

ENCYCLOPAEDIA OF LANGUAGE AND LINGUISTICS



Editor
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Chapter 1
**Philosophical Theory
of Language**

Chapter 1

1.1 To what discipline, exactly, are the arguments of the following chapters intended to contribute? Is it going to be philosophy, or psychology, or linguistics, or some curious and suspect blend of all three? To these straightforward questions we can offer a relatively straightforward answer, though not a very simple one.

It is obvious that children begin by not being able to speak any language at all, and end, barring such accidents as enforced deprivation of social intercourse at maturationally crucial stages of their childhood, by speaking at least one language fairly fluently. One can ask at least two different sorts of questions about this phenomenon. One can ask, on the one hand, "How does it come about that a child learns to speak his native language?" or "What is the psychological mechanism by which it comes about that he learns to speak his native language?"

If psychologists made within their subject the same sort of rough and ready distinction that is commonly made between experimental and theoretical physics, then such questions would clearly be questions for the experimental psychologist. They are causal questions, and answering them would mean adding to our store of knowledge by formulating and testing empirical hypotheses rather than merely imposing some sort of conceptual order or systematization upon existing knowledge.

On the other hand, again, one can ask such questions as "What does an adult speaker's ability to speak a language consist in?" or, what amounts to the same question, "What is it that a child learns in learning to speak his native language?" These questions are conceptual questions. That is, they are not demands for some *facts* about language. We know,

after all, as plain men and (some of us) as linguists, plenty about language. What we don't know is what to make of all the facts we know. We need in some way to make sense of them, to find some order in them: we require a conceptual model.

The construction of a conceptual model is not a matter of spinning armchair fantasies about the workings of nature, founded upon some viciously *a priori* set of assumptions. To begin with it is necessary to establish criteria of adequacy for the sort of model that one intends to attempt to construct. These criteria of adequacy will require in part of any successful model that it be such that explications of a great many diverse phenomena can be given in terms of the model. And they may also require, for example, that the model explicate the phenomena in such a way as to permit them to be made objects of fruitful and nontrivial empirical investigation. Such requirements place strong empirical constraints upon the construction of a conceptual model.

Associationism and stimulus-response theory in psychology are, it seems to me, conceptual models and are widely regarded as providing an essential theoretical basis for the prosecution of experimental studies in psychology. It is evident that a work like Skinner's *Verbal Behavior* is not written to demonstrate a causal concomitance between observed episodes of operant conditioning and the observed acquisition of linguistic skills. For if it were, then it would consist largely of observational reports and results of surveys and experiments, instead of consisting, as it does, of discursive argument of a generally philosophical character. Moreover, Skinner expressly disavows a concern with observation and experiment and explicitly states his intent in the book as that of showing that the phenomena of verbal behavior can be explicated in terms of the conceptual vocabulary of operant conditioning. We can see, moreover, that if Skinner were trying to establish a causal concomitance of the above type, his work would be,

while important, far less important than *Verbal Behavior* would be if it successfully demonstrated the truth of its conclusions. For if experimental work were to establish beyond reasonable doubt that nothing essential happens to a child in the process of learning its mother tongue, which is not strictly analogous to what happens to a rat learning to press the bar in a Skinner box, we would still be no nearer to *explaining* how language is learned. We would have discovered an extremely puzzling natural concomitance; and we would be in much the same position as the one we would be in were we to discover that a child kept in solitary confinement developed the power to talk, in a quite normal way, provided it was plunged in cold water once a day. In other words, we would have discovered *that* a certain event or set of events *E* is a sufficient and necessary condition for the development of a capacity *C*, but we would as yet have no idea *why* *E* is a sufficient and necessary condition for the development of *C*. *Verbal Behavior* sets out to do more than barely postulate the existence of a natural concomitance of that sort (that could be done on half a sheet of note paper and would in any case be pointless unless we had some theoretical reason for expecting such a concomitance to exist). It sets out to establish theoretical connections between operant conditioning and language, such that if we discovered *that* exposure to operant conditioning was in fact a sufficient and necessary condition for the acquisition of language we could reasonably claim to know also *why* it was a sufficient and necessary condition (to know, that is, not only certain genetic facts about language learning, but the rationale of language learning). We are engaged on a similar enterprise. Whether one calls this enterprise philosophy, or philosophical psychology, or theoretical psychology is, to my mind, merely a matter of taste.

The ETL, it seems to me, is a mixture of conceptual model and empirical hypothesis. That is, it offers us simultaneously an account of what is learned in learning language *ab initio* and an account of the psychological mechanism by

which the learning of language proceeds. The plausibility of the empirical hypothesis depends upon the adequacy of the conceptual model: it is only if the ETL theorist is correct in his claim that all of the phenomena of language can be represented in terms of his favored conceptual vocabulary of association, inductive abstraction, ostensive definition, and so on, that it becomes plausible to suppose that children learn their native language by being trained to associate phonemic string with sensory stimulation patterned in ways carefully determined by the linguistic community to which they belong.

What I have tried to show so far is that the conceptual model provided by the ETL is wholly inadequate. It is supposed to offer a nonintuitive explication of a whole range of concepts which are normally explained by appeal to linguistic intuition, of which the concepts of meaning and of concatenation (the central problem here is to explain the ways in which the meanings of sentences depends both on the meanings of their component words and on their syntactic and morphological structure) are the most important and the ones on which the theory concentrates. I have tried to show that the conceptual vocabulary by reference to which the ETL proposes to explicate these linguistic concepts is simply irrelevant to what it is supposed to explicate. For whenever the ETL theorist proposes such an explication (of naming in terms of ostensive definition, for example, or of syntactic function in terms of the patterning of the sensory material associated with differently structured sentences), we find that we must either postulate a purely magical connection between *explicans* and *explicandum*—that is to say, convert the ETL theorist's proposal from a conceptual model to a rather odd empirical hypothesis and interpret him as claiming for example, that it is just a matter of empirical fact that the practice of ostensive definition with small children leads (somehow or other, in some fashion not explained by the theory) to their coming to grasp the concept of a name—or else, in order to make the conn-

ection between *explicans* and *explicandum* intelligible, if only on a trivial level, we must covertly assume that the child in question already grasps, in some form or other, the very concepts which the ETL theorist's account is supposed to be explicating.

The vacuity of the ETL as conceptual model thus vitiates the ETL as empirical hypothesis. For once the illusion of understanding which suggested the empirical hypothesis in question has been dispelled, it is of no more intrinsic interest or plausibility than any quite arbitrary hypothesis that we might frame: for example, the hypothesis that there is a causal connection between learning to talk and eating plenty of fruit.

1.2. So much for the critical and negative portions of the preceding chapters. We have also laid the foundations of a conceptual model different from that offered by the ETL, and we must now ask what sort of model this is, and what are the criteria of adequacy which it must satisfy. As should be now be obvious, we are not in the least concerned to advance *a priori* theories about the psychology of language acquisition. Rather, we wish to discover a mode of representation of what is acquired. We assume that the verbal behavior of an adult speaker of a language *L* is the result of the application of some set of learned conventional rules—the rules of *L*—to the infinitely variable combinations of circumstances in which the adult speaker may produce significant and appropriate utterances. We assume that these rules are a finite set since it is possible to master a language in a finite space of time: once fluent in his native language a man may develop stylistically, but he cannot thereby be said to be learning more about the rules of his language, any more than a chess player who improves his endgame may thereby be said to have improved his knowledge of the rules of chess.

We assume, moreover, that the content of the set of rules of *L* remains virtually constant from speaker to speaker, since

otherwise L would not be a single language.

We wish now to represent the general form of the rules of a language L , in such a way that we can see clearly how we might set about filling in the details of this formal outline so as to arrive at the actual rules of a particular language.

We are trying to construct a theory of linguistic competence in the sense defined by certain linguists, including Chomsky and others. A theory of competence represents as a system of rules some aspect of an adult speaker's linguistic competence—for example, his syntactic competence or his phonological competence. Such a theory is not to be confused with a theory of performance. A theory of performance would tell us the mechanism by which a speaker produces utterances which are original, significant, and appropriate to their circumstances of utterance. Let us call the neurological mechanism which governs this activity the *language machine*. A theory of competence, now, tells us only that the language machine must contain as a component either some specified set of rules S or some formally different set S_1 which is equivalent to S in that the operations of S_1 produce exactly the same linguistic results as the operations of S . But, even if we have separate theories of competence for syntax, semantics, and phonology, there still remain the problems of how the content of these theories is (1) represented and (2) related in the language machine. Both (1) and (2) are obviously aspects of the same problem: it is a problem which is relatively trivial where it concerns the relationship between syntax and phonology, but extremely difficult where it concerns the relationship between syntax and phonology, on the one hand, and semantic on the other.

The first criterion of adequacy for our model is that it should solve this latter problem. We are concerned, that is, with elaborating a theory of the form and content of the rules which govern the production of significant and appropriate utterances: thus, with a theory of total linguistic competence,

in its semantic as well as its syntactic and phonological aspects. This yields two quite severe empirical constraints upon a successful model of this type: it must prove capable of representing in some form or other the content of an adequate competence theory of syntax, and it must supply convincing interpretations of the known facts about children's learning of syntax.

In connection with the latter requirement we must obviously not elaborate systems of rules of types which are clearly, or even probably, beyond the capacity of children to learn. Preferably our rule systems should presuppose only the capacity to execute general types of performance which human beings are known to execute in extralinguistic contexts, but it will not be necessary to confine ourselves to types of performance which animals are known to execute, or to types of performance whose psychological or physiological mechanism is well understood.

It would be impossible to fulfill these rather exacting requirements completely without constructing a more detailed model than I am in a position to construct or could be contained within a book of this length; but I shall try to show in Chapters 6 that a theory of linguistic devices seems at least in principle to be capable of fulfilling them.

A further obvious criterion of adequacy which we must meet is that our model must explicate a number of concepts having to do with language, and normally explained by appeal to linguistic intuition, without itself covertly assuming any understanding of these concepts or making any appeal to intuition. The concepts in question include those of a name, of meaning, of synonymy, of a sentence, and of syntactic relationship (the relationship which unites the component words of a sentence into a sentence), of a word, of semantic vagueness, of metaphor, and of what is sometimes called semantic category by philosophers and semantic anomaly by linguists. We shall not altogether exhaust this list in the

succeeding chapters, and it could be lengthened.

1.3. Someone might object as follows: "Is language *really* a system of rules of any type, let alone the type you propose? Are you not really doing armchair empirical psychology, whatever you say to the contrary, since you propose to tell us *what a child learns, in the process of learning language*, but to do so not on the basis of detailed observation of children but rather on the basis of a purely theoretical argument? And isn't the point at which your argument becomes viciously a *priori* precisely the point at which you assume that an adult's linguistic behavior is the result of 'applying' to something (to his experiences, to his circumstances, to the world as it appears to him?) some system of rules?"

I think what we have already said stands in part as an answer to this objection, but something more needs to be said. A model is not an assertion about reality. It is merely, a demonstration that reality can be systematically represented according to a certain schematism. The reason why I have been careful to stress the status of the assumption that what different speakers of a language have in common is a grasp of the same system of rules is precisely that I wish to do justice to this distinction. Because of this distinction it is quite inappropriate to ask whether a conceptual model is *true* or whether it offers the *correct representation* of the phenomena to which it applies. For no doubt it may be possible to systematically represent any set of phenomena in terms of several, or perhaps even on occasion many, different conceptual schemata. Thus, we can represent a volume of heated gas as a congeries of particles, as a pattern of convection currents, and in other ways. It is not sensible to ask which of these is the correct way, but it is sensible to ask whether each of them is of interest, and why.

Whether a given conceptual model, *qua* demonstration of the possibility of subjecting a given set of phenomena to a given schematism, is of interest, depends on, among other

things, whether it is empirically fruitful. The representation of language as a system of rules has been conspicuously fruitful in linguistics and to some extent also in philosophy; and therefore, it seems worth trying to carry this line of inquiry further in order to see whether it may not be possible to construct a unified theory of linguistic description. And I think it is reasonable to expect that the successful construction of such a theory would make possible a more fruitful intercourse between linguistics, empirical psychology, and perhaps also certain forms of philosophical analysis. Some psychologists have recently turned their attention to the study of children's syntax, proceeding on the assumption that the syntax of children, like that of their elders, can be represented as a system of rules; and it is not unlikely that similar empirical studies could be pursued with semantics, if we could see how to represent the semantics of a language in terms of systems of rules. In this connection it is worth noticing that a linguistic device is a species of *plan*, as that word is defined by Miller, Galanter, and Pribram in their book *Plans and the Structure of Behavior*.