as necessary) for it to be correct to characterize some linguistic behavior as a promise.

It would be necessary to incorporate in one's explanation of the correctness of some linguistic characterization a specification of the speaker's linguistic behavior only if that specific behavior is part of the point, as it were, indicated by the characterizing term. It is part of the point of playing (some) games that one follow the rules defining for the game. So it is not surprising that in explaining why one thinks it correct to characterize someone's behavior as playing the game of chess, one should have to incorporate into one's explanans some formulation of the rules defining for the game of chess. Indeed, I think this is why Searle's insistence upon framing the sort of rules he does on analogy with game rules seems so plausible; what he has failed to realize is why game rules should be included in an explanation (or some of them anyway) of the correctness of characterizing some
behavior as playing the game. Some games are played not simply for the sake of winning, but for the sake of winning according to the rules of the game. Where a type of activity is such that people have a special interest in following the rules of that activity just for the sake of following the rules (and not for the sake of any penalty or failure that may come from not following them) we may say that the rules constitute the activity. But we must not lose sight of the fact that they do just to the extent that people retain an interest in following the rules for the sake of following them, as in (paradigmatically) activities that are correctly characterized as game playing. Polite behavior may be considered game playing, at least on some people's part, just in case one persists in behaving in a (polite) way even though the circumstances (or the changing times) clearly dictate that such behavior is inappropriate (or old fashioned, perhaps), no doubt there are other (social) examples. In a more linguistic context, it used to have capitulated or surrendered until and unless he uttered the word, 'uncle'. Little boys wouldn't accept as correct a characterization of the behavior of one of them as an act of capitulation or surrender unless he pronounced the word, 'uncle'. We may say that part of the point (or rather that a complete specification of the point) of surrendering (for little boys who fight, at least) was uttering the word, 'uncle'.

The interesting question in regard to more familiar (and common) speech acts is whether there exists any specific form of linguistic behavior engaging in which is necessary for the correct characterization of that behavior. And this is not to ask whether there exists any kind of behavior which is such that engaging in it
insures (as much as it is ever possible to do so) that one's behavior will result in an act of the sort in question, say promising. The words, 'I promise', appropriately uttered, may indeed be a way of guaranteeing (as much as is possible) that one's behavior results in a promise. But then uttering the words, 'I promise', appropriately, should be viewed as one among a number of ways in which one may secure by means of linguistic behavior the end of having promised, perhaps the best, to be sure, but only one way.

I suspect that whenever engaging in some specific form of linguistic behavior becomes part of the point of some action, when the utterance of some set of specific words (or their utterance with some specific tone of voice or facial expression) is undertaken for its own sake (as in playing a game, one follows the rules for the sake of following them) then that action has become a ritual a ceremony. And I doubt very much if speaking a language, properly so called, should be considered a ritual procedure. At any rate, to concentrate on such ritualized parts of speech (if there are any) is not going to help us gain any understanding of the working's of live speech situations. To suppose that all speech actions are, more or less, of this formulaic sort (a suppositions easy to make, if one is impressed with the analogies between speech and game playing, yet fails to appreciate the role game rules play in game playing) can only serve to mislead us into thinking that a certain form of behavior which is really only the most likely to result in some end, is the only means available for securing that end. And this is, I believe, just the sort of presumption which has led Searle astray.
Now I think we can at least partially account for Searle's mistake by reconsidering his statement of his general hypothesis:

...speaking a language is performing speech acts, acts such as making statements, giving commands, asking questions, making promises, and so on; and more abstractly, acts such as referring and predicating...

We may need to pay special attention to his use of the term, 'act'; there are two rather obvious and different ways in which we use this term. As we shall see, Searle in speaking of speech acts is using 'act' in the first of these two ways, when, in my opinion, if we are to speak of speech in terms of acts, it should be used in the second way.

We sometimes use the term, 'act', in the sense in which it is used in speaking of circus acts. We speak of putting on an act, acting as if one were on a stage. In this sense of 'act', we perform acts by making our behavior conform, as well as we can, to a certain specifiable procedure or pattern of behavior, a procedure detailed in a script or on our program for the evening or perhaps by a ringmaster's announcement. Of course, it is not necessary that an audience have advance knowledge of the procedure to be followed, but only that there is such a procedure. When one goes to see a comedian perform his act, one does not know (unless one has seen the show before) just what jokes he will tell or in what order, but one does know that there is a specifiable sequence he will follow, or try to follow, depending upon the audience's general reaction. The salient characteristic of this sense of 'act', which I shall call the circus act sense, is simply that there is (or is presumed to be) a specifiable procedure which the agent who is to perform the act will try as best he can to live up to, so to speak, in actually performing the act.
To ask whether someone's act was successful in the circus act sense of 'act', presupposes that there was a procedure which the actor was at least trying to follow. To ask if the acrobat successfully performed his act last night is not to ask whether he gave a performance. If he didn't show up and try to go through his routine then the question of success is mooted. We might say that a success condition on the act of asking whether the acrobat successfully performed his act is that the acrobat tried to run through his routine or procedure.

The second sense of 'act' which I have in mind is that sense in which we use the term in speaking of acts of courage or kindness. In this sense of 'act', there just is no specified or specifiable procedure or routine which someone must have followed or tried to follow in performing an act of, e.g., courage. In speaking of an act of courage, as opposed to a circus act, we are not speaking of some procedure to which an agent in performing such an act may be said to be trying to make his behavior conform. One can't change one's act of courage, but one can change one's circus act. There are all sorts of different things an agent may do (different kinds of behavior he may engage in) in order to perform an act of courage. But in every case he must have by his behavior placed himself in some (real or imagined) danger of which he was aware. The salient characteristic of this sense of 'act', which I shall call the act of courage sense, is simply that the behavior characterized must have had a certain result whether or not the agent was trying to achieve that result. To ask whether someone's act was successful in the act of courage sense of act, is to presuppose, not that there is any specifiable procedure which the actor was at least trying to follow, but rather
that the actor's behavior did have a certain result. What an actor may have been trying to do in behaving as he did may not be a relevant factor in determining whether he performed an act of, e.g., courage. If he was trying to save a life, he may succeed or fail, and still have performed an act of courage.

Now it should be rather obvious that in speaking of speech acts, Searle is using the term, 'act', in what I have called the circus act sense. Arguments aside, there is an unintuitive ring about such a point of view. If we ask, 'What was he doing?', and get the reply, 'Performing speech acts.', or (straining a bit), 'Performing acts of speech', what are we to think? Was he play acting, acting out a part, playing a role, performing tricks? Of course not, he was only speaking. Yet taking Searle's point of view, putting on his blinkers, would seem to make such questions at least appropriate. If we ask, 'What was he doing?', and get the reply, 'Performing an act of promising', the circus act point of view would have us assume that there is some procedure which, in the opinion of the respondant, the agent's linguistic behavior has conformed to, and which the agent was trying to make his behavior conform to. We may say without too much strain, that the procedure or routine to be performed in performing a circus act, constitutes that act. So that if one wants to know what the act is, if one wants an analysis of the act, as it were, one can expect the reply to have the form of a specification of the procedure to be followed and the conditions under which it is to be followed.

If we set out to discover the conditions which are "...necessary and sufficient for the act of promising to have been successfully and non-defectively performed..." (SA, p. 54) as Searle expressly
has, and we presume that speech acts are like circus acts, it
must seem quite natural to assume that there is some specifiable
procedure, albeit a linguistic procedure, which the agent must have
at least attempted. What we shall discover will be conditions on
correctly characterizing the actor's behavior, the procedure he in fact
followed, as a performance of his act. The characterizing term or ex-
pression will be, to that extent, descriptive of the agent's behavior.
If we characterize what he did as, e.g., make a promise, then, under
the circus act presumption, we can infer that he behaved in a certain
specifiable way (or limited range of ways).

One might think that merely pointing out, as Strawson has in
"Intention and Convention in Speech Acts", that for a host of speech
acts there seems to be no specifiable procedure that one must have
made one's own behavior conform to in order for one's behavior to
be correctly characterized as, e.g., a warning, would be enough to
discredit this circus act viewpoint. Clearly, different sets of words
if the circumstances are right, may be used to warn and the same set
may be in one situation a warning, but in another, say, a threat.
To suppose otherwise

...would be like supposing that here could be no love affairs
which did not proceed on lines laid down in the Roman de la Rose
or that every dispute between men must follow the pattern
specified in Touchstone's speech about the countercheck
quarrelsome and the lie direct.²

What Strawson is pointing out is that for most speech acts
there is no overt ritual or procedure that must be performed. But
what he has not counted on is the strength of the scientific prejudice.
If there is no specifiable overt procedure then, this prejudice demands
that there be some covert procedure, as it were. When it is correct
to characterize someone's utterance as a warning even though the speaker did not preface his utterance with the words, 'I warn you that', there must have been (if one is wedded to the scientific) some more subtile form of behavior that the speaker must have made his behavior conform to.

As Searle would say, the speaker in such cases must have produced his utterance with a certain intonation, or stress or even facial expression. That is to say, he must have used some force indicating device in producing the utterance he did, or else the characterization in question would not be accepted as correct.

Now it is my contention that it is a mistake to insist upon the circus act view of speech acts, that doing so is just what has led Searle to overlook or misconstrue the nature of our use of institutional terms. If we think of speech acts as like acts of courage then the temptation to view the characterization of linguistic behavior as a rather special sort of description should at least lessen in intensity. It is because the characterization of some behavior as an act of courage gives no indication of just what the agent did, but rather is the expression of a judgment or verdict that the agent's behavior resulted in his placing himself in some (real or imagined) danger of which he was aware, that I have chosen it as my paradigm.

When I say that the agent's behavior resulted in his placing himself in some (real or imagined) danger, I am not specifying a consequential effect of his behavior. I might do that by noting whether he saved the little girl's life, or became a world famous hero, or died. And I am not necessarily specifying what he tried to do. He might have been trying to do his duty (as a lifeguard),
or trying to show off, any number of things. In characterizing his behavior as an act of courage, I am characterizing his behavior in terms of a result which I believe it had. A specification of that result is, in a sense, an analysis of, an elucidation of, the concept of courage.

We may say that to characterize someone's behavior as a promise is to express a decision to the effect that that behavior resulted in a promise. We make promises, and statements, ask questions, give replies by means of our linguistic behavior. To specify what the result is in the case of promising, for example, is to analyze or elucidate the concept of promising. And to do so is not to specify a consequential effect of some behavior, nor is to specify what a speaker must be trying to do by means of his behavior. As Austin note, "...to promise is not to try to do anything." And it is, after all, the behavior that is characterized as a promise and not any of its effects.

What is important to note is that to characterize some behavior as a promise is to express a decision to the effect that that behavior had a certain result, one that we indicate by characterizing it as a promise. To explain why we think that some behavior is correctly characterized as a promise is to give our reasons for thinking that that behavior succeeded in being a promise, resulted in a promise.

The reasons we may give on an occasion will not define the concept of promising for us, but they will give us some idea of when to apply it. And from that we can get some idea of what is involved in promising. I don't believe we can ever get any set of necessary and sufficient conditions for promising, but we can learn how to promise. Learning what a promise is, is like learning what an act of courage is. New and different situations constantly arise in which
we may characterize someone's behavior in those situations as a promise or an act of courage, and we learn from those situations. We learn to appreciate different kinds of courage and different kinds of promising (swearing, vowing, oath taking).

To begin to get an idea of just how open the concept of courage or of promising or of any speech act is we have only to consider the reasons which might make us hesitate over or question the correctness of or feel something odd about characterizing some behavior as an act of courage or as, for example (and to change my example), an act of warning. What we shall discover are conditions to guard against if one wants one's characterization to be correct; conditions that if they obtain may result in one's characterization being rejected as incorrect or mistaken.

Let us suppose that the agent whose behavior is characterized as courageous holds an office or position such that it is his duty to act courageously, e.g., suppose he is a lifeguard at the beach. He has been especially trained and especially equipped to deal with situations in which he must put himself in danger. Did he perform an act of courage in swimming out to rescue the little girl? He's done it numerous times, of course there's always some danger and he must be aware of it, but...an act of courage? Perhaps not. Or let's suppose our agent to be a glory hound. He didn't care at all about the little girl, he plunged in after her for the acclaim he would receive. He did save her at some risk to his own life, but he didn't have the appropriate thoughts, feelings, or intentions, as Austin would say. Or suppose the little girl didn't want to be saved. She was not about to drown or she was attempting suicide or it was a rehearsal
of Java, and the agent knew it. Do we have an act of courage or just foolishness. Just how praiseworthy must an agent's intentions be, and just how great must the danger involved be and how thorough must his appreciation of the danger be before we have an act of courage, before it is correct to characterize the behavior as an act of courage?

Now let us consider when we might feel odd about, or hesitate over characterizing some behavior as, for example, a warning. Suppose we thank a friend for warning us that so and so gets violent at the mention of Wittgenstein's name and hear our friend disclaim any intention to warn at all - he was merely commenting on a curious idiosyncracy of so and so. Nevertheless, he did put us on our guard against a potentially embarrassing, if not dangerous, situation.

Suppose I tell my companion that the water's cold knowing that he dislikes cold water but he doesn't believe me or doesn't take me seriously and plunges in. If he returns and accuses me of not caring about his welfare, can I reply (correctly) with the words, 'But I did warn you.'? If my companion has a hear condition and plunges in under the same conditions but dies as a result of the shock, can I excuse myself to someone who accuses me of not caring for his welfare by saying 'But I warned him'? And what shall we say about my reply in the last two cases if I am a lifeguard at the beach where my two friends had their respective encounters with the cold water?

In the first case above, what seems out of order, as it were, is the lack of an intention on my friends part to put me on guard, to alert me to the danger involved in mentioning Wittgenstein's name to so and so. The difference in the next two cases seems to be a difference in the importance of the consequences suffered by my two friends,
discomfort as opposed to death. In neither of the last two cases, did I make any special effort to insure that my friends were put on their guard against the cold water, though we may suppose that I intended to warn them in both cases. In the first case we may very well conclude that I did warn him, my reply to his accusation was in order after all. But in the second I doubt whether my reply was correct - death is too important. And in these cases reconsidered under the assumption that I am a lifeguard, if I did nothing to insure that my intentions to warn the man with the heart condition were recognized by him for what they were, I'm sure that my reply to the accusation of his relatives would not be in order.

In all of these cases, the characterization of my linguistic behavior as a warning seems to depend upon the belief (mistaken or not) that my behavior succeeded in being an expression of the intention to put my audience on their guard. The result which my linguistic behavior is (believed) to have had in virtue of which my behavior was (mistakenly or not) characterized as a warning, is simply that my behavior, whatever it was, succeeded in being an expression of an intention to alert my audience to some (real or imagined) danger of which I believed they were unaware. And we should note that in none of the cases did I do anything to insure that my friends recognized my linguistic behavior for what I intended it to be, a warning. There are linguistic devices that I could have used to insure, as much as one can, that my behavior was recognized as an expression of the intention to warn (prefacing my remarks with the words, 'I warn you that', for example). But only in that case where the consequences of ignoring my warning, or failing to recognize
my intention to warn, was death, and particularly when I was a lifeguard
whose duty it is to guard against such a consequence, was I held res-
pponsible for not using such an insuring device. The point is that only
in special circumstances, when the consequences are important enough
or when one holds an office which makes it one's duty to do so,
is the use of some linguistic device, conformity to some specifiable
(circus act) procedure, necessary for the correctness of the
characterization in question.

We can now I think see more clearly than ever just why Searle
should insist on including in the explanans of any linguistic explanation
a specification of some particular linguistic behavior or procedure.
He is apparently interested in formulating rule following which will
insure or guarantee as much as possible that one's behavior will succeed
in being a speech act of the sort named by the institutional term in
question. Perhaps this is how we should take those remarks of his
where he indicates that in his investigation of promising he is
"...going to deal only with a simple and idealized case." (SA, p. 56)
He makes it a point to note that his "...analysis will be directed at
the center of the concept of promising. I am ignoring marginal,
fringe, and partially defective promises...Furthermore in the analysis
I confine my discussion to full blown explicit promises and ignor
promises made by elliptical turns of phrase, hints, metaphors, etc..." (SA, p. 56) Perhaps if he had paid more attention to non-idealized
promises he would have recognized the rules he formulates for what
they are - rules specifying the best strategy available for insuring
that one's linguistic behavior results in a promise.

Now the purpose of this particular section has been to lay the
ground work for a proper account of the relation between explanans and
explanandum in a linguistic explanation by diagnosing in as much
detail as possible (what I believe to be) the mistake Searle has made
which gives my arguments in the preceding chapter their purchase so
to speak. As we recall Searle would have us account for that relation
by attributing to a speaker who accepts the explanans as reason
enough for the characterization in question, tacit knowledge of a
linguistic generalization which may be formulated in the form of a
constitutive rule. Such a generalization will specify a particular
kind of behavior and the conditions under which that behavior is
required (or forbidden or permitted). So the reasons offered on
an occasion for some characterization are (supposedly) accepted as
explaining the correctness of that characterization on the authority
of a generalization the overt formulation of which, together with the
reasons offered, will provide a deductive warrant for the characterization
in question. I have argued that it is a mistake to think that the re-
lation between reason and characterization that accounts for our accep-
tance of the reasons as explaining the characterization may be specified
in the form of a generalization defining for the characterization in
question. I have suggested that linguistic characterizations are not
descriptive of any kind of behavior but are rather expressions of
verdicts or decisions to the effect that the behavior in question has
succeeded in securing the point or has resulted in the end indicated
by the institutional term used to characterize the behavior in question.
I have not attempted (except in the case of warning, and then in a
rather rough and ready fashion) to specify the result or end indicated
by any institutional term, i.e., I have not attempted an analysis of
the point or end indicated by institutional terms. I have been content
to make the point that such terms are used to indicate results or ends or points. As a result I have often spoken of the characterization of some linguistic behavior as the expression of a decision to the effect that that behavior has succeeded in being a promise or warning, for example. No circularity is involved here since no attempt has been made to analyze the concepts of promising or warning. My point has been merely to illustrate that the reasons offered for such a characterization should be regarded as specifications of the means used on the occasion in question to achieve or secure (in the opinion of the characterizer) the end or point or result indicated by the characterizing term.

What we have to do now is to specify the relation between reason and characterization in an acceptable linguistic explanation in such a way that that specification will at least reflect or allow for or account for the following three characteristics of such explanations.

1) The specification of the relation will have to provide the basis for an appraisal of the characterization as a good one to make in the circumstances.

2) The specification of the relation will have to account for the fact that linguistic characterizations are made in terms that indicate ends rather than means, the point or result of behavior rather than the behavior itself.

3) The specification of the relation will have to allow for the fact that the source of authority behind the acceptance of the reason as explaining the characterization in question is not that provided by a generalization defining for the characterization in question.
Now I believe that all three of these characteristics are exemplified by what Searle has called regulative rule formulations. That is, I believe that the specification of the relation between reason and characterization in a linguistic explanation may be formulated as a regulative rule. This means that I am agreeing with Searle's general hypothesis - that speaking a language is a rule governed activity or rather, that it is best viewed as a rule governed activity. Only I believe that the rule to be formulated is a regulative rule. We may take this belief of mine as an hypothesis which will be (at least, partially) confirmed if it can be shown that regulative rule formulations do indeed have these three characteristics. Just what effect this hypothesis if confirmed will have upon our conception of explanation, at least, of explanation of human behavior, we shall have to see. I shall have a word to say on the subject in the concluding section of this chapter.

However, now that we have some idea of where we are going I want to digress a moment and make good on a claim I earlier made. I want to suggest that linguistic behavior is not the only kind of behavior that may be explained in terms of regulative rules. I shall argue in the next section that the correctness of characterizations of behavior in terms that may be broadly called psychological may also be so explained. This digression should not come as too great a surprise. The attribution of an ability (in particular, the ability to speak and understand a language) is clearly a characterization of an agent or an agent's behavior in psychological terms. Indeed, the argument I shall offer in the next section will be one that does not differ essentially from the argument I offered in chapter four. I shall in effect try
to argue that the attribution of a psychological predicate, e.g., 'is angry', is no more a description of the agent than is the attribution of an ability.

**Psychological Predicates**

As I indicated I intend to argue in this section that the attribution of a psychological state (typically, one indicated by the predicate, 'is angry') should no more be presumed to be a description of the agent in question than should the attribution of an ability (in particular one indicated by the predicate, 'can V'). The arguments I shall give will be abbreviated versions of the arguments already presented in chapter four. I have chosen as my stalking horse some of the things J. L. Austin has to say on this subject in his article, "Other Minds". I am picking on Austin not because what he has to say is so terribly wrong, but instead, because what he has to say is, in my opinion, very close to being right. Also as we shall see in the next section, some of what Austin says, or rather what I take him to be saying, will prove very instructive in getting clear on the nature of the explanatory strategy forced on us by my claim that the correctness of such characterizations as those expressed in the form, 'S can V', and 'S is angry', may be explained in terms of a relation between reason and characterization that is best viewed as the formulation of a regulative rule.

Austin was primarily concerned in "Other Minds" with discovering the conditions under which one may correctly claim to know the truth of "...a statement of particular, current, empirical fact..."14 And secondarily, (though perhaps most importantly) with those conditions
under which one may correctly claim to know "...the thoughts, feelings, sensations, minds &c. of another creature..." (OM, p. 76) His strategy throughout the article is to discover the various ways the question, "How do you know?", asked of each of the two statements he takes as paradigms of the above two concerns, i.e., "There's a bittern at the bottom of the garden.", and "He is angry", may be answered. The point of such a strategy is this. Each answer that seems appropriate should uncover one of those conditions, the satisfaction of which, entitle one to claim to know, and which if unsatisfied, leave one open to the reproachful reposte, "Then you don't know any such thing." (OM, p. 78)

In other words (my words), such a strategy will uncover the reasons normally, or most probably acceptable in defense of the correctness of a characterization of a state of the garden or a state of mind, or emotion.

Obviously, I shall in what follows be concerned only with what Austin has to say about those cases when we are entitled to claim to know that someone is angry. Specifically, I shall be concerned with two of the answers he gives to, "How do you know?", asked of someone who claims, "He is angry." The type of conditions which the two answers I have in mind may be said to exemplify are these:

1) The familiarity condition; that

...a great deal depends on how familiar we have been in our past with this type of person, and indeed with this individual, in this type of situation. If we have no great familiarity, then we hesitate to say we know: indeed, we can't be expected to say (tell). (OM, p. 104)

2) the truth condition; that 'is angry'

...is a description of a whole pattern of events including occasion, symptoms, feeling and manifestation, and possible other factors besides. (OM, p. 109)
Now it is my opinion that psychological predicates like, 'is angry', are not used to describe anything, let alone a "whole pattern of events". I believe that they are used to express verdicts made for reasons which may be expressed in the form of descriptions, where the reason is related to the verdict or characterization in a way that may be specified in the form of a regulative rule, rather than in the form of a generalization defining the characterization in terms of the description of any pattern of events. I think it a mistake to suppose the only difference between our use of psychological predicates like, 'is angry' and such predicates as 'is at the bottom of the garden', to be one of degree. Psychological predicates differ from descriptive predicates in a more radical way than simply being more vague, ill defined and covering a more complex and varied situation. They are used to express verdicts not to describe, contrary to what Austin seems to be saying.

All words for emotions are, besides, on the vague side, in two ways leading to further hesitations about whether we know when he's angry. They tend to cover a rather wide and ill defined variety of situations; and the patterns they cover tend to be each of them, rather complex (though common and so not difficult to recognize, very often), so that it is easy for one of the more or less necessary features to be omitted and thus to give rise to hesitation about what exactly we should say in such an unorthodox case. (OM, p. 110)

I believe that if Austin had just paid more attention to his own theory of truth he would have realized that satisfaction of the truth condition, ((2) above), does not require that one presume that there is any pattern of events, however ill defined that 'is angry' is used to describe, but rather that there is a description of a pattern of events that is acceptable as a good reason for characterizing someone as angry on an occasion.
I believe that he is led astray because when he comes to discuss the truth condition he assumes that the familiarity condition is satisfied. Indeed, if 'is angry' is descriptive of a whole pattern of events, it is not clear at all what need there is for satisfaction of the familiarity conditions. If 'is angry' is descriptive of a pattern of events, however vague, one ought to be able to tell whether someone is correctly characterized as angry, even if one is not familiar with the person (or type of person) in question.

This seems, of course, just false and that it is provides a prima facia reason for discounting Austin's claim that 'is angry' is descriptive. Austin himself notes that people differ so much that without prior familiarity with the person characterized we certainly can't claim to know that he is angry.

...the feelings of some individual whom you have never met before - they might be almost anything - you don't know his character at all or his tastes, you have had no experience of his mannerisms, and so on. His feelings are elusive and personal: people differ so much. (OM, p. 104)

If one is not familiar with nor in the presence of the agent characterized as angry, what information about his behavior or the pattern of events of which he is a part, has one been provided? Could one, upon being informed that A is angry, pick A out of a crowd of unangry men? Someone unfamiliar with A and not in A's presence ought to be provided with the same information about A or his situation, upon being informed that B is angry. But that is just not the case, people differ too much.

If 'is angry' is descriptive then, just as with the predicate, 'is red', it ought to be normally inappropriate or odd to remark to someone in the presence of A that A is angry. It is rather odd to have someone remark that that chair is red, when one is standing right
in front of it. But we very often need to be told that someone is angry even though we are then and there conversing with him. There is nothing odd about such a characterization, one doesn't look for a Gricean implicature in such cases. But one does cast about for some alternative explanation of why one should be told that the chair one can see quite well at the time is red.

If we may suppose, following Austin's comments in "Truth" that satisfying condition (2) amounts to recognizing the pattern token of events demonstratively correlated with such an utterance as, 'A is angry', as like the pattern type of events descriptively correlated with 'is angry', then we may ask what marks off an anger-pattern-type from an irritation-pattern-type from a rage-pattern-type? Parenthetically, it should be noted that it must be the predicate, 'is angry' that is descriptively correlated with the pattern-type in question, and not the whole utterance, 'A is angry'. For if it is the whole utterance that is so correlated then A must be a part of the pattern-type correlated with the predicate, 'is angry'. And this will not do at all when we consider utterances having the names of other people as their subject terms. Or if one supposes that different pattern-types are correlated with different uses of 'is angry', then either one shall have to suppose that 'is angry' has as many different uses as there are people or types of people, or that each use has, after all, something in common, which something is just whatever pattern-type is descriptively correlated with 'is angry'. The former alternative seems not very plausible (to say the least) and is clearly at odds with any presumption that 'is angry' is descriptive. The latter is just where previous question comes in: what, if any, is the fundamentum
differentiae, so to speak, among anger, rage and irritation (to mention a few) pattern types?

One cannot appeal to the individual whose name figures as subject term in utterances of the sort, 'A is angry', for pattern-types can involve no particular individual. One cannot appeal to other people and see if they agree or not, for that would presume the others to have answered the question. One cannot appeal to memory since all that one does remember is the pattern-type and which pattern-type is which is just the question, i.e., memories, no more than sensa come tags on, as Austin would say. One cannot discover the differences by investigating pattern-tokens for that would be to put the cart before the horse. Which pattern-tokens one would investigate can be determined only by first deciding which are of a type with which pattern-type.

I want to suggest that determining whether the truth condition is satisfied in any particular case is just not a matter of comparing pattern-tokens with pattern-types, or rather that if it is a matter of comparison, it is not a matter of a token just being like a type. The pattern-token must be sufficiently like the type in question and as Austin notes, "...for a statement to be true one state of affairs must be like certain others, which is a natural relation, but also sufficiently like to merit the same 'description', which is no longer a natural relation." (T, p. 122)

I want to suggest that to the extent that the reason offered in behalf of psychological characterization on an occasion just is a specification of some pattern-token, that reason is accepted as explaining the characterization on the basis of a regulative rule. The formulation of the rule will involve the specification of a
pattern-type which the pattern-token in question is (in one's opinion) like and naturally like. But the choice of regulative rule formulated as the authority behind the characterization is one made on the basis of one's familiarity with the agent or type of agent characterized as, e.g., angry. Whether a reason is accepted as a good one for some characterization depends upon whether and to what extent one is accepted as an authority on the moods, feelings, emotions of the agent characterized. And being accepted as such an authority is just to be accepted as familiar with the agent characterized. And I want to suggest that being familiar with someone seems to vary directly with how many psychological predicates like, 'is angry', one is in a position to characterize that agent in terms of. The description of an agent's behavior we may offer as a reason for characterizing that agent as angry is not (part of) a definition of 'is angry', but is rather a reason offered as a good one on the strength of our authority as experts on the moods, feelings, and emotions of the agent characterized. That such an utterance as, 'A is angry', does inform or describe, as it sometimes seems to, is due to the fact that one is familiar with the agent characterized (or the type of agent characterized). From the fact that we learn to use such predicates as 'is angry' by taking note of those occasions when older and wiser heads use the predicate it does not immediately follow (indeed, I think the above arguments establish that it doesn't follow at all) that 'is angry' is used to describe something, some pattern of events, in common with all those occasions of use. 'Is angry' is used to express a verdict or decision made for reasons that are accepted as good ones on the basis of one's familiarity with the agent characterized
(and his situation). And it must be recalled that this suggestion is just the one I made at the conclusion of chapter four regarding our use of predicates attributing abilities, e.g., 'can shoot', or 'can speak and understand a language.' The relation between reason and characterization in virtue of which the reason is accepted as explaining the characterization is, in those cases where the characterization is a psychological one, just the same as that relation where the characterization is a linguistic one. I indicated before beginning this digression that I believed that the best way to specify this relation is in the form of a regulative rule. In the next section I shall give my reasons why I think so.

Regulative Rules

Just before launching the above digression I noted three characteristics of the relation between reason and characterization in a linguistic (and I think I can now say as well, a psychological) explanation. In order to make a start at giving a theory of explanation (alternative to the scientific) we must find a way to specify this relation that will clearly allow for or account for these three characteristics.

1) The specification of the relation will have to account for the fact that linguistic characterizations are made in terms that indicate ends rather than means, the point or result of behavior rather than the behavior itself.

2) The specification of the relation will have to provide the basis for an appraisal of the characterization as a good one to make in the circumstances.

3) The specification of the relation will have to allow for the
fact that the source of authority behind the acceptance of the reason as explaining the characterization in question is not that provided by a generalization defining for the characterization.

I claimed that if we take the specification of the relation to be the formulation of a regulative rule then all three of these characteristics will be accounted for. Since this claim is offered as an hypothesis, we may begin to confirm it by getting a good idea of the general characteristics of a regulative rule. Searle himself has made a good beginning in this direction, even though his purpose is to argue that regulative rules are not the sort of rule a formulation of which can be used to account for this relation. He notes that:

Regulative rules regulate antecedently or independently existing forms of behavior,...a pre-existing activity, an activity whose existence is logically independent of the rules. (SA, p. 33)

He cites as an example of such an activity, fishing. He remarks that the formulation of fishing rules, as it were, will be specifications of techniques, procedures and even strategies that facilitate or enable one to achieve one's goal or end, namely that of catching a fish. (SA, p. 37) But he disqualifies this type of rule, i.e., strategy rules, as the type to be formulated in explaining our acceptance of some reasons as explaining linguistic (and psychological) characterizations partly on the grounds that the strategies formulated are, or are dependent upon, "...matters of natural fact; such facts, for example, as that fish sometimes bite at worms but very seldom at empty fish hooks..."
(SA, p. 37)

We may say then, that regulative rules specify means-end relations where the means specified is to be followed not for its own sake alone, but for the end which it is believed will be the result of employing the
means specified. One point of regulative rules seems to be, then, expedition. They enable a novice to avoid a dreary and perhaps frustrating periods of trial and error in discovering the procedures (strategies) that will most easily and perhaps most efficiently result in the end in question in the various circumstances in which the end is desired. Regulative rule formulations provide one with knowledge or information one might acquire on one's own, if the end is one of sufficient importance in one's life for one to diligently persist in trying to achieve it.

So we may say that regulative rule formulations exhibit at least these two characteristics.

a) They specify means-end relations, where the means specified are to be followed for the sake of achieving the end in question, rather than for the sake of engaging in the means regardless of whether the end is achieved or not.

b) They specify means-end relations, where the means specified is (believed to be) the most likely or most probably in the circumstances procedure to follow in order to achieve or secure the end.

Now each of these two characteristics stands in need of some further elaboration. Searle takes the first, (a), to imply this.

...behavior which is in accordance with the rule could be given the same description or specification (the same answer to the question 'What did he do?') whether or not the rule existed provided the description or specification makes no explicit reference to the rule. (SA, p. 35)

Supposedly, a non-fisherman may be able to specify what a fisherman did on an occasion as catch a fish, even though, being a non-fisherman, he hasn't the faintest idea how it was one, i.e., even though he knows nothing of the means, the strategy, which may be employed to achieve
that end. In other words, Searle thinks that (a) above implies that one may know when, under what conditions, some behavior may be characterized in terms that indicate the end of that activity (catching a fish, in this example) even though one may know nothing of the means used, nor even that any means at all were used. If the point of employing some means is merely the attainment of some end, and does not involve the idea of employing them for their own sake then (according to Searle, apparently) the ability to characterize the behavior in question in terms of the end achieved does not require knowledge of the means or of any means characteristically employed to achieve that end.

I think Searle is mistaken in this. If we are told that someone caught a fish the other day, it's true that we don't know or haven't been told anything in particular about how he caught it. We can't say exactly what means he used to catch the fish. But if we understand this characterization of his behavior at all, we do know that the fish he caught must have been at one time swimming around in a body of water and that somehow he got it out of that water. We do know that he must have used some means, a hook and line, bow and arrow, spear, his hands, a net, perhaps even his teeth. To the extent that regulative rule formulations specify strategies, good or bad, effective or time consuming, as the circumstances dictate, so that there is in every case more than one, we can't, without further information, tell just what particular strategy was followed, given knowledge of the end achieved. But if we know what kind of fish, salt water or fresh for example, or some of the circumstances under which he caught the fish, in a rod and reel contest, or who the fisherman was, an
aborigine or the bow and arrow enthusiast down the street, we can make a pretty good guess.

I want to suggest that if one couldn't make such a guess, however bad it may turn out to be, then one wouldn't in the first place have any idea of what catching a fish is, one couldn't after all understand such a characterization of someone's behavior. One wouldn't know when under what conditions, some activity may be characterized as catching a fish. Even were one on the spot when the fisherman pulled his fish out of the water, one just wouldn't know that he had just caught a fish. He might be supposed to be taking it for a walk for example.

I think we can say without further ado, that characteristic (a) of regulative rule formulations compares quite favorably with the first characteristic above which a specification of the relation between reason and characterization in a linguistic explanation must exhibit. They obviously do account for the fact that linguistic characterizations are made in terms that indicate ends, the result of behavior rather than the behavior itself. If we are told that someone made a promise, we can't tell just exactly what he did that (in the reporter's opinion) resulted in a promise. But we know he must have done something. He needn't have prefaced his utterance (if he vocalized at all) with the words, 'I promise', but, since we know that is the usual, and indeed, the most effective way to go about promising, we can make a pretty good guess that is what he did. and even if he didn't utter the words, 'I promise', but we know something about the circumstances and the reporter, we might conclude that he has decided that a promise was made on the basis of a simple declarative utterance, e.g., "I will marry you".
And the various procedures that are available to us for promising are procedures that we could learn to follow by trial and error, by trying to promise and having our efforts criticized by our teachers. We learn what a promise is as much by learning how to promise, by learning the various strategies and when to apply them, as we do by being informed of a rule specifying one or another of these strategies. Indeed informing one of these strategies is a way of teaching, but a way that does not simply stipulate or attempt to define, what a promise is. Communication is, after all, a tremendously important end in our lives, one that we will diligently pursue through out our lives. There is never a point in anyone life when he can say, 'There, I have learned all there is to know. Now I know how to speak and understand a language.' One is constantly learning new strategies and new ways to apply old ones. Poetry and slang are on a par when it comes to introducing new strategies for old ends and indeed even for introducing new ends. Making out what someone means, a poet, one's lover, the hippy kid down the block, is a matter of recognizing new ways to achieve familiar ends and even to broaden one's appreciation of these old ends to the point where it seems a new and different concept has entered one's life.

Now let us turn to characteristic (b) of regulative rules - that they specify means-end relations, where the means specified is (believed to be) the most likely or most probable (in the circumstances in which they are formulated, if they are) procedure to follow in order to achieve or secure some end. Searle takes this characteristic to imply (and in this I agree with him) that regulative rule formulations "...often provide the basis for appraisals of behavior, e.g., "He was
rude", "He was immoral", "He was polite"." (SA, p. 35) The 
procedure that is believed to be (by the formulator, at least) a good 
one to follow in the circumstances in order to achieve the end in 
question. To that extent, the procedure specified is being recommended 
or urged on one as advisable or worthy of confidence or implementation. 
The point of the recommended procedure, indeed the point of the 
recommendation itself, is the successful accomplishment of the end in 
the circumstances.

Should one fly in the face of the advice (if it is given), so 
to speak, or should one engage in a mode of behavior that is not 
as likely or as probable to result in the end desired, then because 
of that disregard (or ignorance) one's behavior on the occasion in 
question, will be subject to appraisal in terms that indicate how 
well or badly the behavior conforms to the recommended behavior. 
We have some terms of appraisal that are more or less unique to a 
certain end, e.g., 'politeness' and 'rudeness' are reserved pretty 
much for social behavior. But we also have terms of appraisal that 
seem to range over a host of activities, e.g., 'brilliant', 'stupid', 
'clever', 'unusual', even 'good', 'bad', 'skillful', 'awkward'.

Such terms are applied on the basis of a regulative rule 
specifying a particular form of behavior and thereby recommending 
it as a means to the end desired in the circumstances at hand. 
Commendation or condemnation of the actual behavior on an occasion 
(expressed variously by means of one or another of the sort of terms 
of appraisal above, or in other ways) may be considered the result of 
a decision regarding how well or badly the actual behavior conforms 
to the form of behavior specified in a regulative rule supposed to
apply in the circumstances in which the actual behavior is perpetrated. Such appraisals may be debated not only on the grounds that the actual behavior accords better than one thinks to recommended behavior, but also on the grounds that the rule on account of which the kind of behavior in question is recommended, does not apply in the circumstances.

We wouldn't appraise the behavior of a starving man at the dinner table as rude or impolite unless we accepted in effect the rule of etiquette specifying, and so recommending, dinner table conduct, to be a good one to follow even in such circumstances as these - the man was starving. We may say that regulative rules do not always apply even though, as in the case of the starving man sitting down to eat and gobbling away, the prevailing conditions seem to provide a prima facia case for invoking or formulating such a rule as grounds for the appraisal in question. Appraising someone's behavior on the basis of some regulative rule involves not only deciding how well or badly that behavior conforms to the type of behavior specified in the rule, but also, and perhaps more importantly, deciding whether the rule applies in the first place.

I think we may say that this characteristic (b), of regulative rule formulations compares favorably with the second characteristic listed above which the specification of the relation between reason and characterization in a linguistic explanation must exhibit - that the specification of the relation will have to provide the basis for an appraisal of the characterization as a good one to make in the circumstances. If we characterize someone's behavior on an occasion as making a promise and that characterization is challenged, we may reply by offering as a reason that the agent prefaced his utterance
with the words, 'I promise'. This decision we have made will be accepted as a good one to make only if one accepts as applicable in the circumstances a rule to the effect that prefiguring one's remarks with the words, 'I promise', is a good strategy to follow in order to make a promise. The reason offered specifies the strategy followed on the occasion which, in the opinion of the characterizer, resulted in a promise. Other things being equal, that reason will be accepted as a good one and so the characterization will be accepted as correct, just in case one accepts as applicable in the circumstances a regulative rule specifying that strategy as a means to the end of promising.

Now the decision regarding whether some regulative rule applies in some particular circumstances may be viewed as a decision regarding whether that rule has the authority it is alleged to have in those circumstances. To cite a regulative rule as a reason for some appraisal is to appeal to such a rule as the source of authority for the appraisal. To appraise a linguistic characterization as correct (or a good decision to make in the circumstances) on the basis of some regulative rule specifying the reason offered on behalf of the characterization as a good reason in the circumstances, is to rest one's case so to speak, on the authority of the rule. The reasons offered are not related to the characterization as definiens to definiendum, so the authority of the rule cited cannot have as its source the authority provided by modus ponens. That is, the relation between reason and characterization in virtue of which the reason is accepted as explaining the correctness of the characterization on an occasion, cannot be supposed to be that of a valid deductive argument in which the generalization
logically relating reason and characterization has been suppressed. Our acceptance of a reason as explaining a characterization must be based upon our acceptance of the authority of a regulative rule specifying that reason as a good one in the circumstances. There are, I believe three sources of authority for regulative rules, official rule-books, experts and customs. So we may say that our acceptance of a regulative rule formulation as a reason for our appraisal of some characterization as correct, rests in our acceptance of the source of the rule formulated as an authoritative source.

As an example of regulative rule formulations which have as their source of authority an official rule book, we may take the rules of football as they appear in the official NFL rule book. As an example of regulative rule formulations which have as their source of authority an expert, we may take the pronouncements of a master craftsman made in trying to impart a little of his wisdom to an apprentice. And as an example of regulative rule formulations which have as their source of authority custom, we may take the rules of etiquette.

One may in any case reject (refuse to accept as correct) some characterization of behavior made in institutional terms on the grounds that the source of the rule formulated as the authority for the appraisal (as correct) is not in the circumstances as authoritative source, i.e., one may refuse to accept as correct some characterization on the grounds that the rule formulated is not applicable in the circumstances. Four men on a side are playing football, even though the NFL rule book specifies eleven on a side. If one has only know Smith for a few hours, one may not be enough of an expert on Smith's emotional states to have
One's characterization of him as angry accepted on the basis of that twitch. The table manners of starving men may be neither polite nor impolite because starvation is not a customary state (at least, not in polite society).

Now it seems to me that this characteristic of regulative rules (that their authority to decide the correctness of some characterization depends upon one's acceptance of their source as authoritative in the circumstances of the characterization) accords well with the third characteristic of the relation between reason and characterization in a linguistic (or psychological) explanation - that the source of authority behind the acceptance of the reason as explaining the characterization in question is not that provided by a generalization defining for the characterization. The question, of course, is, if I am right, wherein lies the source of authority for the regulative rules one may cite in defense of an appraisal of a linguistic (or psychological characterization as correct? I have no really satisfying answer to this question. Grammar school texts tell us not to use singular verbs like, 'was', with plural subjects like, 'they', but we know that they were going on a trip even though we heard the words, 'They was going on a trip'. 'I promise' prefaced to some remark predicing a future action of the speaker, usually indicates that a promise has been made, but one can't promise to fall in love. People who can speak and understand their language know these things and many, many more. People who can speak and understand a language are possessed of an expertise, we are skilled in the way a craftsman is, and there are among us those who are more skilled than others. The correctness of linguistic (and psychological) characterizations is essentially debatable,
but the debate can in any case be settled. It is a matter of getting one's audience to understand what one means. There are certainly rules but they are regulative, strategic in nature. They record and recommend procedures to follow to make one's meaning clear. C. H. Whitely puts this point more eloquently than I can.

...as interpreters of speech we operate the rules with discretion, modifying them when necessary for the sake of our primary aim, which is to get at the sense which the speaker wants to convey... Now the difference between sense and nonsense is the difference between that which can be understood and that which cannot. To say of a sentence that it makes sense is to say that it has been or will be or could be understood by some interpreter or class of interpreters; and to say of it that it makes nonsense is to say the opposite. But if this is the case, then no set of rules can arbitrarily determine the difference between sense and nonsense... People cannot be commanded to understand or not to understand; for this command could not be obeyed, even if it was sensible to obey it... And whether a given person or class of persons will succeed in making sense of it cannot be determined a priori, but is an empirical matter... Of course interpretation is a rule governed activity, and of course if is dependent on knowledge of conventions; but the connection between sense and rule following is contingent... Making out what people mean is not the sort of mechanical operation that a computer could perform. Nor is working out the best way of making one's meaning clear to other people, as philosophers of all men should be aware... Rules of meaning... enable a person to convey his meaning to others, and to understand what others are trying to convey to him. They are valid in so far as they assist him in doing these things but no further. 6

Science and Linguistic Explanation

This concluding section will be brief and to the point. Most of my words in this essay have been spent in an attempt to establish that the scientific explanatory strategy (as laid out in the first two chapters) is just not the right and proper strategy to follow in explaining how it is possible for us to speak and understand a language. In this last chapter I have tried to outline (at best) an alternative explanatory program, one that rests on the very reasons for the inadequacy of the scientific. I have concluded that the right and proper way
to specify the relation between reason and characterization in a linguistic (or psychological) explanation, one that will account for the explanatory force, so to speak, of such explanations is in the form of a regulative rule. This proposal of mine must be taken in the spirit in which it has been offered, as an hypothesis, or better yet, as a prolegomena for any future theorizing.

What we have learned (what I have suggested) is that it is a mistake to think (with scientific theorists) that one can specify what a speaker knows (who knows how to speak and understand a language) in the form of generalizations defining for the linguistic (or psychological) characterizations we are able to make. We have learned that a specification of what a speaker knows must be made in the form of regulative rules. We may liken this knowledge to the knowledge possessed by the master craftsman (or if the speaker happens to be a child, perhaps to the knowledge possessed by his apprentice). What he knows are strategies for certain ends. And the more varied is his knowledge of strategies and the more sophisticated is his knowledge of ends, the more able he is to speak and understand a language.

Even in formulating a speaker's knowledge in terms of regulative rules we must remain aware that such a specification will always leave something out - the expertise of the craftsman, the skill and appreciation of good work which cannot be imparted mechanically by means of rules of any sort. Michael Oakeshott in "Rationalism in Politics" calls the kind of knowledge specifiable in rules, technical knowledge and he calls knowledge of the craftsman in virtue of which he is a craftsman practical knowledge.
Technical knowledge...is susceptible of formulation in rules, principles, directions, maxims...It is possible to write down technical knowledge in a book...On the other hand it is a characteristic of practical knowledge that it is not susceptible of formulation of this kind. It's normal expression is in a customary or traditional way of doing things, or simply, in practice...It is, indeed, a knowledge that is expressed in taste or connoisseurship, lacking rigidity and ready for the impress of the mind of the learner...Technical knowledge can be learned by...heart, repeated by rote, and applied mechanically: The logic of the syllogism is a technique of this kind...On the other hand, practical knowledge can neither be taught nor learned, but only imparted and acquired. It exists only in practice, and the only way to acquire it is by apprenticeship to a master - not because the master can teach it (he cannot) but because it can be acquired only by continuous contact with one who is perpetually practising it."

I must profess in closing a certain misgiving over ending this essay on this note. I am after all offering a prolegomena for any future theorizing which seems to deny the very possibility of any such theorizing. But there is a way to mollify this feeling of discontent. In the opening chapter of this essay I briefly noted a contemporary dispute in the philosophy of science between what I called (following Braithwaite) realists and modellists. The debate centered around the question: How is it possible for us to understand the use of a theoretic language? Modellists maintain that this understanding is acquired as a result of analogies recognized or asserted as holding between the use of theoretic terms and the use of terms antecedently understood. Realists, on the other hand, maintain that such analogies or metaphors are, at best, useful heuristic devices. What makes it possible for us to acquire an understanding of a theoretic language is the same thing, as it were, that makes it possible for us to acquire an understanding of any language, in particular our first language, namely a certain ability we possess, whether innate or itself acquired. So if one wants to know how it is possible for us to understand the use of a theoretic language (the realists maintain) the way to find
out is to find out how it is possible for us to speak and understand a natural language.

Now it has been my overriding contention throughout this essay that the deductive-nomological explanatory strategy practiced (or recommended) by a large segment of theorists interested in explaining our ability to speak a natural language will, at best misrepresent, and at worst simply fail to adequately explain, this ability. One lesson to be learned from this essay (though it has not been my primary concern to preach it) is that the modellists are, after all is said and done, on the right track. If an adequate account of explanation in science rests on an adequate explanation of our ability to understand (new) theoretic languages, and such an explanation is to be provided by an explanation of our ability to speak and understand a natural language, then the results of this essay may be taken in support of the modellist contention that

...the deductive model of explanation should be modified and supplemented by a view of theoretic explanation as metaphoric redescription of the domain of the explanandum.8

I think it will be appropriate to conclude this essay with a few words elaborating on this result. One of the undercurrents of this essay may be rather flippantly expressed in the adage - the proper study of Mankind, is man. The proper study of Explanation (in science, as well as in the philosophy of language) is explanation. We must look to the activity of explanation as it is engaged in by the practitioners of the form of behavior in question, if we are to arrive at a coherent and adequate theory of Explanation. Those who are interested in formulating a theory of Explanation-in-science must look to the working scientist and to the history of science for their data. Those
interested in formulating a theory of Linguistic-Explanation must look to the native speaker, qua native speaker, for theirs.

In the critical chapters of this essay (Four and Five) I argued in effect that the deductive model of explanation (abstracted from the practice of scientists by Hempel and his like) just does not provide an adequate model of the activity of explaining linguistic characterizations (nor psychological characterizations, for that matter) as such activity is engaged in by speakers of a natural language. It is not too surprising (to me, at least) to find that this same abstract model of explanation has been found to be also inadequate to the task of modeling the explanatory practice of working (and historical) scientists. Though it certainly is laudable to attempt to abstract from the activity of explanation in a discipline some common elements, i.e., some properties apparently in common from context to context and scientist (or practitioner) to scientist, it seems to me to be a mistake to take too seriously any such distinction between, e.g., explanation in the abstract and the human activity of explanation. Scotus would have called such a distinction a distinctio formalis a parte rei, a formal distinction among 'things' which is real, as it were, but which 'things' not even God (in any possible world) can separate, really separate. For example, Scotus held that there is a formal distinction between the sensitive soul and the rational soul, but that not even God can separate the two for the result on either hand would not be a human soul. I think that while there is a formal distinction between explanation in the abstract and the activity of explaining, any attempt to separate the two in our philosophical practice and pursue one to the exclusion of the other cannot result in an explanation.
There is a corresponding formal distinction to be made involving the notion of an argument. There is argument in the abstract - complex abstract entities composed of a series of propositions (or well formed formula, sentences) each of which is either self-evidently true (tautological or axiomatic) or can be validly deduced from self-evidently true propositions (or from propositions accepted as axiomatic for the purposes of the argument). And there is the activity of arguing, of convincing, persuading, of finding a common conceptual ground upon which to stand in order to argue. In practice, arguments are assessed (judged) good or bad on the basis of just how convincing, or acceptable they are in the circumstances, and not merely on the basis of their validity. They proceed from premisses that express a point of view, a set of experiences which the participants in the activity of arguing (hopefully) share or have in common or can all agree to or accept. Argument and explanation in the abstract contribute to the activities of arguing and explaining, just as geometric proofs may be said to contribute to the activity of surveying. They have a role to play in such activities but we must not think that what makes an argument acceptable or an explanation either, is merely conformity to the patterns or forms we have marked off as argument or explanation in the abstract.

Indeed, the notion of form seems to exhibit the same kind of ambiguity that the notions of argument and explanation do. We speak of observing the formalities of protocol, of issuing formal demands, or making formal apologies or proposals. We speak of good form or proper form on the football field, or in court or in conducting an experiment in the lab. In short, we recognize different kinds of
human activities, different forms of life that must be known and appreciated, at least minimally, in order for protagonists to engage in the activity of arguing or explaining when the subject matter concerns such activities.

I have in this last chapter proposed as an alternative model of the activity of linguistic explanation one composed, as it were, of regulative rule formulations. Such rule formulations (seem to me at least) to model the relation between reason and characterization in the activity of linguistic explanation far more accurately than does the abstract covering law model of Hempel and his like. Now just what consequences does this proposal of mine have for any attempt to provide a model of the activity of explanation in science? Though I haven't the expertise to say very much here, this much seems obvious.

Since regulative rule formulations specify (speaking broadly) strategies for the achievement of desired results, the ability to speak and understand a natural language must be viewed as (should be viewed as) analogous to the kind of ability possessed by the master craftsman, the kind of ability acquisition of which turns a novice into a master. It rests on one's exposure to and appreciation of, the procedures and techniques of one's peers and teachers. It is an ability to recognize and appreciate analogies, similarities and differences among the linguistic (and psychological) behavior of oneself and others. If I am right and our ability to speak and understand a natural language rests on the ability to recognize and appreciate strategies of various sorts then we should expect to find in the history and practice of science that new theories, new theoretic languages, are developed and understood in a similar way.
Just as it is (in my view) essential for an understanding of how we are able to speak and understand a natural language that we more fully than ever before take seriously the role of strategy in speech, an understanding of how scientists are able to develop and understand a theoretic language requires that we take equally seriously the role of strategy in the historic progress (and process) of science. One major problem for the philosopher of science, as I see it, is to come up with a way of accounting for and talking about strategy in science. In the quote just above, Ms Hesse proposes that theories be viewed as metaphoric models of the phenomena to be explained by the theory. Where I am calling for an investigation of strategy (with its attendant notions of authority and acceptance of authority as a prolegomena for any future account of linguistic explanation, she is calling for an investigation of metaphor as a prolegomena for any future account of explanation in science. She rejects the view of metaphor that would have us treat it as a non-cognitive, subjective, emotive, or stylistic use of language. And she rejects the corresponding view of models in science as purely subjective, psychological and adopted by individuals for private heuristic purposes. She thinks that this view wholly mis-describes the role of models in science. It is her opinion (one that she has extensively argued for in a number of publications) that

...models, like metaphors, are intended to communicate. If some theorist develops a theory in terms of a model, he does not regard it as a private language but presents it as an ingredient of his theory. Neither can he nor need he, make literally explicit all the associations of the model he is exploiting; other workers in the field "catch on" to its intended implications - indeed, they sometimes find the theory unsatisfactory just because some implications the model's originator did not investigate, or even think of, turn out to be empirically false. (MAS, p. 165)
What I would call a theory of strategy and Ms Hesse, a theory of metaphor, P. Feyerabend calls a theory of error. It is his opinion that in addition to the deductive nomological abstraction of Hempel (if not in place of it entirely)

...the scientist who works in a particular historical situation must learn how to recognize error and how to live with it. He needs a theory of error in addition to the "certain and infallible" rules which define the approach to truth...[of Hempel]...

Feyerabend is proposing, in effect, that all fledgling scientists be supplied with a book of error, so to speak, one that will contain

...rules of thumb, useful hints, heuristic suggestions rather than general laws...one that...will relate these hints and these suggestions to historical episodes so that one sees in detail how some of them have led some people to success in some situations....Good books on the art of recognizing and avoiding error will have much in common with good books on the art of singing, or boxing or making love....They contain numerous rules of thumb, useful hints, and they leave it up to the reader to choose what fits his case. (AM, p. 18-19)

In trying to describe what he took to be the necessary frame of mind with which the philosopher of science must approach his subject if he is to acquire any understanding of the nature of explanation in science, Feyerabend provides us with a description of the attitude which (I believe) the philosopher of language must bring to his subject. I shall end with this quote (one must stop somewhere) for I think that it more than adequately expresses my own attitude, one that I have developed over the last three years or so that I have been working on the topics of this essay. It is an attitude I heartily endorse for anyone interested in the philosophy of language.

In the case of science the necessary...[attitude]...can be developed only by means of direct participation (where 'participation' means something different for different individuals) or, if such direct participation cannot be had, or seems undesirable from a study of past episodes in the history of the subject. Considering their great and difficult complexity these episodes must be approached with a novelist's
love for character and for detail, or with a gossip columnist's love for scandal and for surprising turns; they must be approached with insight into the positive function of strength as well as of weakness, of intelligence as well as of stupidity, of love for truth as well as the will to deceive, of modesty as well as of conceit, rather than with the crude and laughably inadequate instruments of the logician. (AM, p. 19)
FOOTNOTES FOR CHAPTER VI


BIBLIOGRAPHY


