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THE INDETERMINACY OF TRANSLATION AND THE REGIMENTATION OF LANGUAGE
IN THE PHILOSOPHY OF W. V. QUINE

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THE INDETERMINACY OF TRANSLATION AND THE REGIMENTATION OF LANGUAGE
IN THE PHILOSOPHY OF W. V. QUINE

A Thesis
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CHAPTER I

INTRODUCTION TO THE PRINCIPLE OF INDETERMINACY OF TRANSLATION

A. Sources of the Principle

Willard Van Orman Quine's principle of the indeterminacy of translation, outlined most fully in *Word and Object*, represents the convergence of his acceptance of Skinnerian behaviorism and his rejection of an absolute analytic-synthetic distinction. Both his acceptance of behaviorism and his conclusions about the lack of defining criteria for analyticity are sources of the principle of indeterminacy of translation. A critique of indeterminacy must question the strictness of Quine's behaviorism or the soundness of the argument in "Two Dogmas of Empiricism." It is, therefore, important to examine both sources.

Indeterminacy is a consequence of the "empirically unconditioned variation in one's conceptual scheme." The "net empirical import" of a single sentence, if any, is the "stimulus meaning", if any, of the sentence. Commentators have rightly recognized the concept of stimulus meaning as the crucial one in the exposition. It is defined as "the class of all the stimulations...that would prompt" assent or dissent "for a speaker at a date" to a sentence. Quine acknowledges that "stimulus meaning as defined falls short in various ways of one's intuitive demands on 'meaning' as undefined," but he maintains that it is nevertheless the "objective reality" available to the linguist. Quine's advocacy of "stimulus meaning" is based on his views of (1) the process of language learning; (2) the end product: the learned language; (3) admissible evidence for the linguist.
(1) Quine accepts, rather uncritically, Skinner's analysis of learning a language as being conditioned to give and expect predictable responses to particular stimuli. He does not go to great lengths to defend his acceptance of Skinner's approach. He mentions in passing that Skinner "is not without his critics," and with a bare reference without comment to Chomsky's attack on Skinner's main work in the field of language, Verbal Behavior, concludes, "But at worst we may suppose that the description, besides being conveniently definite, is substantially true of a good part of what goes into the first learning of words."

In a work where very little is taken for granted, Quine is content to start with the assumption that "words mean only as their use in sentences is conditioned to sensory stimuli, verbal and otherwise."

Understanding the sentences uttered by others and constructing one's own sentences are possible only because one has been conditioned by stimulus-response bonds to do so. The history of each speaker's conditioning to meanings is unique, and yet, in a speech community, all are speakers of a common language. Since there is nothing more to language learning than being conditioned publicly by mature speakers of the language, the existence of a common language in the face of this "chaotic personal diversity of connections" appears paradoxical. This situation provides Quine with a hook on which to hang the principle of indeterminacy. As speakers of a common language, we are like the bushes trimmed to look like elephants (of all things) which achieve this remarkable, uniform result despite the different arrangements of twigs in each case.
Quine uses the Skinnerian account not only to explain "the first learning of words" but to explain the entire learning of a language, and in so doing he accepts the "stretched" use of concepts from entirely nonlinguistic psychological experiments to account for the most complex features of language. He also includes features of linguistic development which do not seem to be justified by the theory (although Skinner does the same). For example, he holds that, beyond the whole sentences learned directly by stimulus-response conditioning, some sentences are constructed "from learned parts" on "analogy" to the directly conditioned ones. How one learns to draw an analogy — to recognize a relevant similarity — is a process very much in need of clarification which neither Skinner nor Quine gives. This might be said to be the fundamental question that a linguistic theory must answer in order to account for a speaker's ability to construct and understand previously unheard sentences.

Chomsky tends to deprecate a reliance on "analogy", but this is to make his own explanation for the "creativity" of speakers appear the only possible one. Let us grant that the ability to draw analogies is a basic intellectual skill, without which we would be hard pressed to account for language or for any knowledge whatsoever. The question to be asked is what relation the development and exercise of this ability have to the proposed theory of stimulus-response conditioning. It looks suspiciously as if "analogy" were inserted to allow for a sufficiently rich language although it has no foundation in the learning theory.

Initially the stimuli involved are nonverbal, or nonverbal together with verbal, but in order to achieve a language which is more
than the "fancifully fancyless medium of unvarnished news," provision must be made for sentences to act as stimuli for other sentences, the so-called "interanimation of sentences." One cannot account for scientific theories, or even for an enriched everyday language about things in the world, without a "sullying of the stream" of experience by constructs and concepts. This must surely be granted, but Quine also wants to hold that even these relations between sentences are established "by the mechanism of conditioned response." For this to be the case in a nonmetaphorical sense, learning would have to be strictly by rote because the sentences in a stimulus-response relationship would have to be always the same, word for word.

There is nothing in a straightforward account of conditioned response to explain how one learns to understand that any one of a large group of differently worded, but cognitively equivalent, utterances calls for one of another large group of differently worded, but cognitively equivalent, responses in a particular situation. The fact that this is so raises those troublesome questions about synonymy which are so neatly avoided by evoking conditioned response. A straightforward interpretation of conditioned response would have to allow that a separate bond must be established between each possible wording of interanimating sentences, involving a troublesome infinity of established conditioned responses for communication to be possible.

(2) Once one has progressed from babbling infant to speaker of a language, it is the "totality of [one's] dispositions to verbal behavior" which are the concern of the linguist. This is, in fact, how Quine defines language. As recently as February, 1967, he wrote, "Lan-
guage is first and foremost a system of dispositions to observable behavior." One does not want to restrict 'language' to the totality of observed verbal behavior, of an individual or a speech community, because of the arbitrary limits of time, speakers, situations, as well as observed chance errors, etc. And yet one wants to be empirical, to speak of verbal behavior, and not of "competence", as Chomsky does, which is the "mental reality underlying actual behavior."

Moreover, dispositions, as we have been assured by Carnap, Goodman, and in Word and Object by Quine as well, are really nothing to worry about. Despite their kinship with counterfactual conditionals, they are quite harmless. For example, the "disposition to assent to or dissent from" a sentence, which is part of the definition of stimulus meaning, is "no worse" than 'x is soluble in water' and that is not bad at all. "The disposition involved in stimulus meaning may be presumed to be some subtle structural condition, like an allergy and like solubility; like an allergy, more particularly, in not being understood." Again in section 46, when Quine is "explicating" dispositions, he posits "subvisible structure" for 'soluble' and "some subtle neural condition, induced by language-learning" for the disposition to assent or dissent, and thus eliminates the subjunctive mood from expansions of expressions containing these dispositional predicates. But this "presumption" of unobservables is convenient but not necessary: "we are familiar enough in a general way with how one sets about guessing, from judicious tests and samples and observed uniformities, whether there is a disposition of a specified sort."

How does one test, however, for a disposition to verbal behavior?
We needn't test at all: we know all human beings have it. (By the same reasoning, one could establish an innate capacity for speech in humans.)

The totality of dispositions to verbal behavior? That is everything that a speaker has said, will say, could say, and would say. We can "presume" to attribute the totality of dispositions, structurally, to "principles of neural organization", but that phrase, strangely enough, is Chomsky's, arch-foe of empiricism. One might indeed wonder what is so empirical about the "totality of dispositions" except that it is an extension of particular dispositions, which can be empirically tested for. At any rate, for Quine, a language is a system of dispositions to verbal behavior, and stimulus meanings are a subclass of dispositions in a language.

(3) In "Two Dogmas of Empiricism," Quine argues against the verification theory of meaning by arguing that "in taking the statement as unit [for confirmation, disconfirmation by experience] we have drawn our grid too finely. The unit of empirical significance is the whole of science." He rejects the empirical-simpleness of verificationism, and the holism of scientific theory, which he espouses in "Two Dogmas," reappears, only slightly qualified, in Word and Object.

However, in that later work, with which we are here concerned, he writes, in a section titled "Evidence," "words mean only as their use in sentences is conditioned to sensory stimuli, verbal and otherwise. Any realistic theory of evidence must be inseparable from the psychology of stimulus and response, applied to sentences." If one allows Quine to treat whole theories as "verbal stimuli", the first sentence quoted is no great departure. We have seen, above, that there are serious difficulties in-
involved in treating the "interanimation of sentences" as conditioned responses, but that is not at issue here. The second sentence quoted appears to take single sentences as units, and, as it turns out later in the exposition, the necessity of doing so — that is, of taking the sentence as the unit of meaning in a language — reveals the depth of the problem of meaning in a language and its dissimilarity to the problem of truth in science.

The linguist’s evidence consists of sentences, one by one, as stimuli and as responses, and of nonverbal stimuli. A scientific theory does not stand or fall on confirming or disconfirming "sensory evidence" for single statements, even when these are predictions arising from the theory. Standards of coherence, simplicity, and centrality of or to the system must also be met, and may override sensory evidence. Linguistic theories are "worse off" than scientific theories because, although both attempt to account for "systems of dispositions" — of physical objects generally and of speakers of a language — "sentences are thought of as conveying meanings severally" while "statements about the external world face the tribunal of sense experience not individually but only as a corporate body."

If sentences must be assigned meanings one by one, what is the only possible empirically sound 'meaning' of a sentence? Stimulus meaning, answers Quine. Only for stimulus meaning can the observer have reliable evidence, the correlation of verbal and nonverbal stimuli with assenting and dissenting responses. Quine assumes, but does not defend explicitly because he does not doubt, the reliability, under suitable controls, of these correlations. What he questions, and rejects, is that
the sort of hypothesis-confirming/disconfirming evidence that the scientist looks for is "reliable" evidence for the linguist. On the contrary, he holds that the consequences of the linguist's hypotheses "can be defended only through the ... hypotheses, now and forever."

According to the principle of indeterminacy, there is no empirically sound way of determining that two terms, within one language or between two languages, are synonymous because, beyond stimulus synonymy, which is not adequate to the job, an indefinite number of pairings can be made without any defensible way of choosing between them. There is no way, in other words, of singling out those true statements of a language which are true solely by virtue of meanings, rules of the language, linguistic usage, and totally independent of facts of the non-linguistic world. The arguments of parts 1.-4. of "Two Dogmas of Empiricism," which conclude that "a boundary between analytic and synthetic statements simply has not been drawn," are thus an important prop for the approach Quine takes in Word and Object, and it will be useful to review them here.

Quine distinguishes two classes of accepted analytic statements. The first includes those statements which are logical truths (i.e., truth-functional tautologies), true under any interpretation of their nonlogical components. This first class is unproblematic, and philosophically uninteresting. The second class comprises statements which can be transformed into tautologous truths by substitution of synonyms. Thus, 'Brothers are male siblings' can be transformed into the logically true 'Brothers are brothers' if it can be established that 'male sibling' is synonymous with 'brother'. Quine then turns his attention to the relation
of synonymy to see if it can provide support for the attribution of analyticity to statements.

Synonymy is not, as might conveniently be thought, established by definition except in cases of explicitly stipulated definitions which establish synonymy by fiat. Definitions rest on "prior relations of synonymy" discovered by the lexicographer, an "empirical scientist". They are "grounded in usage," and the criterion of synonymy used by the lexicographer "has still to be clarified, presumably in terms relating to linguistic behavior." Quine does no more in this essay than assert that compiling dictionaries and providing for the possibility of translation and paraphrase (intralinguistic translation) is the business of the lexicographer. Just how much of an "empirical scientist" the lexicographer really is becomes an important issue in "The Problem of Meaning in Linguistics" and of central importance in Chapter II of Word and Object. The point made about definition here is merely that a dictionary definition cannot justify transforming a statement which is not a tautology into one which is. Unlike a definition in a logical calculus, a dictionary definition is the end product of an empirical investigation. It is as strong or as weak as the procedure, assumptions, and data which have produced it. It is not prescriptive.

Quine does not mention this, but another weakness of dictionary definitions, overlooked by those who put their faith in them for sorting out truths of language from truths about the world, is that standard (as opposed to ideal) dictionaries do not distinguish "factual" from "purely verbal" components in the definitions given. The Preface to the Concise Oxford Dictionary recognizes this lack of a clear boundary.
The book is designed as a dictionary, and not as an encyclopaedia; that is, the uses of words and phrases as such are its subject matter, and it is concerned with giving information about the things for which those words and phrases stand only so far as correct use of the words depends on knowledge of the things. The degree of this dependence varies greatly with the kind of word treated, the difference between cyclopaedic and dictionary treatment varies with it, and the line of distinction is accordingly a fluctuating and dubious one. 42

The dictionary reader must sort out the verbal from the factual components himself, if he can. Those who rely on dictionary entries to support synonymy implicitly use some further criterion, such as the inconceivability of the contradictory.

The next approach to synonymy is through interchangeability. Granting that any substitution of terms changes something (the inscription itself, psychological or metaphorical associations, etc.), what sort of substitution preserves "cognitive synonym" — whatever that is — and provides a basis for the second class of analytic statements? Quine simplifies the problem by considering as indivisible words compound terms, including terms in quotation marks. Thus, we need not worry that we cannot substitute 'male sibling' for 'brother' in 'brotherhood' or in "'Brother' is a two-syllable word." He does not discuss the problem of distinguishing different senses, or readings, of a term: a 'brother' of a religious order may appear in sentences in which it cannot be considered a compound term, and yet one would not want to require substitutability of putative synonyms with all readings of both terms. Quine also does not discuss here substitution into intentional contexts of believing, looking for, hoping, etc. However, Quine finds reasons to reject the criterion of interchangeability even for the
simpler cases; a fortiori, it will not do for the more problematic ones.

Interchangeability which preserves the truth-value of the statement is found to be too weak as a criterion of synonymy for extensional languages. Terms which have the same extension (i.e., are true of the same entities) include both those pairs apparently related by linguistic conventions only (e.g., 'brother' and 'male sibling') and those pairs related by any facts at all (e.g., 'creature with heart' and 'creature with kidneys'). Such interchangeability cannot be the criterion for a synonymy which supports distinguishing analytic truths (about language) from synthetic truths (about the world).

On the other hand, interchangeability does not serve as an independent criterion in languages which include modal operators (e.g., 'necessarily') because the inclusion of such terms presupposes that meaning has been given to 'analytic'. An attempt to distinguish pairs of interchangeable terms which cannot but be paired in the language in question from pairs which are not, but could be, unrelated, cannot make any headway without the use of concepts intrinsically connected to the concept of analyticity. "Cognitive synonymy" itself appears to suffer from the same malaise: it is the sort of synonymy which preserves analyticity.

At this point, Quine gives up looking for a firm support for analyticity in synonymy, and turns to semantical rules, such as those formulated in artificial languages. He does not consider any treatment of "rules of language" other than for artificial languages. His main conclusion is that constructed languages use 'is analytic' and 'semant-
tical rule' as unanalyzed terms, and cannot therefore illuminate the nature of analyticity. A metalinguistic definition of 'analytic' in a constructed language, which uses the term 'analytic', cannot clarify analyticity. A statement held to be true by a 'semantical rule' likewise does not explicate 'true-due-to-language-only' in a natural language. What are semantical rules in a natural language? In a constructed language, they are not characterized, but only listed under the heading, 'semantical rules'. These comments reveal a sharp difference of opinion between Quine and several commentators on what formalized languages can contribute to the understanding of natural languages.

Martin is a good representative of this other viewpoint, particularly since he addresses himself directly to some of the arguments of "Two Dogmas". Martin asserts that "Quine's demands for a definition of 'analytic in L' in 'L' ranging over the natural languages is exorbitant." A demand for a definition "ranging over all formalized language systems is ... at best premature." Carnap's work has been on languages "having a very simple structure," and the "hope" of such work is that, in time, the definitions given for these simple languages can be "extended". Such explication consists in "making more exact, clear, and precise an older, less clear, less precise concept. ..." "That the new concept is an explication for the older concept is indicated by using the same word for both." To this assertion, which seems to overlook the fact that "using the same word for both" is insufficient to ensure that the new concept is an explication of the older, Martin adds that to call the new concept by a less "tendentious" name, as Quine suggests, since it is an unanalyzed " 'analytic-for-Lo' ", "would be to
miss the point." L-truth uses the term 'analytic' to "indicate" that it is an explication of analytic truth.

To Quine's complaint that he does not know what "semantical rules" are, Martin answers that, in a constructed language, they are definitions in the metalanguage. He implies that Quine is being perverse in not acknowledging this. In answer, Quine has added a paragraph to the essay. He knows what definitions, postulates and axioms are in a logical calculus, and he knows what "semantical rules" are in a formalized language. His point is that, apart from being selected as "semantical rules" for a particular formalized language, there is nothing intrinsically semantical-rule-like about the statements so specified. "Semantical rules" are relative to the purposes and procedures of the languages for which they are formulated. Any statements could be semantical rules in a language. Thus, "no one signalization of a subclass of the truths of L is intrinsically more a semantical rule than another; and, if 'analytic' means 'true by semantical rules', no one truth of L is analytic to the exclusion of another."

Martin's defense of the use of formalized language systems to clarify concepts in natural language has two prongs: (1) one can clarify concepts in a simple structure which resist analysis in a complicated — and not systematically complicated — one; (2) one can, with further knowledge (of an unspecified sort: perhaps of the structure of natural languages? perhaps of formalized languages, comparatively?) extend the results of explication in simple artificial languages to natural language.

With reference to this, consider, as Quine does, the application to natural language of Carnap's explanation of an analytic statement as
one which is true in all state-descriptions. Quine holds that this method is not applicable to natural languages because they contain some mutually dependent pairs of sentences. This can be clearly shown, as follows:

(1) \( (x) (x \text{ is a bachelor}) \equiv -(x \text{ is married}) \) purportedly analytic

(2) \( (x) (x \text{ is a bachelor}) \equiv (x \text{ is married}) \) '\( \equiv \)': both statements, or neither, hold in all state-descriptions

(3) \( a \text{ is a bachelor, } b \text{ is a bachelor} \ldots \)

\( -(a \text{ is married}), -(b \text{ is married}) \ldots \) ' \( (x) \)': all substitution-instances of its scope hold in all state-descriptions

(4) Tom is a bachelor

(5) Tom is married

State-description\(_1\) State-description\(_2\) State-description\(_3\) State-description\(_4\)

\( (l) \text{ is T } \quad (l) \text{ is T } \quad (l) \text{ is F } \quad (l) \text{ is F } \)

\( (5) \text{ is T } \quad (5) \text{ is F } \quad (5) \text{ is T } \quad (5) \text{ is F } \)

State-description\(_2\), in which 'Tom is a bachelor' and 'Tom is married' are both true, is obviously at variance with the conditions (2) and (3) of the analyticity of (1). In natural languages, which contain such "synonym-pairs" as 'bachelor' and 'not married', this method will not pick out the substitution-of-synonyms-type analytic statements.

The above is merely an example of Quine's general point about the limitations of explicating concepts in natural language via simple constructed languages.

Appeal to hypothetical languages of an artificially simple kind could conceivably be useful in clarifying analyticity, if the mental or behavioral or cultural factors relevant to analyticity — whatever they may be — were somehow
sketched into the simplified model. But a model which takes analyticity merely as an irreducible character is unlikely to throw light on the problem of explicating analyticity.

One might say that the failure, which "Two Dogmas" so neatly shows, of the traditional criteria of analyticity has led Quine to his holistic view of knowledge, expressed in various picturesque metaphors: fabric, field of force, arches and blocks. All statements are open to review; all statements can be held "come what may"; all statements have some, at least indirect, relation to experience.

Whether some statements can be asserted to be analytic on the basis of inductively established synonymies is a question to which Quine turns in "The Problem of Meaning in Linguistics" and, in greater detail, in Word and Object. In this undertaking, his results can be criticized by questioning the strictness of his inductive procedures and the stopping-point of the investigation which his behaviorism dictates, as well as by meeting the "Two Dogmas" arguments. Such criticisms will be taken up, below, after we see the consequences of giving up the absolute distinction between analytic and synthetic statements and of determining to give an account of language in terms, exclusively, of observable behavior, as Quine conceives it.

B. Two Expositions of the Principle

It might be interesting to look first at a brief, earlier formulation of indeterminacy of translation which appeared in the essay, "The Problem of Meaning in Linguistics". The situation is similar to that in Chapter II of Word and Object: a linguist finds himself among speakers
of an unknown language whose culture is very different from his own. He observes correlations between "Kalaba" utterances and "the other things that are observed to be happening." He forms hypotheses about these utterances as wholes and tests for them. After "picking up some initial Kalaba vocabulary," he tries breaking whole utterances down into shorter components and correlating these with English words. He tests these correlations "as best he can" and ultimately compiles his Kalaba-English lexicon.

The serious theoretical difficulty with this procedure, as Quine sees it, is that "the relevant features of the situation issuing in a given Kalaba utterance are in large part concealed in the person of the speaker," and by this Quine does not mean only the speaker's unique language-learning history but what he sees his experience "as" because of the structure of his language. The formulation of indeterminacy of translation in this essay is more explicitly Whorfian than that in Word and Object. To read the following statements, and to view this presentation as equivalent, in capsule form, to the one in Word and Object, is to understand why Jonathan Cohen sees Quine as the heir of Whorf:

Theoretically the more important difficulty is that, as Cassirer and Whorf have stressed, there is in principle no separating language from the rest of the world, at least as conceived by the speaker. Basic differences in language are bound up, as likely as not, with differences in the way in which the speakers articulate the world itself into things and properties, time and space, elements, forces, spirits, and so on. It is not clear even in principle that it makes sense to think of words and syntax as varying from language to language while the content stays fixed; yet precisely this fiction is involved in speaking of synonymy, at least as between radically different languages.
What provides the lexicographer with an entering wedge is the fact that there are many basic features of man's ways of conceptualizing their environment...which are common to all cultures.

I have suggested that our lexicographer's obvious first moves in picking up some initial Kalabu vocabulary are at bottom a matter of exploiting the overlap of our cultures. 63

Quine appears to be stressing the cultural factor in language differences and similarities in these passages. However, even his Whorfian starting point takes account of most of the criticisms one can make of Whorf's relativity thesis. Quine does not go so far as to claim that "basic differences in language" determine "differences in the way in which the speakers articulate the world itself into things and properties, [etc.]" He says that these two sets of differences are "bound up" with each other, a much weaker claim. He recognizes that one cannot think of the "content" of an utterance as fixed, independently of the way a given language slices up experience. This is also the essence of Whorf's claim, but Whorf, unlike Quine, does not think it inconsistent to "translate" Hopi locutions at the same time that he is insisting that they cannot be translated, almost as if he proves his point by showing how odd the translations are.

Perhaps most importantly, Quine sees that the undertaking of translating a radically different language, from the very start, "encourages the misconception of meaning as reference" because it begins by pointing (and other ostensive procedures) and then proceeds by resting as much as possible on the "presumably common fund of conceptualization" among human beings. Thus, even if the linguist keeps testing his hypothetical translations, "the clarity of any possible conflict
decreases." Even in the most clearcut, public situations, the terms of the English sentence may simply not pick out the same "relevant features" as the terms of the Kalaba sentence. The English sentence and the Kalaba sentence may both denote 'that, over there' without having the same meanings. The farther one gets from reports on 'that, over there', the more important the conceptual structure of a language is to the translation of a sentence. Part of Whorf's puzzle comes from his failure to distinguish meaning from reference. Perceiving that Hopi enshrines different features of experience as relevant, Whorf concludes, not only that Hopi sentences have different meanings from their putative translations in English, but also that the Hopi experience of the world (i.e., what in fact is sensed) is different from the American-European experience of the world.

The linguist's procedure rests on interchangeability salva veritate plus an attempt to get beyond this thoroughly inadequate standard of synonymy (see above, p. 11) by projecting "himself, with his Indo-European Weltanschauung, into the sandals of his Kalaba informant." His completed lexicon, the result of this method, has more than the usual inductive weaknesses. If this were all, further field work could locate mistranslations and confirm correct ones. But in the case of a lexicon compiled in the absence of a criterion for synonymy, there is "nothing for the lexicographer to be right or wrong about."

For the second formulation of indeterminacy, in *Word and Object*, Quine sets his linguist up in an even more explicitly forbidding situation. The language he is to translate is totally unrelated to his own, or to languages familiar to him; the culture is likewise without any
points of similarity to his own or related cultures, nor have the people had contact with any outsiders. Furthermore, he does not have the services of an informant. It is not a practical case, but a Gedankenexperiment intended to make the conclusions stand out more sharply and with more plausibility — one English-speaking observer and a number of Other-speaking natives. "Radical translation" turns out to involve problems no different in principle from those of translation between closely related languages, even by one bilingual speaker, or of paraphrase or understanding within one language. The purpose of this approach is, first, to overcome resistance to the principle of indeterminacy of translation by demonstrating its plausibility in an extreme case, and then to show that it does not result from cultural peculiarities but from problems with synonymy and with the limits of observability.

The linguist begins by matching stimulations (controlled for clarity, duration, etc., by him) with utterances, taken to be sentences, and from these correlations derives the only empirically sound results in his translation of the language, stimulus meanings of highly observational occasion sentences. He moves from these, on peril of increasing uncertainty, to "results" which are ultimately neither confirmable nor falsifiable.

"Stimulus meaning" is defined for any one speaker, for a sentence, as his disposition to assent or dissent from the sentence when confronted by it in conjunction with one of a class of stimulations at a given time. Two sentences are stimulus-synonymous when they have the same stimulus meaning for a speaker; "socially" stimulus-synonymous
when this correlation holds overwhelmingly throughout the speech community. A sentence is stimulus-analytic when a speaker would assent to it or to nothing, or react with shocked disbelief and sudden doubt of his questioner's grasp of the language to the denial of the sentence. Stimulus-analyticity can also be "socialized" by taking it to apply to those sentences to which almost all members of the speech community would so react.

Stimulus meaning is granted by Quine to fall far short of the traditional concept of meaning. Stimulus synonymy and stimulus analyticity, since they are based on stimulus meaning, also represent only a "behavioristic ersatz" for the concepts of synonymy and analyticity. They represent for Quine how far — and it is not very far — the linguist can go on available (acceptable) evidence. Before sketching the linguist's next steps, beyond the evidence, as it were, we will look at what Quine means by "stimulations" and at the kinds of sentences for which stimulus meaning adequately represents meaning. Perhaps even more important is his rationale for correlating stimulations with sentences rather than with words.

The stimulations are taken to be "repeatable event form[s]", and they are counted as similar, if not identical, for different speakers if the speakers are similarly placed with respect to lighting conditions, distance of objects, etc., on assumptions about "the anatomical resemblance of people." As Quine writes in a later paper, "It is the stimulation at the bodily surface that counts, and not just the objective existence of objects of reference off in the distance, nor yet the events deep inside the body."
Other factors may influence a speaker's assent or dissent in a particular case, such as the duration of the stimulation (too long or too momentary), the speaker's condition, "intrusive information" given by another native speaker on the scene or known through experience by the queried speaker himself (but not by the linguist). Some of these factors can be controlled and compensated for by the linguist, but not all of them. "Intrusive information," particularly when it is widely shared in the community, cannot be reliably stripped away from the disposition to assent or dissent to sentences in the presence of stimulations. This is, of course, a homely illustration of the lack of a boundary between facts about language and facts about the world. Quine writes that "we have made no general experimental sense of a distinction between what goes into a native's learning to apply an expression and what goes into his learning supplementary matters about the objects concerned."

Some kinds of sentences are "less susceptible than others" to the influence of intrusive information. If "occasion" sentences are distinguished from "standing" sentences by the fact that the former command "assent or dissent only as prompted all over again by current stimulation" whereas for the latter assent or dissent may be prompted or may be repeated in the absence of prompting, then "observation" sentences are those occasion sentences "whose stimulus meanings vary none under the influence of collateral information." Observation sentences may be an idealization; at least, however, there are "degrees of observationality" of occasion sentences. Stimulus meaning, according to Quine, is meaning for highly observational occasion sentences.
Pairs of sentences can be matched for stimulus synonymy, and sentences can be tested for stimulus analyticity, even if the sentences are not observation sentences, but these sentences cannot be translated except with the help of "analytical hypotheses". Observation sentences can be translated by their stimulus meanings.

One could ask why the linguist starts by correlating whole utterances with stimulations. The first reason is that, initially, he obviously does not know which are the independent parts of any utterance. It is true that, given differences in conventions about the rise and fall of the voice, and pauses, in different languages, some "whole" utterances may be larger units than single sentences. However, any number of sentences in an utterance can be taken to be one sentence which is a conjunction of the contained sentences. It seems evident that the linguist could make more mistakes by premature segmenting than by taking as a sentence an utterance which is actually two or more sentences. Quine's view of the primacy of sentences is based on a number of different lines of reasoning, which go beyond this practical one.

With respect to learning a language, even one's native language, he believes that words are learned as parts of sentences, or as one-word sentences, and that many early compound sentences are learned as wholes. Sentences, therefore, have a genetic primacy.

Conventions for identifying well-formed sentences are more reliable than those for identifying separate words. With words, one has the problem of words contained in words or bridging words: not only 'bachelor's-button' or 'bachelorhood' but also 'cat' in 'cattle' and
'ice' in 'I scream' (when spoken). There are also numerous "formula" expressions in a language which consist of at least two words habitually joined: e.g., 'How do you do?', 'pure creamery butter', etc. This is not to say that 'how' never exists alone or in other combinations, but, just as 'cat' is concealed in the word 'cattle', 'how' may be thought to be concealed in the "word" 'Howdoyoudo'.

Another reason for the primacy of sentences is found in "The Problem of Meaning in Linguistics". "Primary synonymy" is between fairly long segments of discourse. Sentences, on this view, are the shortest units which could have the same meaning. In some cases, the scope has to be extended to sentence pairs or whole paragraphs. So-called synonymy between words, according to Quine, amounts only to "lame partial synonymy plus stage directions." One only makes the effort with words because the pairs of truly synonymous long segments are "altogether limitless in number and variety," and therefore un-compilable. Dictionaries are a convenience. However, because dictionaries exist, and because we look up "the meaning" of a word in them, we mistakenly come to think that synonymy is a relation between words. Not so.

The most important reason for the primacy of sentences, however, is the one that has already come up in connection with the plight of the linguist in "Meaning in Linguistics". An English and a Kalaba utterance were both correlated with the same public experience, but there was no presumption from that that they meant the same, in a fuller sense of 'meaning' than "stimulus meaning", or even that the terms in each sentence denoted the same entities. As Quine puts it in Word and Object,
"Occasion sentences and stimulus meanings are general coin; terms and reference are local to our conceptual scheme." There is even a suggestion in the passage from which this sentence comes that syntactic features may be so radically different between the two languages that the linguist can do violence to the language he is trying to translate by hypothetically constructing and analyzing sentences from and into "parts". But Quine is mainly talking about the conceptual categories to which terms belong — is 'gavagai', as a term, 'rabbit' or 'rabbit-stage' or 'part of scattered rabbithood', etc. — and he may be thinking of syntax, as Whorf does, in terms of the semantical correlates of syntactic categories.

The inaccessibility of both the extension and intension of terms is the basis of Quine's answer to Carnap's proposal in "Meaning and Synonymy in Natural Language." Carnap has his linguist engage in a series of questions and answers with "Karl" to determine the intensions of terms, after taking it as "granted" that the extensions of terms can be determined. Quine notes that a considerable command of the language is required to carry on this questioning, but that would be a practical matter and a question of time, if it were all. The real flaw in the procedure is that questions which would elicit the intension of a term must be couched in the "provincial" conceptual framework of Karl's language which the linguist does not know — and which, according to the principle of indeterminacy, he cannot get to know even with empirical probability. As he frames his questions in Karl's language, without this knowledge, what the questions mean to Karl is inscrutable to the linguist.
The linguist must ask Karl such questions as "Is that\textsubscript{1} (stimulation of a rabbit) the same as that\textsubscript{2} (stimulation of another rabbit) ?" or "Is that (stimulation of a horse with a horn in the middle of its forehead — a picture) a 'Pferd'?" The reliability of the answers depends entirely on the accuracy of the linguist's hypothetical translation of 'the same as' and 'Pferd'. The same responses which would confirm his hypothetical translations could also confirm other, entirely disparate hypothetical translations, as well as the actual intensions of the terms for the native speaker. If 'the same as' does not mean 'is the same kind of thing as' but 'is part of the same scattered rabbithood as', the responses will be the same, but the linguist, interpreting 'is the same as' as 'is the same kind of thing as', will be wrong in his subsequent translation of 'gavagai'. Likewise, 'Pferd' may not mean 'horse' to the native speaker, but 'that participant in the tribal totem with cloven hooves'.

It is important to see, as Carnap apparently does not, that the extension of a term is as much in question here as its intension. The linguist cannot determine the range of entities to which Karl is willing to apply a term with only "the uncertainty of all inductive inference," because, after the first few responses, he must formulate an hypothesis about the extension and intension of the term in question, and his hypothesis will determine the sorts of stimulations he presents to Karl. His results may therefore show areas of positive, negative and uncertain responses and yet not be a map of the extension of the term.

Imagine a Kalaba-speaking linguist investigating English, who wanted to determine "at least" the extension of the term 'blue'. After
the first few assents and dissents of his informant to presentations of small physical objects, eyes, sky and clothing, he hypothesizes that the sound 'blue' picks out a quality of being not readily edible. With this in mind, he presents a variety of edible, inedible, unappetizing, and possibly edible things to his informant. As long as he works with that hypothesis, he will leave totally out of account certain kinds of testing presentations, and his results will only show the areas of responses within the range of presentations which have been chosen with the hypothesis in mind.

It has not seemed possible to discuss why stimulus meaning is a relation between stimulations and sentences, rather than terms, without bringing in the linguist's hypotheses. "Analytical hypotheses", as Quine calls them, are directly responsible for the indeterminacy of radical translation. Quine does not demonstrate that such hypotheses are not also involved in the determination of stimulus meanings. This requires a brief defense.

After all, one does not know intuitively what signs speakers give, orally or by their behavior or gestures, for assent and dissent. It will take some sort of hypotheses and the testing of them to establish this. The nod, headshake, and shrug are not universal, and the Kalaba linguist may well get them wrongly sorted out on his first try with English speakers. The point about these hypotheses is that they are very limited, and even if the linguist is slow or has bad luck with them, he will soon have tried them all out, with different results (since assent and dissent are contradictory attitudes), and determine which is which. Analytical hypotheses cannot be tested in this way.
The assumption is being made that there is something equivalent to 'Yes' and 'No', in words and/or behavior, in any language, and it might be argued that this is not necessarily so. In that case, the linguist would have to go home prematurely in abject defeat. However, it seems more fruitful to assume that, both for any kind of reasoning, problem-solving, etc., and for survival itself (there has to be a way of giving in when your arm has been twisted long enough), some expressions of assent and dissent are basic — and even conspicuous — in human communities.

From this assumption about assent and dissent, Quine also derives the translatability of truth-functional connectives, 'and', 'or' and 'not'. These can be given behavioral meaning in terms of assent and dissent to sentences combined or negated. Their translatability rests on a further assumption, closely related, however, to the first, that there must be this much implicit logic in any language. One is not imputing the knowledge of logic in some form, or logically consistent behavior most of the time, to members of all societies, however primitive, but merely some means of making distinctions and resisting self-contradiction, again for the sake of survival (which should probably be taken to include problem-solving).

This justification of the universal existence of assent, dissent, and truth functional logic in languages by the need to "survive" is not to be found explicitly in Quine's writings. It is my attempt to state presuppositions of his linguist's starting point which are consistent with Quine's general position. Peter Winch, in an article which argues for attempting to understand "native" beliefs and practices through in-
ternal, rather than externally imposed, criteria, nevertheless explicates the "necessity" of rationality in any language, on somewhat more restricted grounds, as follows:

Where there is language, it must make a difference what is said and this is only possible where the saying of one thing rules out, on pain of failure to communicate, the saying of something else. 96

The question might arise, after this line of argument, whether, in this sense, the higher primates are not also "logical" in their behavior. "Logic" is certainly being defined very broadly here. Furthermore, if one accepts Vygotsky's analysis that, in a child's mental development, there is a stage of pre-linguistic thought (problem-solving) and pre-logical speech (the earliest expressive utterances), why is it not possible for a very simple society to have a language which is expressive but not logical, although they can go about the business of survival as animals do? Quine might answer that we simply could not understand a nonlogical language, and if a linguist found such a society, he would conclude that they have no language. (In fact, I believe that there are no known cases of societies with incoherent, babbling languages. The degree of complexity of language has resisted all correlations with the degree of complexity of society.)

Let us return to the Quinian linguist. Having translated highly observational occasion sentences and logical connectives by stimulus meanings, and having identified stimulus-analytic sentences and stimulus-synonymous pairs of sentences, the linguist begins to segment utterances into units, which he takes to be the words of the language, and to match these up with words in his own language. From observing
the important daily activities and preoccupations of his native speakers, he comes to conclusions about what brief expressions there are likely to be (e.g., a term for rabbit in rabbit-hunting country; several words for kinds of snow among Eskimos). Experienced, perhaps, with several languages, the linguist's guesses are informed by his sharpened sense for formal similarities among languages. The "typical case" of formulating an hypothesis is where the linguist "apprehends a parallelism in function between some component fragment of a translated whole native sentence and some component word of the translation of the sentence." He tests his tentative definitions against the sentences he has already translated by stimulus meanings and those identified as analytic and synonymous. When the latter two kinds are translated, they should encouragingly yield analytic and synonymous sentences in English. And so he goes on, "thinking up" hypotheses, testing and rejecting, until he has compiled his lexicon which, he believes, conforms to the "totality of speech behavior" he has observed, and even to dispositions beyond what he has observed.

The analytical hypotheses "extend the working limits of translation beyond where independent evidence can exist." They do this primarily by what may be a most skillful and subtle perception of analogies. While there may be no denying that the analogies (i.e., similarities in certain respects) perceived by the linguist do exist between the language under study and, say, English, this in no way entails that these features in the unknown language, on which the analogies are based, have the same significance, or any significance, to the speakers of that language. It also does not entail that all English-speaking linguists
would perceive the same analogies. There is nothing in the overt behavior, elicited or spontaneous, of the native speakers which could falsify an analytical hypothesis. The completed English-Other lexicon is "woefully under-determined" by the stimulus meaning translations of observation sentences. The translations of these sentences and all the observed behavior of native speakers are compatible with countless "rival systems of analytical hypotheses." Nothing that the linguist has observed, nothing that the linguist can or could observe, can select a "right" system of analytical hypotheses or eliminate a "wrong". Translations based on these hypotheses are supported by the coherence of the system of hypotheses, and by nothing more, "now and forever."

There is no criterion for choosing among different translations of a sentence, each supported by a different system of hypotheses. A systematic criterion, such as simplicity, is not wanted here because, at the level of comparing different individual sentence-translations, the simplest system may have produced a translation which is wrong; which has a different truth-value from the sentence translated.

Here we are at the heart of the problem. The ascription of synonymy between terms is supported by a system of analytical hypotheses. Many such systems are possible, each self-coherent and consistent with all dispositions to verbal behavior in both languages. However, the mapping of synonymy-relations between terms will differ in different systems in such a way that one Other sentence will receive several — and not always even truth-functionally equivalent — translations in English.

Quine has accepted with equanimity a theory-relative concept of truth for science. "May we conclude," he asks, "that translational
sy nonymy at its worst is no worse off than truth in physics? To be thus reassured is to misjudge the parallel." To assert that a statement is true, where 'true' means 'true according to Newtonian physics', is to be ready, if challenged, to support the claim with other statements from the theory. This is, in fact, what happens. The reasons given become increasingly theoretical, if the challenge is pressed far enough, until one comes to the basic assumptions of the theory. In a conflict between A, defending common sense, and B, defending modern physics, with respect to a statement which A claims is true and B claims is false (e.g., "A chair is solid."), the supporting statements made by A and B will quickly enough get down to fundamental assumptions. One may not persuade the other, but neither will be puzzled at the end as to why they disagree on the truth of the statement.

On the other hand, the analytical hypotheses used in compiling a lexicon are hidden from view. They are not even entirely explicit for the lexicographer himself, and are not available for settling disputes about variant translations of a sentence. One does not, on any account, look to principles of translation for support for the translation of a sentence. A sentence is thought to have a meaning, related to the meanings of its constituent words and its syntactic form. One would look up the words, check a grammar, and if A and B found one other-word translated by two entirely different and incompatible English words or expressions in their two lexicons, they would conclude that one lexicographer had made an empirical error which could be settled by a field trip and usage-questionnaires. The trouble is that a given word in the unknown language could be translated in the following assorted ways for
the following assorted, but unstated, reasons:

Native word: 'Plunko'

<table>
<thead>
<tr>
<th>Lexicon</th>
<th>English translation</th>
<th>Analytical hypotheses</th>
</tr>
</thead>
<tbody>
<tr>
<td>A</td>
<td>'animal'</td>
<td>There must be a word for 'animal' because many kinds of animals are hunted and indiscriminately stored.</td>
</tr>
<tr>
<td>B</td>
<td>'spirit dwelling in all animals'</td>
<td>Term is used only in sentences which appear to function as prayers, expressions of awe, wishes, etc.</td>
</tr>
<tr>
<td>C</td>
<td>'spirit dwelling in all living things'</td>
<td>as above</td>
</tr>
<tr>
<td>D</td>
<td>'result of a successful hunt, or disposition to be such'</td>
<td>Language nominalizes effects, not things.</td>
</tr>
<tr>
<td>E</td>
<td>'portion of running, or disposition to be such'</td>
<td>Language nominalizes actions, viewed as divisible wholes, not things.</td>
</tr>
</tbody>
</table>

This may give some hint of the drastic differences possible, which cannot be dissolved by further field work because they are all compatible with all the observations and depend on interpretations of those observations.

The problem cannot be solved by bilingual linguists, because a bilingual only represents an internalized system of analytical hypotheses, one of many, and two bilinguals could find the same "differences in net output" between themselves as between two rival lexicons. Furthermore, the problem of indeterminacy exists, although in a practically much less troublesome form, in translating closely related languages, or languages of closely related cultures. It even exists, as a distinct theoretical possibility, in understanding and paraphrasing the utterances of another speaker of one's own language. How often, for example, in conversation, one makes allowances for another's slips of the tongue, false starts, deviant usages, etc. If, instead of making al-
One frequently hears [footnote to Cassirer, Lee, Sapir, Whorf] that deep differences of language carry with them ultimate differences in the way one thinks, or looks upon the world. I would urge that what is most generally involved is indeterminacy of correlation.

When the culture is very different from the linguist's, it is more difficult to distinguish cultural differences from failures in translation due to indeterminacy. However, indeterminacy exists, although it does not always intrude as a practical problem, all along the continuum from two speakers of the same language to two speakers of totally unrelated languages.
CHAPTER II
THE PROGRAM OF REGIMENTATION

A. Purpose and Procedure

In turning to the subject of this chapter, we are following Quine's own course in *Word and Object*. The relevance of the problems raised by regimentation to indeterminacy of translation will be developed in the course of the discussion. At first, however, it looks as if Quine is abandoning the virtually impossible — translation between languages — in order to deal with the manageable and promising — the clarification of ambiguity within one natural language by logic.

Using the case of radical translation, Quine has argued that the extensions, as well as the intensions, of terms in two languages cannot be matched on the basis of "surface irritations", but only via analytical hypotheses which are unempirical because they are not falsifiable by any observable behavior. Although, in principle, the English of A and the English of B can be considered two different languages — as, in some ways, they are, with respect to language-learning history and numerous differences in usage — and thus subject to the same indeterminacy as the English of A and the Kalaba of Q, A and B manage to communicate in English, making allowances for isolated peculiarities of each other's utterances, and avoiding speculations about irresolvable differences of meaning between them. Quine therefore proceeds to outline how a child learns the referential devices of his native language, in this case, English, and then, taking "the mastered language as a going concern... consider[s] the indeterminacies and irregularities of refer-
Quine puts aside, after Chapter II, the indeterminacy of translation of terms, which so drastically hinders the linguist in his enterprise, and turns to problems in communication between speakers of a common language which could not even arise for the linguist attempting radical translation. There are two preliminary comments: (1) the theoretical indeterminacy of translation between two speakers of the "same" language, and the practical problem of different speech sub-communities within the "same" language, play no part in the discussion of these problems in communication; (2) the problems dealt with are no longer those of a fictional linguist, but of a philosopher concerned with "logical grammar".

The problems arise from several kinds of ambiguity (of terms, of particular constructions, of syntactic structure, of scope) and of the failure of reference in indirect discourse and intentional contexts. There are two stages in the procedures for dealing with them. The first stage involves "practical temporary opportunistic departures from ordinary language" for getting over a sudden block in communication. These may be the use of variables to clear up problems of cross reference, the use of parentheses for problems of grouping, the "such that" construction for problems of scope, and the distinction between referential and nonreferential position to clarify one's intentions in sentences of believing, hoping, thinking, doubting, etc.

The second stage is "regimentation". Regimentation is not different in kind from the paraphrasing operations of the first stage, which also drew on the standardizing and simplifying functions of logical
theory in the use of parentheses, variables, etc. The ultimate purposes are different: on-the-spot paraphrasing has as its purpose "getting over a sudden block in communication"; regimentation has as its purpose "simplification of theory". We regiment, rather than merely patch up ordinary language when "we are limning the true and ultimate structure of reality." In this posture, we adopt a more severe attitude toward the expressions of ordinary language than the usual attitude of using words without intending to postulate the entities they name. This more severe attitude is the acceptance of the "ontological commitments" of our discourse. Quine's "double standard" is the differentially rigorous treatment of expressions in ordinary language, depending on one's purposes: everyday or scientific discourse.

A paraphrase intended to clear up an ambiguous construction in everyday discourse may use fragments of logical notation in a sentence of English, for the sake of the clearer standards attending the use of, for example, parentheses in logic than of commas and relative word position in English. In a regimented paraphrase, the logic must be more thoroughly "digested" by the English sentences than it is in its "opportunistic" use above. In order to make all one's commitments explicit, and to take advantage of deductions possible in quantified first order logic, English sentences must be "transformed" into logical notation "adapted to the theory." The English sentences may be drastically altered in the process. Some redundant constructions will be assimilated, some puzzling ones eliminated altogether. The quantity of change exceeds that of the first stage because the motive for change is different. Quine compares paraphrase in regimentation to the reformulation of a prob-
lem being programmed for machine computation: it prepares the way for
"the methodical manipulation of formulas according to fixed rules of
algorithm".

Whether the ambiguity of grouping attending 'pretty little girls'
camp' is clarified by the use of parentheses, as '(pretty(little girls')
camp,' depends on whether the speaker or writer intends to refer to a
pretty camp for little girls or rather to a camp for girls who are both
pretty and little. The sentence-paraphrase in full "canonical notation"
must also bear a relation of some sort of equivalence to the pre-paraphrased sentence. If not, there is no gain in the undertaking. It would
be difficult to count how many times Quine reiterates in these pages
that the ordinary-language sentence and its paraphrase in canonical no-
tation are not synonymous. What does this negative claim amount to?

Let us take the following two ambiguous sentences:

(1) Everyone in the room knows at least two languages.

(2) At least two languages are known by everyone in the room.

Both of these sentences are ambiguous because it is not clear whether
the two languages are the same two or any two. The following paraphrases
are the two ways of resolving the ambiguity:

(3) (x)(∃y)(∃z) (x is in the room • y is a language • z is a lan-
guage • x knows y and z)

(4) (∃y)(∃z)(x) (x is in the room • y is a language • z is a lan-
guage • x knows y and z)

Paraphrase (3), with its arrangement of quantifiers, resolves the ambi-
guity in favor of any two languages (which does not entail, of course,
that each person in the room must know different languages from every
other person in the room), and paraphrase (4), with its arrangement of
quantifiers, resolves the ambiguity in favor of the same two languages.

It is clear that neither (3) nor (4) is synonymous with either (1) or (2) simply because (1) and (2) are ambiguous and (3) and (4) are not. (1) and (2) have each two possible interpretations, and therefore two possible truth-values for any given utterance, while (3) and (4) have each a single interpretation and only one truth-value for any given utterance. In this sense, no paraphrase, successful in resolving ambiguity, can possibly be synonymous with the ambiguous ordinary language sentence to be paraphrased.

Let us consider the following two sentences, in what Quine calls "semi-ordinary" English:

(5) Each person in the room is such that there are two languages that he knows.

(6) There are two languages such that each person in the room knows them.

(5) is synonymous with (3), and (6) is synonymous with (4), or might "naturally enough be spoken of a synonymous", according to Quine, because (3) and (4) "mechanically" expand into (5) and (6), respectively, by the metalinguistic rules of the logic. But, from this, one cannot move to assert synonymy of either (1) with (3) or (1) with (5). The transition from (1) to (5) involves settling on one interpretation of the ambiguous sentence as well as accepting rules in the logic for the significance of the order of quantifiers and the "such that" construction.

With respect to the order of quantifiers, whether in logical notation or in "semi-ordinary" English, the user is strictly committed by them to determinate interpretations as he is not by the inversion of word order
In ordinary English, which may be used merely for emphasis, poetic meter, or emotive effect.

In the above examples, we did not touch the questions of what "knowing a language" is, or, for that matter, "being a (distinct) language", because, problematical as those predicates are, they are unrelated to the ambiguity of sentences (1) and (2). There are cases, however, where paraphrasing must directly attack the predicates. Examples are found on pages 152-154 of *Word and Object*, when Quine is dealing with the referential "opacity" of particular verbs. One brings out the propositional attitude implicit in "The commissioner is looking for the chairman of the hospital board" by expanding 'look for' into 'endeavor to find'.

"Anyone is ready enough to paraphrase" "Giorgione was so-called. . ." into "Giorgione was called 'Giorgione'. . ." "Just as looking for is endeavoring to find, so hunting is endeavoring to shoot or capture." The underlined words show the expressions Quine uses in substituting predicates for one another, without ever claiming that the substituted predicate means the same as the predicate substituted for. His insistence on the nonsynonymy of sentences with their paraphrases applies fully to cases of paraphrase which involve changes of predicates.

Quine takes a very permissive course with predicates, even in the process of full regimentation. There is "no inventory of allowable terms," "a fixed, closed vocabulary of simple terms" in the manner of Carnap — and no intrinsic distinction between "simple" and "complex" terms in the manner of Russell. Any predicates are, in principle, acceptable, however complex, however unobservable their referents. Some-
times paraphrase will nevertheless involve analyzing predicates, not because the theory requires it but because the paraphraser finds it perspicuous to do so, and these predicate-pairs are not to be considered synonymous. Indeed, two paraphrases of one English sentence may exhibit wide differences with respect to analysis (or "translation") of predicates. Stimulus synonymy of general terms, even within English, is likely to be "too loose" to individuate finely enough. How, then, can they be compared with each other and with the paraphrased sentence?

To this question, as well as to the one about comparison of paraphrases and paraphrased sentences generally, the answer is that it all depends on the purposes of the paraphraser. It is assumed that the paraphraser is also the utterer of the English sentence which is being paraphrased. This is at least the "paradigm case". He knows what he means, and he knows if the paraphrase resolves difficulties and preserves what he intends to preserve of the "meaning". He can compare alternative paraphrases, and choose between them on the basis of his intentions. He may be advised by others, and even on occasion persuaded, "but his choice is the only one that binds him." Two speakers of English, both engaged in the regimentation of discourse, can, by "semantic ascent", make clear to each other what they are doing, and perhaps why, but, for a given English sentence, there are no common standards for choosing the paraphrase of one over that of the other, nor for determining that one paraphrase — or both, or neither — is a paraphrase of that English sentence.

Quine's argument seems to turn on the acceptability of any predicates, and the problems that follow from that. He does also mention that there is no mechanical procedure for determining the equivalence of
quantificational sentences in notation, "no general limit to the length of inquiry that may be required." The more crucial premise appears to be the dependence of the "survival" or "disappearance" of general terms on the "momentary purposes" of the paraphraser. Comparing sentences in notation involves comparing both their logical forms for equivalence and their predicates for synonymy. However, this test for "structural synonymy" mistakenly emphasizes predicates, because, in regimentation, their choice is "wholly casual".

He rejects, as we have seen, altering this state of affairs by formalizing a vocabulary of science. Regimentation is not system building in the Carnapian manner. Quine's arguments against the usefulness of formalized languages for analyzing analyticity apply here. He characterizes the specification of only certain predicates as acceptable in notational paraphrases as "arbitrarily assembled groundwork". One cannot expect to delineate "the true and ultimate structure of reality", the expressed task of regimentation, from such a base.

Once we grant that all predicates are acceptable, we are up against the problems raised by the principle of indeterminacy when we try to match them. Paraphrase is a kind of translation, after all, and, beyond stimulus meaning, which will fail to distinguish sufficiently in many cases, there are no satisfactory criteria for "meaning the same". Without drawing explicitly on the conclusions of Chapter II, Quine repeatedly denies synonymy claims and proposes the criterion of the paraphraser's purposes.

The question that arises is how "purposes" can be specified and compared. It is hard to believe that Quine is sketching a state of affairs
in which individual philosophers, with idiosyncratic purposes, regiment ordinary language for the sake of expressing an "ultimately true" world view, not measurable against anyone else's. This would be absurd. Indeed, and more reasonably, in his discussions of particular explications in mathematics, the "purposes" are spoken of as common purposes. Anyone interested in certain problems is interested in certain devices or locations, which are generally agreed to serve particular purposes with respect to the problems in question. The "purposes" are thus given, in the context of the device and the field of discourse. Regimentation is the procedure by which one redefines the device so that it serves its purpose more clearly and effectively. Explication will be discussed next, and these features will come out in some detail.

Word and Object can be viewed as a sustained, and even virtuosic, effort to interpret scientific discourse within the framework of an "austere" extensional logic. If it now turns out — and it seems to be far from hidden in Quine's exposition — that "purpose" and "function" are fundamental notions, has he not built unquestionably nonextensional concepts into the foundation of his extensional superstructure? And if there are good reasons for using these concepts as fundamental, why can they not be used with equal effect in translation? These questions will come up again in later sections.

Some paraphrases achieved by regimentation have the status of "explications". Explication is often held to be the fundamental task of philosophical analysis. It is the clarification of an important, but ambiguous or otherwise troubling, expression by reformulation. How is the reformulated expression, the explication, related to the original expres-
We do not claim synonymy. We do not claim to make clear and explicit what the users of the unclear expression had unconsciously in mind all along. We do not expose hidden meanings, as the words 'analysis' and 'explication' would suggest; we supply lacks. We fix on the particular functions of the unclear expression that make it worth troubling about, and then devise a substitute, clear and couched in terms to our liking, that fills those functions. Beyond those conditions of partial agreement, dictated by our interests and purposes, any traits of the explicans come under the head of "don't cares".

Note that determining the functions of the expression "that make it worth troubling about" is one step in this procedure, and that "devising... a substitute... that fills those functions", and (implicitly) determining that the substitute fills the functions, is another step. There is no "uniquely right" explication — here is Quine as relativist again — but it is clear in this section that there are standards for being an explication of a given expression. "Explication is elimination but not all elimination is explication." The former, because explication always eliminates some unclear features of an expression; the latter, because some pruning may disqualify the expression from serving the function for the sake of which it was cleaned up. Elimination is explication "just in case the new channels parallel the old ones sufficiently for there to be a striking if partial parallelism of function between the old troublesome form of expression and some form of expression figuring in the new method."

Quine recognized this requirement well enough in "Two Dogmas," although it is expressed there in terms of "contexts" of the use of an
expression rather than of "functions" of an expression. But the
function of an expression is its use in some contexts, so this dif-
ference of terminology is not important. What is significant is that
explication is mentioned in "Two Dogmas" as a kind of definition, and
the point Quine wants to make about all definitions, except stipulative
definitions by fiat, in that essay is that they rest "on prior relations
of synonymy." For explications, the prior synonymy is between contexts:

Any word worth explicating has some con-
texts which, as wholes, are clear and pre-
cise enough to be useful; and the purpose
of explication is to preserve the usage of
these favored contexts while sharpening the
usage of other contexts. In order that a
given definition be suitable for purposes
of explication, therefore, what is required
is not that the definiendum in its ante-
cedent usage be synonymous with the defini-
ens, but just that each of these favored
contexts of the definiendum, taken as a
whole in its antecedent usage, be synonymous
with the corresponding context of the defini-
ens.

In Word and Object, Quine insists that explications are not synonymous with
their explicanda, and he does not mention "contexts" of the expressions
at all. However, there is a germ, in the similarity between the dis-
cussions of explication in both works, of a theory of sameness of mean-
ing which rests on sameness of function or purpose (however that may be
measured).

It might be useful to go from this abstract discussion to a con-
crete illustration of explication. We will consider Russell's theory of
definite descriptions, "that paradigm of philosophy," according to Ramsey,
and then Quine's extension of it, the elimination of all names and sin-
gular terms. We will be interested in seeing what the salient features of
Russell's theory are, and in trying to see which assumptions are central and which fortuitous. With respect to Quine, we will raise the question of the extent to which his "extension" of Russell's theory is a significant modification of it, and how the program of regimentation looks in the light of the consequences of this example of regimentation.

B. Two Examples of Explication: Russell and Quine

Russell's treatment of uniquely denoting phrases can be readily seen as an explication by Quine's standards. Russell has "fix[ed] on the particular functions of the unclear expression that make it worth troubling about"; he has eliminated some features and preserved others. The substitute expression serves the desired functions of a uniquely denoting phrase while avoiding the problems which Russell wished to avoid. When we come to Quine's modification of the theory of definite descriptions, we shall see that among the undesirable features for him is a distinction which Russell particularly wished to preserve. In such details are the different purposes of philosophers made manifest.

Russell wants denoting phrases in propositions to be meaningful, whether or not there is anything in the physical world which is actually denoted by them, because he believes that much of our "knowledge about" things is expressed in this form. He also wants to keep the distinction between such "knowledge about" and knowledge by acquaintance which, for him, entails preserving the distinction between descriptive phrases and proper names. Russell aims to selectively define "'the' in the singular." Indeed, this aim has been taken by some, for example, G.E. Moore, as the sole purpose of the theory of descriptions, but this seems too
narrow because Russell demonstrates the same technique with respect to
indefinite descriptions. Descriptive phrases beginning with 'the' rather than 'a' are more likely to deceive, according to Russell, but 'the' phrases differ from 'a' phrases only in implying uniqueness and he is more seriously concerned with existence than with uniqueness.

The final purpose of Russell's analysis is the closing of truth-value gaps. Every proposition expressed by a sentence containing a uniquely denoting phrase is either true or false. Strawson's criticism of this feature of the theory is based on the supposed fact that in ordinary conversation we take certain statements containing descriptions to be "neither true nor false". This may be, but Russell sees the closing of truth-value gaps as a "great advantage" not as a reproduction of the vagaries of ordinary language. From the viewpoint of the logical simplification of the analysis of language, it certainly is an advantage. Of course, from Strawson's point of view, simplification is a mistaken goal. Fundamental differences of approach are at issue here.

Russell wants to avoid attributing significance to a denoting phrase as a unit in isolation, that is, apart from the meanings of the constituent words of the phrase and apart from the meaning of the whole proposition. He believes that the meaning of the phrase as a whole can only be its denotation. The argument runs as follows: The meaning of the denoting phrase is the meaning of its denotation. If 'the author of Waverley' denotes, it denotes a person, in this case named Scott. The meaning of the denoting phrase is, therefore, the meaning of 'Scott', and the meaning of 'Scott', according to Russell's doctrine of proper names (see below), is the man Scott.
If the phrase does in fact denote something existent, then the sentence 'Scott is the author of Waverley' is reduced to a trivial statement of identity, 'Scott is Scott,' which Russell says is "plainly different" from the first sentence. What he seems to mean by "plainly different" is that the former sentence conveys information (i.e., there were people who knew of Scott and who knew Waverley and who did not know the fact expressed in the sentence because the work was pseudonymously published) while the latter is a tautology. This is really an illustration of the inadequacy of a denotational theory of meaning, but Russell does not see the problem that way because of his theory of proper names. If the phrase does denote, and if its meaning is its denotation, then taking it as independently significant changes the meaning of the sentence as a whole.

If, on the other hand, the phrase does not denote anything existent (e.g., 'the round square'), then there is a fatal metaphysical temptation to posit "subsistent" or "nonexistent" entities which Russell wants to avoid at all costs in the interest of "that feeling for reality which ought to be preserved even in the most abstract studies." "There is only one world, the 'real' world."

To preserve the meaningfulness of the whole proposition while denying meaningfulness to the denoting phrase in isolation, Russell shows that in a "rightly analyzed" proposition of this type the denoting phrase disappears, or rather appears "broken up" in a "fully expressed proposition". In *Principia Mathematica*, in somewhat different terms, he "proves" that descriptive phrases are "incomplete symbols" which have meaning only in use, in context. The logical apparatus which al-
ows him to do this includes variables, propositional functions, and quantification. Variables stand for entities nameable by proper names. Propositional functions "define the property that makes a thing a so-and-so." The distinction between what Russell means by a "proper name" and a descriptive phrase is crucial.

Although, in practice, "almost all uses of what look like proper names" are "abbreviated description[s]," the distinction is important in principle. A proper name has meaning "by itself, without the need of any context." A proper name "directly designates an individual which is its meaning." The individual so named is a "constituent" of the fact or proposition expressed by the sentence in which the name appears as subject. A proper name cannot appear meaningfully as subject in the sentence '____ exists' because, if it is a name, it must name something, something which is. It is because none of these statements are true of descriptions that descriptions, unlike proper names, depend on their context for their meaning and do not necessarily denote anything. It is curious that Russell wants to maintain that there are proper names, in his sense, when they are admittedly so elusive and rare. The examples he uses, 'Socrates' and 'Scott', are not real proper names at all. All that concerns us here, however, is that he does maintain this distinction in all formulations of the theory, and that we may accept it as important for him.

Russell has no doubts on the score of the meaningfulness of single words.

In the proposition 'I met a unicorn,' the whole four words together make a significant proposition, and the word 'unicorn' by itself is sig-
significant, in just the same sense as the word 'man'.

'I met a unicorn'...is a perfectly significant assertion, if we know what it would be to be a unicorn...i.e., what is the definition of [a unicorn].

He passes, without stopping to explain or justify, from the apparent-name subjects in descriptive phrases (e.g., 'father', 'author') to verb-phrase propositional functions (e.g., 'hegat', 'wrote') on the basis of his unquestioning understanding of the meanings of single words. Moore, in commenting on and generally approving of Russell's analysis of descriptive phrases, comes up with an incredibly trivial objection, namely that an author need not have written a work since he might have composed it within an oral tradition. Moore suggests that an author is one who has "invented or composed [the work] without the collaboration of any other person" subject to some other conditions. After pages and pages of this, Moore grants that this is not an argument against Russell's theory since this point (i.e., the adequacy of the propositional function chosen in that example) is "no part of the theory." Moore might have questioned the validity of the procedure in general, but he does not. In the remainder of his essay, using the 'King of France' example, Moore does not bother to "translate" the subject-term but uses 'is a king of France' as the propositional function. Russell does the same with this example but "translates" his other examples.

As for the analysis itself, for an example where something is predicated of the presumed referent of a uniquely denoting phrase, Russell offers an existentially quantified conjunction such that, if
any conjunct is false, the proposition is false. In its ordinary-language formulation, for 'The author of Waverley was Scotch,' it looks like this:

(1) at least one person wrote Waverley;
(2) at most one person wrote Waverley;
(3) whoever wrote Waverley was Scotch.  

More formally,

\[ \{\{x\in\mathbb{R} | \phi(x)\}\} =: (\exists x) \phi x \equiv x = c : \rho \]  

or, in "semi-ordinary" language,

There is one entity such that 'something is _____' if and only if that 'something' is the entity, and the entity is (also) _____.

The descriptive phrase, in words or notation, has indeed been analyzed out and no longer appears. The existential assumption in the correct use of a definite description stands revealed.

Strawson’s criticism of Russell, to the effect that a proposition containing a non-denoting phrase is not false but neither true nor false, rests on his refusal to acknowledge that the implicit existential assumption is binding in all such statements. Strawson’s position is that if one does not explicitly assert the existential part of such a statement, existence is not being asserted, even though it is presupposed, "implied" (in an "odd", nonlogical sense), "signalled" by the utterance of a sentence containing a denoting phrase. Strawson grants that the utterance of such a sentence is "evidence" for the speaker's belief in the existence of the denotatum; it "shows but does not state" that the speaker thinks the "existential conditions" are fulfilled.

Now it is literally true that what is not audibly and explicitly asserted is not an audible and explicit part of the utterance. But there
is a sense in which we mean more than what we say: the sense in which we stand ready to respond to queries and challenges to our utterances, to clarify, to act on, to elaborate. This is the sense in which what is implied or assumed is also asserted even if it is not audibly uttered. However, although many of the examples Strawson gives are subject to this analysis, there are undoubtedly sentences which are not. One is reminded that explication is not the reproduction of ordinary language in a different form, nor the supplying of "hidden meanings", but it is a reconstruction of an expression, preserving its useful functions and eliminating its problems.

Does it matter to Russell's theory what is substituted for the $\Phi$ in the propositional function $\Phi x$? In most of the examples he has chosen, this is not a problem, pace Moore. However, he writes as if one should normally transform what appears to be a name in the denoting phrase into a propositional function (or conjunction of propositional functions) by selecting the relevant property or properties and expressing it in verbal or adjectival form. This is what he usually does. 'I met a man' is translated as "'I met $x$, and $x$ is human' is not always false."

Thus when we say 'x was the father of Charles II' we not only assert that $x$ had a certain relation to Charles II, but also that nothing else had this relation. The relation in question... is expressed by 'x begat Charles II'.

Russell assumes that the analyzed sentence is equivalent in meaning to the preanalyzed sentence. Moore is right in saying that these translations of apparent-names into predicates are not part of the theory. Russell offers no guidance on how one should go about selecting defining
properties, but he takes it for granted that it can be done.

It is interesting that Russell left the 'King of France' example untransformed, substituting for the \( \phi \) in this case 'is-King of France', because a more complicated propositional function is involved here than in the other examples he uses. He may have wanted not to get embroiled. Consider the contrast between it and his other examples:

(1) 'Author' may be defined as one who writes (or composes) books.
(2) He is the author of x but he did not write x.
(3) 'Father' may be defined as one who is male and has beget a child.
(4) He is the father of x but he did not beget x.
(5) 'King' may be defined as one who is a king.
(6) He is the king of France but he is not the king of France.

As the 'King of France' example has been treated, (6) is self-contradictory as are (2) and (4). However, only (6) is purely logically self-contradictory. The other examples depend on a relation of synonymy between the predicates and the apparent-names. Russell had no misgivings about synonymy in principle, and, in any event, (1) and (3) are reasonably acceptable definitions in English. What would be the defining property of being a king?

(7) 'King' may be defined as one who is male and rules a state.
(8) He is the King of France but he does not rule France.

We can see from (8) that this is not an example that lends itself to analytic statements. Someone may be king in name only, a figurehead; someone may be king but in exile; someone may be king and refuse to perform his functions.

And yet merely being called 'king of France' will not do. It
would be simple to make false statements true and true ones false if that were allowed. 'The World's Best Bakery' may be, for advertising purposes, a good choice of name, but "'The World's Best Bakery' is not the world's best bakery" is not self-contradictory. Similarly, the most powerful man in Washington is not likely to be called that, but may be called 'Assistant Secretary' or 'Boss So-and-so'. To move from denoting phrase to propositional function on the basis of x being called whatever the words of the denoting phrase are is to treat the denoting phrase as a name, as is mentioned, not used, term, within quotation marks. As Russell puts it, a denoting phrase within quotation marks "is merely part of the symbolism by which we express our thought." He goes on to assert, "What we want to express is something which might (for example) be translated into a foreign language; it is something for which the actual words are a vehicle, but of which they are no part."

This translatable "something" is commonly called a proposition. To admit propositions is to grant translatability, because propositions are meanings which are not tied to a particular language. One may choose to retain the wording of the denoting phrase in moving to a propositional function (e.g., from 'the king of France' to 'is-king of France'), but the theory neither requires it nor favors it. Russell's epistemology does favor translation into relatively simple predicates, but this is separable. What cannot be as easily removed from consideration is that admitting propositions entails admitting meanings and sameness of meaning. The sentence explicated by Russell's analysis means the same as the unexplicated sentence (with its assumptions spelled out) because they both express the same proposition. It will be important, in comparing Russell
with Quine, to remember that the former but not the latter assumes that criteria exist for synonymy and uses the concept in his analysis.

Russell has explicated descriptive phrases by analyzing them out in favor of quantifiers, predicate letters, variables, and identity, while retaining the desired functions of those phrases in propositions. He has shown that descriptive phrases always have meaning in context, even in cases where they do not in fact denote anything. By making the implicit existential assumption explicit, and separating it from the meaning-in-context of the descriptive phrase, Russell has also achieved the not quite incidental advantage of making all such statements either true or false and eliminating previous truth-value gaps.

The second stage of what Quine does is, with only minor notational variations, the same as what Russell has done: he shows that singular descriptions are eliminable. However, in the stage preliminary to this Russellian maneuver, Quine shows that all singular terms are eliminable in favor of singular descriptions, or, as he puts it, can be "reparsed" as singular descriptions. This is a step that Russell decidedly did not want to take, since he wanted to preserve the name/description distinction.

Before we take up Quine's motives and justification for this move, we should notice just how vast is the scope of this particular act of regimentation. Quine is not merely explicating those phrases which have the form of descriptions, definite and indefinite, as Russell is. His is a sweeping regimentation of the entire referential apparatus of a language. Henceforth, we have only to deal with general terms — predi-
cates — which are "true of" whatever entities we allow as referents of bound variables, plus variables, quantifiers, identity, and truth-functions. If all terms are general terms, one can admit, without qualms, any words to termhood and "safe" predicate position. We are only committed to the assertion of the existence of an entity when we allow it to be designated by a variable. One cannot avoid quoting Quine's famous mot at least once, "To be is to be the value of a vari-

Just because this reform is so vast, it is doubtful that one should call it an explication, comparable to the explication, for example, of 'ordered pair'. It would be hard to ask if the explicatum fulfills the function for which the explicandum was considered worthy of attention. Quine is dealing here with the referring and predicating functions of expressions in a language, and these are functions of very high generality. One can inquire whether, with the materials Quine assembles, it is still possible to predicate something of a unique individual, to assert or deny the existence of an individual with a particular set of properties, and so on. It is difficult to frame the questions generally enough, and difficult not to fall into expressing them in post-regimentation terms. Is there anything the reformed referential-predicative apparatus does not let us say which we could have said before, with the exception of just those ways of speaking which Quine wants to avoid? Perhaps this can be more meaningfully discussed after an exposition of Quine's procedure and reasons, and perhaps we can then give more substance to what Quine means by the "function" of an expression.

Singular terms have the troublesome feature, for Quine as for
Russell, that they "always purport to name an object, but powerless to guarantee that the alleged object be forthcoming." Like Russell, Quine rejects positing special entities, existing otherwise than physically, to stand for the objects named. In "On What There Is," he does this partly in a spirit of aesthetic fastidiousness. In Methods of Logic, almost in Russell's words, he rejects "existence in mythology" as a way of preserving the namehood of Cerberus:

Myths are literally false, and it is sheer obscurantism to phrase the matter otherwise. There is really only one world, and there is not, never was, and never will be any such thing as Cerberus.

But whereas Russell is content to deal with singular descriptive phrases, including some names which he, on grounds which are seldom mentioned but which involve his philosophy of "logically proper names," recognizes as "abbreviated descriptions," Quine casts his net wider and finds that the same difficulty attends all singular terms. He includes all apparent names, algebraic (i.e., combinatorial) singular terms ('x plus y'), demonstrative singular terms ('this apple'), descriptions in forms quite different from 'the ...' and 'a ...' phrases, and even class-names, attribute-names, and relation-names.

Fundamentally, the difficulty is that whether or not there is an object named by a singular term "cannot be systematically spotted by notational form." If there is no such object in a particular case, one's statements purportedly referring to it and predicing things of it are meaningless, and to say that it does not exist involves one in the Parmenidean self-contradiction with which Plato struggled in the Sophist. Quine succinctly states the problem in Mathematical Logic:
To say that something does not exist, or that there is something which is not, is clearly a contradiction in terms; hence $\neg(x)\neg(x \text{ exists})$ must be true.  

Russell writes that 'x exists' is meaningless, by which I take him to mean empty, giving no information, which amounts to the same thing in that context. If grammatical namehood alone qualifies a term to stand in the place of 'x', then all sorts of things have a claim on existence, and Russell's "robust sense of reality" would be rendered anemic. What exists is a matter of fact and not of grammar. To allow some terms the status of names — which is Russell's way — depends on being able to recognize which apparent names really name. Subjecting all singular terms to the same treatment that Russell devised for singular descriptions "is a way of maintaining control over questions of vocabulary independently of questions of fact."

Before departing from Russell in sweeping away all singular terms, Quine considers an "impractical sort of reform" which "recalls, if only in caricature, Russell's early philosophy of proper names and descriptions." One might keep as singular terms only those words learned as single words "through the primitive kind of conditioning that antedated the learning of compound singular terms." This means retaining a special category for those "few hypothetical" names which we may be supposed to have learned by direct confrontation with name and object." This is "impractical" indeed because we don't remember which words we learned by ostension, and because the set of such words would be different for each speaker of a language. It would be a small, dubiously instantiated, purely subjective category, and there is not even much reason to suppose that keeping it would clarify the problems attending
singular terms. Even 'mama', if x's 'mama' no longer lives, would not avoid confusion in singular term position in a sentence uttered at the present time.

Quine's conclusion, therefore, is that the category of singular terms is an unnecessary one. It creates some problems: deciding which words belong to it, and it does not solve any. It is, furthermore, tied to a particular theory of knowledge. The advantages of analyzing out singular descriptions can be extended to all singular terms "without prejudice to epistemology or ontology." This move is "at the level strictly of logical grammar," a decision on which categories to admit to the regimented language, and it is a decision in favor of "the primary of predicates". No entities are eliminated by this move, but entities become not "things which names name" but "things which predicates are true of," in some cases uniquely.

General terms and singular terms "are properly to be distinguished" by their "contrasting roles" in predication. In pre-regimented logical grammar, 'Socrates is a man' would be represented by 'Fa', where 'a' stands for a singular term 'Socrates', and 'F' stands for a general term 'man'. For purposes of predication, 'F' may have the form in English of a verb phrase ('drinks hemlock') or an adjectival phrase ('is snub-nosed') as well as that of a general noun. 'Is' or 'is a' may be regarded as a predicate-forming prefix. In the course of showing that all singular terms can be reclassified as general terms, with general term roles in predication, Quine is more explicit about what is, in effect, a reinterpretation of 'is'. 'Is' or 'is' remains "a separate relative term" only between variables; other-
wise, it acts as a predicate-forming prefix to a general term. The general term thus formed contains 'is' as an "indissoluble" part, as in 'is-snub-nosed' or even 'is-Socrates'. The effect of this is to combine 'is' or '=' with a general term or a singular term to form a new general term.

There are two important consequences of this reinterpretation. The first is the elimination of the need to deal separately with singular terms. By the "theorem of confinability" a singular term can be confined to the context ' = a' in which it is merely a part of an indissoluble general term which functions as a predicate rather than as a name. The second is the rejection of the notion that "translation" is necessary to carry a singular term into a general term. Superficial grammatical considerations are irrelevant. In parsing English, we would not consider 'is-Socrates' a predicate, but it is a predicate in the regimented grammar because it occupies predicate place in a sentence, according to the reparsing effected by regimentation. Epistemological considerations are irrelevant. How we could know that it is true or false to predicate 'is-Socrates' of an x is beside the point. We might turn our attention to this problem in the course of another kind of inquiry, but it need not concern us here. For Russell's theory, translation of apparent names into predicates was, as we have seen, if not forced, then at least strongly suggested by the theory of knowledge mixed in with the theory of descriptions. Quine deliberately rejects this.

This second consequence — the casting aside of "translation" — is obviously of considerable importance to an inquiry into regimentation, particularly one which seeks to relate regimentation to indeterminacy
of translation. This is the point on which Quine has altered the wording of his exposition and the manner of presentation more from source to source than any other. In "On What There Is" (1948) Quine suggests coining predicate-words where convenient.

In order thus to subsume a one-word name or alleged name such as 'Pegasus' under Russell's theory of description, we must, of course, be able first to translate the word into a description. But this is no real restriction. If the notion of Pegasus had been so obscure or so basic a one that no pat translation into a descriptive phrase had offered itself along familiar lines, we could still have availed ourselves of the following artificial and trivial-seeming device: we could have appealed to the ex hypothesi unanalyzable, irreducible attribute of being Pegasus, adopting, for its expression, the verb 'is-Pegasus', or 'pegasizes'. The noun 'Pegasus' itself could then be treated as derivative, and identified after all with a description: 'the thing that is-Pegasus', 'the thing that pegasizes'.

Two lines of criticism follow from this, for which Quine's own text can be cited, and which lead to conclusions unacceptable to him. One is that the statement resulting from reparsing singular terms as descriptions and then eliminating the descriptions must be "cognitively synonymous" with the original statement, and that Quine's rejection of synonymy undermines his own program of regimentation. Quine writes in this essay, in approval of Russell's contextual definition, "No unified expression is offered as an analysis of the descriptive phrase, but the statement as a whole which was the context of that phrase still gets its full quota of meaning — whether true or false."

If we look at the underlined phrase, we can see that it appears to be out of line with Quine's standards for "explication" which we outlined earlier. The claim that post- and pre-regimented sentences are
synonymous is repeatedly rejected in *Word and Object*. "Full quota of meaning" is not the clearest expression Quine could have used had he wanted to refer to synonymy between statements. Possibly significance, not synonymy, is the intended concept, and yet significance is not a matter of degree, at least not for Quine. The words "full quota" suggest that the regimented statement does not mean less than the original, which leads one to an interpretation that the regimented statement means the same as (is cognitively synonymous with) the original because we do not operate with concepts of more or less meaning, but with concepts of same and different.

Note, however, the curious tag end of the quoted sentence, "—whether true or false." The hyphen is ambiguous here. If it were a comma instead, we could clearly read that both true and false statements still get their full quota of meaning. This way, it is possible to interpret the whole sentence as asserting that, under the program of the elimination of descriptions, true statements come out true and false statements false. This interpretation would seem more in keeping with Quine's allergy to synonymy, although it is a very weak justification for contextual definition since any true statement and any false statement would serve to fulfill the condition. Quine's position on synonymy undoubtedly hardened after "On What There Is" so we need not expect complete consistency. It is at least plausible to interpret this formulation of the elimination of singular terms as resting on cognitive synonymy between statements. Both Quine's close juxtaposition of Russell's program with his own, and his ambiguous language, encourage the interpretation.
The second criticism focuses on the choice of 'pegasizes' as the "artificial and trivial-seeming device: ... [appeal] to the ex hypothesi unanalyzable, irreducible attribute of being Pegasus." This choice of words led Quine into a dialogue with his imaginary debating partners, McG and Wyman, in the pages of the essay itself, where he denies that the selection of the coined predicate commits him to recognize the existence of a corresponding attribute, meaning, or other sort of universal. He undoubtedly wanted to argue these points, and coined the predicate because it raises predictable objections. However, from the point of view of the program of eliminating singular terms, it was probably unwise to raise the spectre of appearing to introduce an "undefined property term". 'Pegasizes' stands on uncertain middle ground between a translated predicate such as 'wrote Waverley' and a name made a predicate solely by being in predicate form and position such as 'is-Socrates'. 'Is-Pegasus' is introduced as an alternative to 'pegasizes' from the start, but critics and commentators have usually referred to the more colorful of the predicates, and sometimes to the issues apparently associated with it.

In Methods of Logic (1950) coined predicates are abandoned, and the futility of a standard of synonymy is expressed but not lengthily argued.

Whether a proposed deduction is to enjoy the benefits of a descriptive premiss depends, evidently, on whether a given singular term can fairly be translated into the form of a description. Now fairness of translation is a vague matter, hinging as it does on the concept of synonymy which was so dimly regarded in [para.] 33.
Perhaps it will even be found that of significance and synonymy only significance admits of a satisfactory criterion, and that all effort to make sense of 'synonymy' must be abandoned along with the notion of meaning.

Given any singular term of ordinary language, the proper choice of 'F' for translation of the term into '(\(1 x)Fx\)' need in practice never detain us. If a pat translation such as '(\(1 x)(x \text{ wrote Waverley})\)' lies ready to hand, very well; if not, we need not hesitate to admit a version of the type of '(\(1 x)(x \text{ is-Socrates})\)'. . . since any less lame version would, if admissible as a translation at all, differ at most in expository value and not in meaning. 125

One would think that Quine's conclusion should be that since "fairness of translation" does rest on synonymy, and since synonymy lacks defining criteria, the attempt at translation should be given up altogether. And yet, if there is a "pat translation", "very well". Strange-ly enough, Quine selects 'wrote Waverley' as an example, ignoring Moore's quibble over just this translation. If there are no satisfactory standards for sameness of meaning, what makes a translation "pat"?

Leaving this aside, and looking at the underlined phrase, what is even more curious is that Quine appears to justify predicates of the 'is-Socrates' type on grounds of synonymy ! The argument runs as follows:

(1) 'Socrates' is a singular term 'a' which we want to transform into a descriptonal phrase '(\(1 x)Fx\)).

(2) 'Athenian philosopher who drank hemlock' is suggested as a choice for 'F' which "translates" 'Socrates' into a predicate.

(3) 'is-Socrates' is suggested as a ("lame") choice for 'F' which takes the singular term and joins it to a copula, forming a predicate.

(4) The predicate suggested in (2) is acceptable as a translation of 'being Socrates' if and only if it is taken to mean the same as 'Socrates' and to be substitutable (no conditions specified) for 'Socrates' in 'x is Socrates'.
(5) The predicate suggested in (3) clearly fulfills the conditions of (4), on the grounds that \((x)(x = x)\).

(6) If the predicate suggested in (2) is accepted as a translation, then it means the same as the predicate suggested in (3) because of the transitivity of identity.

The best that a suggested predicate can do is to mean the same as the singular term it is intended to translate. It cannot possibly mean as much the same (to be deliberately paradoxical) as a predicate using the exact word(s) of the singular term. The latter kind of predicate is synonymous on any criterion of synonymy.

The "less lame version... differs at most in expository value..." That is, a translated predicate gives information that a particular singular term means the same as a particular description. Identity is seen in this passage as the limiting case of synonymy, but no one would claim that "'Socrates' means the same as 'Socrates'" has any expository value. However, whatever information is conveyed by a translated predicate is irrelevant; the predicate is acceptable to the extent to which it "means the same" as the singular term. One might expect Quine to conclude that the "lame" predicate is preferable to the "less lame", on grounds of equivalence, and because his program is not tied to a theory of knowledge for which "expository value" is wanted. However, he is only arguing for the acceptability of these predicates despite their artificiality.

In Word and Object (1960) the 'is-' type of predicate is not a second-best, but has become the model for taking a singular term into a predicate. Starting with the case where the singular term is in "purely referential position", Quine shows that, by substitutivity of identity and a few moves in elementary quantification logic, a sentence containing
a singular term is equivalent to a sentence of the form 
\[(\exists x)(x = a \text{ and } \ldots x \ldots)\]. The singular term appears only as \(= a\), which, "taken as a whole is in effect a predicate, or general term." Quine shows that non-referential occurrences of a singular term can be so paraphrased to be accommodated to the same procedure. These paraphrases involve placing the singular term in referential position at least "with respect to its immediately containing sentence." We will stay with the simple case because it illustrates the non-translation of singular terms as well as any other.

A singular term can be transformed into a general term without verbal translation because it can be moved into predicate position according to permissible transformations of the logic. Quine speaks of "manoeuvering" or "getting it down to" predicate position. That this can be done, and that doing it removes the problems of reference and existence attendant upon singular terms, justifies the procedure. However, Quine does not suggest an indiscriminate use of the elimination of singular terms in this way.

On the contrary, we suddenly find ourselves face to face with the "purposes of the paraphraser" as Quine reminds us that "on pain of introducing new problems of analysis of general terms" we should limit the reparsing of singular terms "to those singular terms that have no internal structure we care to perpetuate." Which terms these are is "purely relative to varying projects in hand", "the particular needs of the argument or investigation that we may imagine ourselves engaged in." In other words, we should move a singular term 'a' into the position \(= a\) only if we are not interested in the meaning of the
term, however complex it may be. Such a singular term is "simple", because we choose to deal with it as an unanalyzed whole, and Quine rather mischievously suggests calling it a "name", in the sense of a proper name, whose meaning in natural language we also choose to ignore for most purposes. If our purpose seems to call for analysis of a singular term into a conjunction or disjunction of general terms, there are of course no guidelines here for going about it. The selection of particular general terms in such cases will also result in the elimination of singular terms, according to the same procedure with respect to quantification and identity, but not with respect to "maneuvering" the singular term into predicate position.

We cannot very well ask, 'Well, what is the purpose of a paraphraser in this project?' because, as was noted above, the scope of the reform effected here is so wide that innumerable projects can be carried out within its terms. However, Quine himself attempts to ask if, in achieving the elimination of the undesirable existence claims of singular terms by reparsing singular terms as general terms, and thus restricting referential position to variables, other useful functions of singular terms have been tampered with. His conclusion is that

Directness aside, no losses are sustained.
It can be shown that everything that used to be demonstrable or deducible from given premisses when 'Socrates' was manipulated unquestioningly as a singular term is still demonstrable or deducible from those same premisses with the added help of the uniqueness premiss '(\exists y)(y is Socrates and y only)'

... when 'Socrates' is reparsed as a general term. 135

The function of an expression, according to this statement, is its role
in a deduction. A given expression, regimented with respect to its singular terms, still performs its function in a deduction if it permits the same and only the same inferences to be made. An expression regimented by this procedure will not permit false conclusions to be inferred from true premises, nor true conclusions from false premises. It is the case that the class of statements which exhibit a non-denoting name in referential position will come out false after regimentation, whereas they were uneasily neither true nor false previously. "But this was a purpose of the re-parsing." Thus, one can claim for the elimination of "simple" singular terms — and it can easily, if tediously, be shown — that in those cases where the content (or meaning) of the singular term is irrelevant to the deduction in which an expression containing it appears, Quine's transformation of singular terms directly into 'is—' predicates does not adversely affect the function of the containing expression.

Quine deals with an objection that perhaps "the purport of uniqueness" is part of the meaning of a simple singular term, a "name". Quine grants that this "may be conceded to be somewhat intelligible, whatever its cogency," a rather back-handed concession, but reminds those who would press this point that some general terms also "obey laws that seem accountable to the meanings of the terms and not to contingent fact" (cf. the symmetry of 'cousin'). Nothing prevents a weak implication of uniqueness ("one at most") from being part of the "very meaning" of some general terms. Strong uniqueness ("one exactly") has, for good reasons, been externalized and made fully explicit in the formula, '($\exists y)(x) (x \text{ is-Socrates if an only if } x = y)$.'
One wonders why Quine is willing to grant this much in the way of "meaning" to "names". Singular terms may be treated as "names" and reparsed as general terms by 'is-__' predicates in cases where the resulting predicates are "no more than dummy predicates, blanks in a sentence diagram." One can, at increased length but with no logical losses, transform 'Socrates' from singular term to predicate position, supplying the existential quantification and the uniqueness premise, in deducing 'There was a philosopher who drank hemlock in 399 B.C.' from 'Socrates was a philosopher, and he drank hemlock in 399 B.C.' The use of what is in ordinary grammar a proper name may lead us astray here. Of all the range of singular terms and singular descriptive phrases, we may choose any to transform, treat any as "names" in this legislated sense, "that have no internal structure we care to perpetuate." Such singular terms can be "systematically spotted by notational form" after we have chosen them and regimented the expression accordingly, but not before. We are not dealing only with examples like 'Socrates'. We are, however, dealing only with examples of singular terms whose meaning is not relevant to the argument in which they appear.

Consider the deduction of 'Someone wrote both Waverley and Ivanhoe' from 'The author of Waverley wrote Ivanhoe,' an exercise proposed by Quine in Methods of Logic. Not actually to object to asserting that an 'author' can be said to have 'written' a work of which he is the author, it is clear that this deduction depends for its validity on a meaning-relation between 'author' and 'wrote'. 'The author of Waverley' cannot be taken into predicate position as 'is-author of
Waverley' without changing the function of the sentence in the deduction from one which establishes the conclusion to one which does not. One could only deduce 'There is someone who is—the author of Waverley and who wrote Ivanhoe.'

To sum up Quine's most recent formulation of the elimination of singular terms:

(1) All singular terms are eliminable.
   a) Some singular terms are eliminated because of the problems they create.
   b) All other singular terms may be eliminated by the same general procedure for the sake of the simplification of logical theory and the clarification of ontological commitment.

(2) "Simple" singular terms are trivially eliminable by the formation of 'is-' predicates.
   a) Simplicity is relative to the deduction in which the expression containing a singular term occurs.
   b) Singular terms which are not "simple" should not be eliminated via 'is-' predicates.

(3) Singular terms which are not "simple" may be eliminated if the paraphraser finds it perspicuous to do so.
   a) How this is to be done, "the nature of the more minute analysis", depends entirely on the paraphraser. 145
   b) "We are reminded once again that paraphrase makes no synonymy claim." 146

Quine has set up a procedure for regimenting a vast class of expressions in a natural language, and one might agree that the scope of this procedure is justified by the considerations referred to in (1)b. However, he disclaims responsibility for that part of the regimentation which involves questions of meaning and synonymy. Russell was not much more satisfactory with respect to predicate translations. He merely took it for granted that translations could be made and that the analyzed and pre-analyzed propositions (with respect to a determinate interpretation of questions of existence) meant the same. Quine also assumes
that there are cases where predicate "translations" will be made, but he might not even agree that there are limits to what would be considered a "minute analysis" of a particular general term, although it seems unreasonable to allow translation "ad libitum". Quine has no standard within his theory of language for setting limits. He has insisted that the choice of predicates is "wholly casual". All that he definitely has is stimulus synonymy which, it is quite clear, cannot even begin to serve the purpose.

We are left with the uneasy situation of a regimenting procedure with many advantages, but without standards of adequacy for the resulting regimented expressions. In cases where "more minute analysis" is judged by the paraphraser to be required, one can determine whether the regimented expression has the same function in a deduction as the pre-regimented expression only by understanding the meanings of the terms in both. There are no grounds for this understanding within the theory. Furthermore, the paraphraser is in no way constrained by someone else's evaluation of what he has done.

A foggy appreciation of this point is expressed in saying that there is no dictating another's meaning; but the notion of there being a fixed, explicable, and as yet unexplained meaning in the speaker's mind is gratuitous. The real point is simply that the speaker is the one to judge whether the substitution of S' for S in the present context will forward his present or evolving program of activity to his satisfaction. Does the "real point" really provide a sufficient criterion of adequacy for an activity that purports to clarify language for scientific purposes?
C. The Program of Regimentation Reconsidered

Regimentation, as presented by Quine, is a technique for moving from a sentence in a natural language to a formula in a logic with rather meager resources. The purposes of regimentation are (1) clarifying or eliminating ambiguity; (2) making it possible to bring to bear some of the mechanical advantages of the logic; and (3) simplifying theory by reducing the variety of idioms of natural language to those few constructions and modes of combination allowable in the logic.

The first purpose is also served, more informally, by piece-meal paraphrasing within the natural language or with some help from logic. If it were not for the other two purposes, full-scale regimentation would not be called for. The second purpose is important although, as Quine points out in "On the Application of Modern Logic," we are far from being in the position of physics vis-à-vis mathematics when we draw on the resources of logic to mechanize deductions. We are at such an early stage in the analysis of our concepts that we can use the resources only of elementary logic. The advantages of more powerful logical tools await the refining of our ability to paraphrase sentences. (This can be seen by noticing how simple are the logical operations used in the notational parts of most philosophical arguments.)

The third purpose is repeated often enough, but never made as clear as one might like. Is the "theory" that is simplified by regimentation logical theory? scientific theory in its most general outlines? the theory of the referential-predicative part of language? or perhaps not theory but theoretical language? Are these perhaps not alternatives
but related to one another?

We have noted earlier that regimentation is not system construction. Quine rejects system construction largely because of the artificiality and arbitrary limitations of a system with respect to allowable terms. The situation he envisions is one in which the philosopher keeps moving between severely regimented language, in which the principle of minimum resources prevails, and fairly ordinary language, with its conceptual untidiness but greater directness and brevity of expression. This is the stance of Quine's in which he shows most strongly the influence of Neurath, whose often-quoted simile about rebuilding a ship on the open sea appears at the beginning of *Word and Object*.

In the same paragraph in which the simile appears, Neurath writes,

> Es gibt kein Mittel, um endgültig gesicherte saubere Protokollsätze zum Ausgangspunkt der Wissenschaften zu machen. Es gibt keine tabula rasa. Wie Schiffer sind wir... etc... etc... 152

* There is no way to produce absolutely unadulterated protocol sentences as a point of departure for the sciences. There is no tabula rasa. We are like mariners who must rebuild their ship on the open sea, without ever being able to bring it into dock and erect it anew from the best materials.

Quine adds the commentary, "Our boat stays afloat because at each alteration we keep the bulk of it intact as a going concern." With respect to "theory-building", "we all must start in the middle."

And, one could add, in a sense we all must stay in the middle. A closed system, which is what a formalization is, can do as little justice to how theory relates to the world as an insistence that any departure from ordinary usage is to be avoided. Neurath writes, "The imprecise
'conglomerations' [unanalyzed terms from ordinary language] are always somehow part of the ship. If the imprecision is restricted in one place, it can very well turn up strengthened in another."

Regimentation does have systematic consequences, related to purposes (2) and (3). However, with respect to (1), clarifying or eliminating ambiguity, there is every reason to think that we are entitled to consider the results of regimentation one by one, that we can compare the ambiguous natural language sentences with their clarified counterparts in logical notation, and do so at a sub-systematic level. Judging a sentence to be ambiguous involves singling it out from the corpus of sentences of the language. There is no such thing as an ambiguous language. Quine considers it a distinctive feature of languages that "sentences are thought of as conveying meanings severally." It is proper, therefore, to expect that, in order to judge particular applications of regimentation we can match sentences with formulae, and that not all the formulae which could be offered will be acceptable. This last point is not as silly as it seems. If any formula at all were acceptable as a clarification of a given ambiguous sentence, then "clarification" would trivially be merely the replacement of the sentence by any formula. This is not what we normally mean by clarification. We normally mean that a sentence which was subject to more than one interpretation has been reformulated, or put in linguistic or nonlinguistic context, in such a way that it is now subject to only one interpretation, and that that interpretation is the one intended, or probably intended, by the speaker or writer.

The Quinian matching of sentences with formulae is one which
rejects a criterion of "meaning the same" in favor of a criterion of "functions the same". Both criteria need to be qualified by "in some contexts" because a sentence would not be a candidate for regimentation if there were not some problematic contexts to revise or do without as well as some important contexts to preserve. The regimented sentence cannot "mean the same" without qualification because, at the very least, regimentation eliminates ambiguity. It also cannot "function the same" without qualification. Quine's rejection of a criterion of meaning goes back to the "Two Dogmas" arguments: he can find no criteria for "meaning the same" which are both defining and independent of other undefined concepts.

To function the same in a deduction, two sentences must be inferable by the same rule of inference from the same premises, and must permit the same inferences by the same rule of inference. If these requirements are compatible with two sentences having different (intuitive) meanings, so much the worse for meanings, on Quine's view. According to the "maxim of the identification of indiscernibles: Objects indistinguishable from one another within the terms of a given discourse should be construed as identical for that discourse."

"Function", as a matching and individuating criterion, will result in quite a different correlation of sentences to sentences, or sentences to formulae in the logic, than "meaning". However, for Quine, the advantages all lie on the side of "function" because "function", in this restricted sense of "... in a deduction", is an extensional criterion with all the straightforwardness that "meaning" so notoriously lacks.

Perhaps one should now put a fundamental question to Quine: Why
is regimentation a good way to clarify ambiguous sentences in a natural language? Quine's answer might include these points:

(1) What is ordinarily called 'grammar' in a natural language is inadequate to the task.

(2) Paraphrasing ordinary language sentences into logical notation yields "a sharp analysis of concepts, a revelation of fundamental structures" previously undetected; logic can be a "source of syntactical insights".

(3) Sentences paraphrased into logical notation can be handled by "methodical manipulation...according to fixed rules of algorithm." The advantages of efficiency, speed and accuracy are similar to those of machine calculation.

(4) Regimentation offers the systematic value of simplicity. As argued in "Simple Theories of a Complex World," and in Word and Object, the simpler theory, or base, accounts for more data, establishes more interrelations, has a wider margin for error, and is therefore more reliable than a less simple theory.

(3) and (4) are, in effect, versions of purposes (2) and (3) of regimentation. (See above, p. 71.) Let us grant that these are advantages. (1) is an interesting claim. Quine is only one in a long tradition of analytic philosophers who, without a very careful study of linguistics, conclude that the "logic of language" and the syntax of a language have almost nothing in common, and may even be at cross-purposes. This tradition, which includes Frege, Russell, and Wittgenstein, takes it as conceded that traditional syntax makes distinctions which are unimportant to the logical analysis of language and overlooks distinctions which are essential to this analysis. One can hardly chastise these philosophers for their assumptions since grammar in the mold of Latin grammar and its successor, descriptive taxonomic grammar, were not very promising as tools of logical analysis.

Transformational grammar, on the other hand, decidedly claims to include in its province the clarification of ambiguity. This rela-
tively new approach to grammar has appropriated the distinction between surface, or superficial, and deep, or underlying, grammatical structures which philosophers of "logical grammar" have previously used to demonstrate the insufficiencies of grammatical (taken to mean surface grammar exclusively) analysis. The deep structures determine the semantic interpretation of a sentence. In fact, the border between syntax and semantics is not drawn, and every attempt is made, by Chomsky at least, to include as much as possible of semantics within the territory of syntax. This is done by the use in syntactic analysis of such categories as 'abstract', 'human', 'animate', as well as 'transitive', 'pre-adverbial', etc., each associated with context-limiting rules. If this could be satisfactorily worked out, it would be a way, within syntax, of accounting for many kinds of ambiguity. It is interesting that Chomsky is trying to include so much within syntax; his distrust of 'meaning', of "unanalyzed semantic intuition" is not far from Quine's, and his attempt to rely on the syntactic component, identified by purely formal features, is perhaps parallel to Quine's choice of an extensional logic for the regimentation of language.

Whether transformational grammar has the means to clarify all the ambiguous constructions which are of interest to philosophers remains an open question. It is certainly not yet established, and when Word and Object was written, it was hardly a possibility. On any account, Quine has opted for a program of clarifying language which proceeds directly from sentences in natural language to formulae in logic, and the claim that we have to consider is the one made in (2):
(a) sentences in natural language can be nontrivially transformed into logical formulae; and (b) this procedure does clarify sentences of a natural language.

Quine, as we know, assumes (a) and is enthusiastic about (b). Another way of stating this claim is to say that truth-functional predicate logic with quantification and identity can be considered a suitable, and rewarding, model for either English alone, or for all — or some — natural languages. Taking up the questions of the restriction of the domain of application of the model first, is it plausible to restrict the domain of application to English? Quine deals only with English. He introduces regimentation only after the principle of indeterminacy of translation has undercut an interlinguistic concept of meaning beyond stimulus meaning of observation sentences. He writes that "the artificial notation of logic is itself explained, of course, in ordinary language" and that sentences, regimented in "canonical notation", are a "sub-class of the eternal sentences" of the language in question.

On the other side, Quine's rejection of the putative "prelogicality" of some cultures, and his attribution of this apparent characteristic to "bad translation," seem to be evidence for a belief that this logic can be a model for all languages. Only the truth-functional part of logic can be behaviorally determined and translated, however; indeterminacy sets in for the predicates and even for the quantifiers because the criteria for these go "beyond extension." One might conclude that every language could be mapped on to the fuller logic, as English can be, but that it is not possible, because of indeterminacy,
to correlate these mappings interlinguistically. It would be, if true, an astounding linguistic universal, but perhaps an empty one. If the logic is interpreted in the language which is to be mapped on to it, then, without further restrictions, the universal might be only that the particular set of symbols which constitute the logic can serve as a model for every language individually. But any set of symbols, with an appropriate interpretation, can serve as a model for any domain.

Returning to the main part of Quine's claim, that truth-functional predicate logic with quantification and identity can serve as an illuminating model for English (at least), let us consider what is implied by it.

(1) The English language, or at least the assertive part of it, is the field of investigation we are trying to illuminate, and logic, as specified above, is the "more familiar, or better-organized secondary domain."

(2) There is some structural relationship between the original and the secondary domains. According to Black, it should be "identity of structure," and according to Swanson, the property of symmetry that identical structures possess is not wanted because a good model has "surplus sub-structure", is richer in structure than the original domain. Swanson argues that a model identical in structure to its domain of application is "conceptually vacuous".

(3) "Explicit or implicit rules of correlation are available for translating statements about the secondary field into corresponding statements about the original field."

(4) Inferences that can be made in the domain of the model can
be translated by (3) and applied to the primary domain.

(5) These inferences can be "independently checked" in the primary domain.

If these conditions are satisfied, the logic proposed is a successful model for English.

Can English and logic, respectively, take the roles of field of investigation (primary domain) and model (secondary domain), according to the above requirements? There is some structural relationship between English and logic, but clearly not structural identity. Is the logic we are contemplating "richer" in structure than English? It depends on what we mean by "structure". The logic is "more familiar" and "better organized"; rules of inference are explicit and inferences are more secure. However, Quine often writes as if we are to map the more complex structure, English, on to the simpler, logic. Explication, after all, is elimination which still preserves desired functions. The demonstration of the eliminability of all singular terms illustrates as well as anything that mapping, in this case, proceeds from the "richer" to the simpler.

On the other hand, if we consider structural relationships rather than structural elements, the logic can be seen to be richer than English. The number of elements and the number of relationships are correlative in a system: the fewer the elements, the greater the number of relationships that can be elaborated in terms of them. By mapping English, with its multiplicity of idioms and relatively few relationships, onto a logic with a very small set of elements highly interrelated, one is mapping the simpler on to the more complex struc-
Condition (3), which calls for "rules of correlation" between the domains, presents the greatest problem for the program of regimentation. If this, and related condition (4) are not satisfied, we need not consider (5) at all because we can not get that far. There are "explicit or implicit" rules for translating the logical connectives, and probably also the quantifiers (since we are operating within English now). However, the sweeping away of all but general terms, and the lack of guidelines about the degree of minute analysis of general terms, deprive us of even "implicit" rules of correlation between the two domains. There will be cases where the standard of "functioning the same" works perfectly well, but there will also be cases where "function", in the narrow sense in which it is here used, will be unsatisfactory because it does not make fine enough distinctions. When predicates appear vacuously in arguments, there is no problem in transferring inferences from the secondary to the primary domain, and in checking them by "function in a deduction". When predicates appear non-vacuously, however, we can neither translate nor check with assurance.

Because of the freedom allowed the paraphraser with respect to predicates, Quine rules out a standard of synonymy between sentences and paraphrases. Conversely, the difficulties that Quine sees with synonymy remove any grounds for restricting the paraphraser's freedom. A regrettable consequence of this is that there will be cases where the sentence and the paraphrase are functionally equivalent by fiat of the paraphraser alone, and this greatly weakens the "rules of correlation" between the domains and, therefore, the claim for the model.
CHAPTER III

ALTERNATIVE APPROACHES TO QUINE'S PROBLEM

The program of regimentation is justified by its alleged efficacy in eliminating ambiguity from expressions of a natural language, and on the other grounds discussed in the previous chapter. Regimentation is not required because of a principled indeterminacy of translation of terms. Quine does not justify turning to logic by the failure of full-blooded concepts of meaning in natural language. However, the ways and means of regimentation are affected by this failure. Quine's denial that there is a relation of sameness of meaning between the expressions in a natural language and the logical formulae which regiment them is a direct consequence of his belief that meaning is an insufficiently precise and operational concept. His restriction of regimentation to one language, and his denial of the possibility of interlinguistic meanings for logical formulae, are directly related to indeterminacy of translation. A closer, critical examination of indeterminacy of translation, therefore, may well have consequences for the procedure of regimentation, if not for its rationale.

Indeterminacy of translation deserves another look on its own account as well. It is a "shocking" doctrine, appearing to undercut supposedly empirical labors in linguistics and anthropology, as well as denying any empirically sound concept of meaning beyond "stimulus meaning". It has received relatively little response in the professional literature. Often, when it is mentioned, it is dismissed without serious counterarguments as something too foolish and obviously false to bother with. Sometimes Quine is taken to be proposing "stimulus meaning" as the concept
of meaning adequate for describing or translating a language. This is an easy enough straw man to knock down, but it is certainly not Quine's position. Quine holds that, if you are a behavioral empiricist, you cannot defend more than a "behavioristic ersatz" for "intuitive" semantic concepts. "Stimulus meaning", the behaviorally justifiable substitute for meaning, will not take you as far as meaning; therefore, there is indeterminacy of translation.

At the beginning of Chapter I, it was asserted that Quine's behaviorism and his "Two Dogmas" argument against an exhaustive analytic-synthetic distinction were the two strands in his thought chiefly responsible for the principle of indeterminacy. It might be interesting to examine some criticisms of these positions, and some alternatives to them, to see how much and what kind of change in Quine's behaviorism or in his view of analyticity would be necessary to produce a change in the doctrine of indeterminacy of translation.

These two strands are obviously not completely independent of each other. A linguist wants to investigate whether 'brother' and 'male sibling' mean the same for a sample of speakers of English. This is the linguist as "empirical scientist", and the methods allowable to him depend on the behaviorist strand. But what is to count as 'meaning the same'? Affirmative answers to the question under investigation will vary with the respondents' interpretation of 'meaning the same'. The linguist's operational definition of 'meaning the same' "in terms relating to linguistic behavior" will depend on his choice of interpretation for 'meaning the same'. The decision on a definition of 'meaning the same' depends on the analyticity strand. Different behavioral tests will be applicable
to different theoretical criteria.

We can test empirically for the meaning of 'meaning the same' if we take as data that certain pairs of words are synonymous and other pairs are not synonymous. We can test for the synonymy of particular pairs of words if we fix on an interpretation of 'meaning the same' and devise a behavioral criterion for it. We cannot very well test in the absence of any fixed points.

8

A. Behaviorism, Language-Description, and Meaning

One can distinguish a description of language-learning from a language-description. According to Quine's account, learning a language is being conditioned, "trained", by society, by means of rewards and penalties, to respond correctly to stimuli. As with most stimulus-response theories, learning is viewed as a rather passive process. Because one is hard put to account for the learning of language, even the "first learning of words", by simple association of words and things, albeit reinforced by rewards and penalties, Quine posits a "sort of prelinguistic quality space" as part of the infant's innate equipment. This is held to be a "prior tendency to weight qualitative differences unequally."

Indeed, the problem for an associationist theory is that "a dozen reinforcements of his response 'Red', on occasions where red things were presented, would no more encourage the same response to a thirteenth red thing than to a blue one," but one would not think that positing such a specific innate discriminatory ability would be preferable from an empiricist's viewpoint than abandoning association for a more creative process. Quine puts it a little differently elsewhere: one learns a word "by induction from observed instances where it is applied," and therefore
the instances must be sufficiently alike to "afford a basis of similarity to generalize upon." "Prelinguistic quality space" is not mentioned here; the viewpoint is external instead of internal (i.e., the word must only be applicable in cases where... etc.; who determines and enforces this?), and the criterion for "sufficient" likeness is not made explicit.

In the case of radical translation, the linguist can not count on induction to determine even the extension, much less the intension, of a term. The likenesses that he perceives may not be the relevant likenesses, and he has no way to discover his error. If the child learns his native language under the same conditions in which the linguist attempts to translate that language, what accounts for the child's succeeding and the linguist's failing? Surely Quine would not want to defend children being born with a prelinguistic conceptual set for learning a particular language.

Quine grants, furthermore, that, in principle, indeterminacy may exist within a language because of the individual language-learning histories of the speakers. How, we may ask, do speakers come to speak a common language? What answer can Quine give that is not metaphorical, in terms, for example, of "trimming" shrubs? Language is "the complex of present dispositions to verbal behavior, in which speakers of the same language have perforce come to resemble one another;" "it is to the interests of communication to efface" the individual variations in language-learning. His answer is that it is a matter of practical necessity: if they didn't come to speak the same language, they couldn't speak to each other. However, this is not an explanation.

The point we are getting to is this: Quine rules out a difference
in kind between the case of the linguist and the case of the child. He even grants that a linguist can fluently learn the language he is out to translate, particularly if he "simulates the infantile condition". But if the child and the linguist are equally held to the same kind of evidence, how can either one learn a language that goes beyond "stimulus meanings"?

Jonathan Cohen asks how, using Quine's standards of acceptable evidence, it could be determined that

1. anyone can adequately learn to speak and understand his native language?

2. there is a speech community beyond observational sentences and their truth-functional compounds?

Quine does not regard (1) and (2) as doubtful. He assumes that a child born in a speech community will, under normal conditions, learn to speak and understand the language of that community. Although he makes a point of the fact that, in learning a language, each person is conditioned differently, these original associations are not remembered, and the result is the "trimming" of each speaker into the shape of the language. He also assumes that one can speak of a single language in one speech community which goes well beyond those sentences translatable by stimulus meanings.

Quine explicitly presents indeterminacy of translation as an intra-linguistic as well as an interlinguistic problem. Two speakers of a language may interpret a given sentence of that language differently; that is, they may choose different paraphrases of it or infer different conclusions from it (by equally valid procedures). One native speaker may impute "unimagined views" to another which nevertheless conform "to all his dispositions to verbal response to all possible stimulations."
pite this acknowledgment of indeterminacy within as well as between languages, Quine sees no problem at all in distinguishing the intralinguistic and interlinguistic cases. The former, the "domestic" case, has no serious consequences. "Positivistic reasonableness" tells us "that if two speakers match in all dispositions to verbal behavior there is no sense in imagining semantic differences between them." It is in the interlinguistic case that "semantic indeterminacy makes clear empirical sense." Quine simply bypasses the question of the individuation of languages, although it would seem closely related to indeterminacy. He assumes that one can speak of a speech community, a language, and that indeterminacy does not require positing one language per speaker. In the case of radical translation, the linguist attempts to translate a totally unknown language, not the set of languages spoken and understood by the speakers within a certain geographical boundary.

Given Quine's assumptions about the ability of speakers to learn a common native language, plus his granting that a linguist can learn a foreign language as a child learns his native language, it would not seem that the difference between the native and the foreign cases were more than a difference in degrees of difficulty.

Let us go back for a moment to the case of the linguist's success in learning the language fluently which he remains unsuccessful in translating. It is a little surprising that Quine is willing to grant this possibility, but, as Cohen points out, Quine does not want to conclude that a language is unlearnable. If one denied the possibility of learning the highly exotic language we are dealing with to the mature linguist, despite his experience, we could always arrange for the linguist's infant
to be born and spend his first decade among native speakers, learning the language as any other infant would, and learning English as well from his English-speaking parents. It is significant that the test of a bilingual's adequacy in both languages is "observing the fluency of his communication in both communities." It would not be enough to test the correctness of his stimulus meanings against the standard of the community. Quine assumes that there is more to the language than this, and therefore what one tests is his ability to get along with other speakers of the language in a variety of language-using situations. Does he make himself understood? Does he appear to understand what is going on? Does he respond in nonanomalous ways, and do the others behave normally toward him? This is not terribly precise, but it is reasonable enough.

Cohen's criticism is that, with the tools allowed by Quine, the linguist could not judge either his own child's successful learning of the language unknown to him or any other speaker's mastery of it. The test of "fluency of... communication", proposed by Quine, is not justified by his own standards of evidence. The linguist can only, on those standards, certify the existence and meaning of "sentences asserting the immediate presence of conspicuously segregated objects... and their truth-functional compounds."

In the same way, the language-learning procedure that Quine endorses shrouds in the mysterious perception of similarities and analogies everything beyond the learning of the first few words. A passive process of association of words and things, or words and other words, cannot account for how one learns to isolate the correct properties picked out by words, nor for how one learns to understand and use a word in a variety of contexts, including in new contexts, nor for how one learns to use and
understand a word discriminately with respect to belief and other intentions. A natural language, of the richness that Quine as well as most people take for granted, cannot be learned by a process of conditioning.

In describing the learning of a language, one can plausibly reproduce utterances in the order learned, and rely on one's psychological theory of learning to explain how these utterances are related to events within and outside the learner. In describing the speaking of a language, as a native speaker or a linguist might, one is concerned to do more than that. Short of attempting to translate, or give equivalences for, expressions of the language being described, one will attempt to correlate things said with observable situations. One can begin by taking "observable situations" in the large, rather than trying to narrow them down to the "prompting" elements. These situations will then not only be the presence of objects which appear to prompt particular utterances, but will include a variety of human actions and inferred purposes, attitudes, expectations, etc. In observing infants learning to speak, we rightly fail to impute a complex conceptual background to what we directly observe; we assume that children acquire a language and a conceptual framework together. We therefore have only the very limited evidence of uttered noises plus the presence of objects, grimaces, clutchings, movements, our knowledge of the time of the last feeding, and so on.

It is true that the expression of inner states by outer movements may vary drastically in different cultures. Error is likely, and complete eventual success doubtful. The question is whether, when describing the language of mature speakers, one is required by sound empiricism to rule out, at the start and forever, any evidence but the evidence for the
stimulus meanings of observation sentences? One can give up stimulus-
response associationism without giving up the view that "Language is
a social art. In acquiring it we have to depend entirely on intersubjectively available cues as to what to say and when." Let us take the
strong view that there are no specifically linguistic innate mechanisms
required by a theory of language. Everything that becomes a part of the
learned language is learned by "intersubjectively available cues". Let
us even grant Quine that "entification begins at arm's length," that
ordinary physical things are in "sharpest focus", and that words designating them are learned first. None of this commits us to limiting our
description of a language to a description of the conditions for the use
of those words. Not all "surface irritations", not all of the "past and
present barrage of non-verbal stimulation", derive from those ordinary
physical things whose paradigms are stones and apples and rabbits. To
doubt that we can always infallibly identify what someone is doing is
perfectly right; to entertain a general doubt that we can ever identify
what people do, intend, expect, etc. is not rigorous empiricism but un-
reasonable skepticism.

I would like to take up a few examples of this broadly-conceived
way of describing a language. The examples are very different from one
another, because the writers proceed from different starting points for
different purposes. The first is Stanley Cavell's essay, "Must We Mean
What We Say?".

The significance of the title is as follows: Cavell is discussing
statements about the use of ordinary language of the type, "When we say,
"...", we mean, imply, suggest, etc. that ..." (where the second blank is not merely a paraphrase of the first, but a specification of the "pragmatic" implications, presuppositions, etc.). He is asking in the title, and examining in the essay, whether such statements are necessary truths. He concludes that they are, although in a special sense. On the one hand, they are "obviously" not analytic, and, on the other, "the question of evidence is irrelevant." Their "necessity" resides in the normativeness of "ordinary use itself." On the existence of a relationship, he argues that something does follow from the fact that a term is used in its usual way; it entitles you (or, using the term, you entitle others) to make certain inferences, draw certain conclusions. (This is part of what you say when you say that you are talking about the logic of ordinary language.) Learning what these implications are is part of learning the language; no less a part than learning its syntax, or learning what it is to which terms apply: they are an essential part of what we communicate when we talk. ... We are, therefore, exactly as responsible for the specific implications of our utterances as we are for their explicit factual claims. And there can no more be some general procedure for securing that what one implies is appropriate than there can be for determining that what one says is true."  

On the necessity of the relationship,

If truth consists in saying of what is that it is, then (this sense or source of) necessary truth consists in saying of what is what it is.  

Max Black subscribes to this point of view, and, in "Presupposition and Implication," draws some interesting contrasts between "what is said in so many words" and what is implied". He interprets implication in these contexts nonlogically, as neither material implication nor strict implication, but rather as belonging "to the same family as 'suggest,' 'hint,' 'insinuate' ..." These are "two modes of expression" and
the latter, "implicit communication", is important "for an adequate conception of language" because the former, what is "said outright", is interpreted so strictly as only a report of the speaker's words in indirect discourse, or even, "under pressure" of a dispute, quotation of the speaker's words. This omits such features of utterances as (significant) silences, "ellipsis, stress, intonation, sentence construction, choice of words, or allusion" which are expressed by specifiable linguistic devices.

Sometimes one checks whether some implications of the utterance were intended by questioning the speaker. He may "disclaim some responsibility for his implications." "To the extent, however, that the speaker uses formal linguistic signals of implications, [alternatively, "conventional rules for implications"] he forfeits the option of disclaiming the implication." One can distinguish in some cases between what the speaker intended to imply and what his words implied, where the speaker is responsible for the implications of his words, like it or not. This is close to Cavell's normative "necessity".

The "normativeness" of the ordinary use of language that Cavell has in mind is not to be opposed to "descriptive" nor exemplified by "prescriptive utterances". The statements of the type Cavell is discussing are normative because they are action descriptions, and the "most characteristic fact about actions is that they can go wrong, that they can be performed incorrectly." Something only counts as an assertion, a question, a command, if it is performed in a certain way. They are rules, in the sense that they "tell you what to do when you do the thing at all [not] how to do the thing well, with skill or understanding." The latter sort of directions are expressed by prescriptions, principles.
The ordinary language philosopher, or indeed any native speaker, is "entitled, without special empirical investigation," to make these assertions about utterances and their meanings and implications. A linguist, on the other hand, can make a corresponding assertion, but for him it is the result of empirical investigation, and therefore not necessarily true, not a rule. The two assertions have the same truth-conditions, and describe "the identical state of affairs," but "the question of evidence is irrelevant" to the native speaker because he knows that his assertion is (necessarily) true because he has learned his language.

Cavell's chief purpose is to deny that the ordinary language philosopher who is a native speaker requires the empirical apparatus of questionnaires, as Mates has suggested in "On the Verification of Statements about Ordinary Language," to go about his job. Native speakers "do not, in general, need evidence for what is said in the language; they are the source of such evidence." No one would deny that native speakers are the "source" of evidence about usage in their language, nor that, "in general" a native speaker knows how to speak his language. However, usage is not totally uniform within a speech community so that each speaker is not a "source" in the same way that all the speakers together, or a properly selected sample, are. Cavell grants that "to answer some kinds of specific questions" even native speakers may find it wise to conduct surveys.

For our purposes, the Cavell essay is interesting for its strong defense of a close connection between some surrounding circumstances of an utterance-type and the utterance-type itself. Some of these surrounding circumstances: typical ways in which the speaker is prepared to follow up
that utterance with others or with nonverbal actions, typical expectations aroused in the hearer expressed by verbal or nonverbal responses, are eminently observable. Since we know it to be the case that these connections vary in different subcommunities of one speech community, and that they vary even within one subcommunity over time, it is harder to grasp what Cavell means by their necessity. He concedes that there is temporal change in a language but holds that, because it changes "naturally", as opposed to artificially (i.e., by stipulation), ordinary use at any given point in time (how small?) is normative. He concludes, both mystically and tendentiously, "To see that ordinary language is natural is to see that (perhaps even see why) it is normative for what can be said."

The next position we will consider diverges from Cavell on the question of the normativeness of language-descriptions, although norms have a place elsewhere in this fully worked out theory of meaning. To understand what Jonathan Cohen has to say about language description and concepts of meaning in The Diversity of Meaning, it is necessary to understand the three fundamental distinctions which he makes. (See Figure 1.)

The distinction of levels of discourse is the distinction between talking about particular words or sentences and talking about what is conveyed by particular words and sentences. On the verbal level, a quoted word cannot be translated; on the conceptual level, it can be. On the verbal level for a language-sentence, one discusses syntactic structure, paraphrase and translation; on the conceptual level, one discusses the implications and appropriateness of its utterance.

The distinction between occurrences of words and sentences and words and sentences in a language is a very important one for Cohen. An
## Figure 1

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<th>Level of discourse</th>
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<th>Kind of semantics</th>
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occurrence of a word is not a word-token because the duplication of a particular utterance-word (by recording or writing) does not double the number of utterance-words as it doubles the number of tokens. An occurrence of a word or a sentence is its utterance on a particular date in particular circumstances which render its meaning fully determinate and fixed once and for all. A word or sentence in a language, on the other hand, has a meaning which is a "changeable property" of it. One cannot specify the meaning of any language-word timelessly, because words change their meanings through time, nor even the correct meaning of a language-word on September 1, 1968, because meanings change with the noticeable acceptance of a new use and one can never predict when a new use will be initiated.

Utterance-words and sentences and their conceptual counterparts, saying-words and -sentences and terms and sayings (the last being Cohen's proposed substitute for propositions), are the province of translators and logicians. Language-words and sentences and their conceptual counterparts, culture-words and -sentences, are the province of lexicographers and historians of ideas. This brings us to the third distinction, between de jure, or rule-guided, "timeless" semantics, which is required for occurrences, and de facto, or purely descriptive, "temporal" semantics, which is required for language. Cohen belabors the totally factual, descriptive nature of semantics for language words and sentences and the absence of the slightest taint of normativeness from its generalizations. He is equally adamant about the absolute nonfactualness of semantic rules for the meanings of utterances. It is highly questionable that any empirical generalizations are "purely" descriptive, in Cohen's sense, and that
de jure semantic rules are independent of any and all factual statements, as he claims. However, Cohen feels that much harm has been done in philosophical linguistics by a normative "rules of use" approach to word use, and that a descriptive approach to translation of utterances and to the correlation of logical formulae with sentences is misplaced.

For the semantics of words and sentences in a language, Cohen takes as paradigm the procedure of the compiler of dictionaries, the lexicographer. In arguing against the relevance of rules to this branch of semantics, he points out that "Fowler's Modern English Usage and the Concise Oxford Dictionary were written with obviously different aims in view, even though they had an author in common. The evaluative approach is as pervasive and characteristic in the former as it is rare and superfluous in the latter. ..." In the Preface to the latter work, the authors state their purpose as "on the one hand restricting ourselves for the most part to current English, and on the other hand omitting nothing to which that description may fairly be applied." Once a change of meaning for a word, "an old word in a new use", has gained sufficient currency to be recorded in a dictionary, its use is traced, as far as possible, to its first recorded occurrence. That first occurrence is not treated as a "breach of an old rule or obedience to a new one." One might open a chink in Cohen's impenetrable wall between de facto and de jure semantics here: there is no automatic recording machine which simply ingests all uses of all words and produces an up-to-date list of actual usage. The lexicographer must decide that a new meaning is in wide enough use "to be worth recording". The principles for selecting relevant data are no different in kind for the lexicographer than for any other empirical scientist, and no less normative.
Cohen then considers different ways of discussing word-use in a language in order to determine the best concept of meaning for temporal semantics. The first, and simplest, uses a causal concept of meaning. If there is an observable correlation between sound-patterns and situations and behavior (on the part of both speaker and hearers), then, without taking anything else into account, the linguist can take the meaning of an utterance for a hearer as "its causal property of generating such—or-such responses." He can likewise take the meaning of an utterance for the speaker as a "causal property [of the circumstances] to generate the utterance." Cohen finds this restriction of evidence justifiable in discussing the infant's first learning of words since, in effect, no other evidence is available. The causal concept of meaning, which is substantially "stimulus meaning", can be used here because no fuller concept is applicable. However, we do not continue to think of a word as having two meanings, one for the speaker and one for the hearer, and we do not consider utterances meaningless which are not simply "evoked" by situations.

If the linguist adds to his assumptions that particular utterances are made for particular purposes and that the speakers know which utterances are likely to achieve which purposes, then he can go beyond correlating utterances with situations and describe speakers as "giving orders, conveying information, communicating gratitude," etc., and take "the meaning of each sound-pattern as the normal purpose for which it is uttered." This is a simple purposive concept of meaning. The difficulty with a purposive concept in this form is that each sound-pattern uttered for a purpose is considered independently of every other sound-pattern. A linguist
cannot account for the fact that we relate sound-patterns in reasoning
and often make utterances about other utterances. If some sound-patterns
are taken to be "supplementary of their context," the linguist can
allow for talk about the meaning and truth-value of the remaining sound-
patterns. This amended purposive concept of meaning can account for a
language of a sort, but it is not yet sufficient to account for the
known natural languages. Its main shortcoming is lack of economy.
Utterances are taken as a whole, and individuated by purposes. Learning
such a language, or describing it fully, would be a task of superhuman
proportions, involving "a number of sound-patterns equal, say, to the
number of non-equivalent statements, commands, promises, etc., containing
some four dozen words or less than can be constructed from the five-
thousand odd words of commonest use in mid-twentieth-century English."
This concept of meaning fits rather well the foreign language-English
phrase books which one uses to get by for a brief interval in a foreign
country. Anyone who has ever used one knows how difficult it is always
to find exactly the right sentence, and how helpless one is in an effort
to alter an almost-right sentence without knowing the grammar of the
language.

For an Überwindung of this difficulty, the linguist must break
utterances down into "words" and must attempt to formulate the grammar
of the language. We know that the impossibility of this is just what
the principle of indeterminacy asserts, but we will not consider Cohen's
answer to Quine at this point. The linguist will discover that all words
can appear in different positions in sentences, play different roles, and
that a word's "meaning" is not exhausted by what it means "when uttered
Instead the meaning of a word or common phrase as
dictionaries conceive it is the part it plays in
achieving the normal purposes of the serial arrays
of words in which it occurs. Its meaning is the
function or functions it performs in the utterances
of those who speak the language to which it belongs.

This is a functional concept of meaning, and according to it, the meaning
of a word is described by (1) giving another word or phrase in the same
or another language which performs the same (semantic) function, (2) plac-
ing the word among the classifications of the grammar in order to give its
syntactic function, and, on the conceptual level, giving the implica-
tions and appropriate contexts of the use of the word.

A number of different points are emphasized in this account. Each
single word in a sentence does not achieve its own distinctive "normal
purpose", part of the "normal purpose" of the whole sentence. This would
be the image of a sentence as a string of beads, where a word is added to
add some partial purpose to the total purpose. The functional view stresses
the interaction of words in a sentence: a word's meaning in a given sen-
tence is affected by its syntactic category (which may not be uniform for
one word-shape: e.g., 'fly' as both noun and verb), its position, endings,
and the other words with which it is associated.

A functional concept of word meaning depends on being able to des-
cribe "the function or functions [a word] performs, as a matter of fact,
in the sentences of [the] language." The syntactic function can be des-
cribed if the word can be located in a grammar of the language, for which
there are grounds of adequacy, if not of uniqueness. The semantic function
can be described if a word or phrase in the language, or in another lan-
guage, can be given which can be held, with evidence, to serve the same
or a closely similar function in a sentence with a particular "normal
purpose. With a purposive concept of sentence-meaning, the pragmatic function of a word is closely related to the semantic. Indeed, if a word has no close equivalent in its language, or in the translating language, one may have to move directly to the pragmatics of its use and specify the situations in which the word is used and what are the implications and consequences of its use. These functions must all be "identifiable. . . in normal utterances" and Cohen compares the approach with functional description in biology (although he also perceives differences, chiefly that particular organs, but not particular words, are indispensable for the performance of their functions.). Cohen's exposition is again couched in terms of wholly de facto temporal semantics, and again one must protest that functions of words are not changed by all actual uses of words, but that a normative element operates to identify the functions of words and to trace their changes.

The points at issue between Cohen and Quine are critical ones. For a functional description of language to be a viable alternative to a causal, or associational, one, the different kinds of functions must be identifiable, and yet functions are not observable in the way that the concomitance of a presented physical object and a prompted (elicited) utterance is. This may be expressed in different ways. One must observe the use of words in situations with a considerably longer "stimulus modulus" than Quine would accept as experimentally satisfactory. One is committed to holding that sentences are not atomic, isolated, but that they have implications, such that when a speaker says 'p' he is saying something that can be expressed in indirect discourse; he is doing something like asking a question, conveying information, paying a compliment, delivering an insult, etc.; he is uttering a sentence in the subjunctive
mood or in the past tense or referring to plural objects, etc.

Words relate to other words in sentences and in the language; they relate to nonverbal events of all kinds. These relationships and implications go far beyond the extensional bounds that Quine would set. We know that there is a failure of extensionality in verbal contexts of indirect discourse, believing, knowing, doubting, hoping, etc. because of "referential opacity". And yet here Cohen is proposing that when one says something, one is also (simultaneously albeit silently) saying, conveying, and implying much else besides. Quine would perhaps accept those implications which come from the truth-functional connectives in sentences. If x says, 'Cicero denounced Catiline,' he implies that it is false that Cicero did not denounce Catiline. However, he will surely not accept those implications from the use of one word to the use of one equivalent in function in the same or a different language because the only kinds of equivalence of words that he recognizes — and rejects — are too-weak extensional and inscrutable intensional equivalence. Thus the condemnation of indirect discourse as "perhaps irreducibly non-extensional" can only be softened by the reflection that it is "in any event at variance with the characteristic objectivity of science. It is a subjective idiom."

A regimented sentence serves the same function as its replaced sentence if, in a deduction, the same inferences can be drawn, etc. (See text, pp. 66-67, 74.) There is little doubt that "serving the same function" can be applied with far clearer standards here than among language-words and -sentences. There is also little doubt that, on Quine's concept of meaning and standards of evidence, determining the functions served by language-words is a task which cannot be performed empirically. A functional approach always rests heavily on a working assumption of the universality of some
functions in the universe under discussion (in this case, speech communities). For the description of languages, the number of functions is liable to be extremely large and the selection of functions vulnerable to the charge of "ad hocness".

Cohen's defense of the empirical soundness of the linguist's hypotheses based on a functional concept of meaning is that they can be tested in use, by being "acted on", and that they are therefore subject to confirmation and refutation as "analytical hypotheses" are not. This approach uses precisely the test which Quine proposed — but really had no grounds for proposing — for the fluency of the bilingual: observing his success in communication. The linguist can himself "act on" his hypotheses; two linguists at an impasse or two rival bilinguals can do the same.

K.L. Pike, whose creation of an imaginary language, Kalaba, as a test case for students of phonemic analysis inspired Quine's imaginary case studies in "Meaning in Linguistics" and Word and Object, makes this point strongly in the section of his monograph on identifying "words" among the phonetic data. He writes that "our establishment of phonemic principles and procedures must ultimately rest upon our observations of native reactions to the phonetic data." The native speaker's "unconscious physical, linguistic or social reactions to the structural unity of his phonemic system may be analyzed by the observer." "A procedure which ignored [these reactions]... would be just an arbitrary type of 'algebra' which does not analyze the facts of the language as a structural system functioning as a medium of communication."

According to the functional concept of meaning, a word functions both in the language and in the world. The putative syntactic, semantic
and pragmatic functions of any particular word have implications in the language and in the world. This means that a functional definition of any one word affects many other words: "families" or "linguistic fields" of words which may substitute for each other in some contexts or associate with each other in sentences. A functional definition of any one word affects many nonverbal contexts in which that word may be used. There is more relatedness in a language so conceived than Quine wants to grant.

Not only does Quine take as the linguist's only proper method the matching of things presently happening (narrowly interpreted) with utterances, but he takes it to be a distinguishing feature of language that meanings are assigned to sentences one by one rather than systematically. In a certain sense, the sentences of a language are clearly not comparable to the sentences (assertions) of a scientific theory. This is the sense in which the sentences of a language are the data for the theory of that language, and not the theory itself. We may equally "wonder at" the significance of some phenomena which are held to be the results of an experiment, if no one has told us the hypotheses being tested in the experiment. However, the sentences which constitute the theory of language are precisely like the assertions of any other scientific theory, and the language-sentences generated by the theory (including translations, interpretations, paraphrases) are also interrelated to the extent that there are families of words, limited grammatical categories, semantic and pragmatic connections, and so on. No decision in the theory can affect only one sentence in the language. A sharp difference of opinion between linguists on one sentence is not a dead end, and is not without empirical possibilities for settling the dispute, or at least for carrying it much farther along than the doctrine of indeterminacy allows.
Cohen's approach undoubtedly owes much to Wittgenstein, as he acknowledges. He has, however, been selective in drawing from the insights of the later works. He is as critical of "rules of use" as an illegitimate blend of factual and normative approaches as he is approving of Wittgenstein's emphasis on "use" and on words as "institutions".

It is a decided risk of the aphoristic style of writing philosophy which Wittgenstein practiced that bits of one's work can be appropriated by followers of widely different persuasions. Wittgenstein eschewed the systematic development of his ideas and was deliberately un- and even anti-theoretical. It is therefore not always easy to determine when his words are being used in the spirit of his philosophy, and when not.

For an example which is pertinent to our present concern, Quine quotes Wittgenstein's "Understanding a sentence means understanding a language" in support of indeterminacy of translation. It is possible that he is not really entitled to do so. In context, Wittgenstein is emphasizing the interrelatedness of sentences in a language, and the impossibility of understanding any sentence of English without understanding English and its whole system of "customs" with respect to such features as pronouns, pluralization, tense, sensation words, etc. Some stress must be placed on "understanding"; one can surely know how to use a single French sentence in a specific limited context without knowing French, but one cannot know how to use a sentence fully and in all possible contexts without knowing the language.

Quine is extracting from Wittgenstein the expression of a sentence's intimate connection to its language, and he concludes from this that there is no "linguistically neutral meaning." However, what is to prevent Wittgenstein from stating as a corollary, "To understand that a sentence in
English and a sentence in German are equivalent means to understand both English and German," particularly since in the *Investigations* the sentence we are considering is the purported translation of its German original, "Einen Satz verstehen, heisst, eine Sprache verstehen," which appears on the facing page?

Another functional approach to language description, which utilizes the concept of a "linguistic role", is taken by Wilfrid Sellars and Bruce Aune. Their point of departure is a critique of an associational concept of meaning. Sellars writes that many theories of meaning which are nominalistic with respect to "'thinking in absence'" turn out to be quite 'Augustinian' in accounts of 'thinking in presence': of those occasions on which the fundamental connection of language with non-linguistic fact is exhibited." However, this is a mistake, according to Sellars. There are no "selfauthenticating nonverbal episodes"; learning to apply a word to a thing presupposes a great deal besides the presence of the thing and the utterance of the word, such as background knowledge about observers, conditions of observation, grounds for inference, etc.

"De facto correlations of different sound patterns with one another, with physical movements, and with certain features of reality that we might regard as occasioning them" cannot determine meanings. To "decode" sound patterns, the observer needs to know the "criteria internal to the conceptual scheme" of the language.

There is nothing here that Quine would not assent to when he is discussing radical translation, except that perhaps Sellars and Aune take a dimmer view than he does of even the "stimulus meaning" correlations. The Sellars-Aune view reinforces Quine's argument against the translation of terms by the same means by which he has translated observation sentences.
The translation of the sentence 'Gavagai' is probably acceptable, but one cannot go on to translate 'gavagai' without "criteria internal to the conceptual scheme". This is just what Quine means when he writes, "Occasion sentences and stimulus meanings are general coin; terms and reference are local to our conceptual scheme."

However, Quine wants to insist in Chapter I that conditioning (association plus reinforcements) is responsible for all language learning, and the challenge of this view is that Quine must explain how such conditioning can account for more of language than can be translated by stimulus meanings. How is the conceptual scheme of the language (what counts as an object, as standard conditions of observation, etc.) learned by association and reinforcement? Association alone will clearly not do it, as Quine recognizes in Chapter II. What must implicitly be packed into "reinforcement" in order for it to make up for the inadequacy of association? Is it entirely nonlinguistic? If it is, it should be available to the linguist in his efforts to learn the language within the community. If reinforcement is not entirely nonlinguistic, then Quine is paying lip service to association, while not really holding association responsible for learning the enriched language.

An associational theory of meaning, which postulates separate conditioning bonds for each word-sentence/thing or word/word or sentence/sentence relationship, fits well with an atomistic view of language. Functional theories of meaning, on the other hand, tend to emphasize interrelatedness: of linguistic elements with each other, with action, and with knowledge, and to stress the importance of norms. As Sellars writes,
The language of observation is learned as a whole; we do not have any of it until, crudely and schematically, perhaps, we have it all. We acquire the ability to use colour words along with the ability to speak of physical objects located in space and time. ... etc. etc.

Sellars approves of the pragmatist's definition of the meaning of a term as "its role as an instrument in the organism's transactions with its environment" only if this connection of language with conduct is interpreted as "intrinsic to its structure as language, rather than a 'use' to which it 'happens' to be put. ..."

Both interrelatedness and normativeness are brought out by the notion of "linguistic role". The language, its use, and its related conceptual scheme are conceived as a single system in which different elements play parts. Different languages are seen as different systems of the same type. Their speakers, that is, are the same sorts of entities, observably engaging in the same sorts of activities, both sound-emitting and not sound-emitting. This is the framework in which translation is viewed as quite unproblematic. With respect to the normativeness of linguistic descriptions, characterizing the role of an utterance is seen as "subsum[ing] it under a network of essentially normative concepts," in the way that characterizing movements as actions or shapes of wood as chess pieces involves norms.

Sellars objects to taking the term 'means' in interlinguistic semantic statements as a relational term, and defines it instead as

a linguistic device for conveying the information that a mentioned word ... plays the same role in a certain linguistic economy as does [another word] which is not mentioned but used ... and which occurs 'on the right-hand side' of the semantical statement.

The two central points here are that

(1) a linguistic element plays a role in a "linguistic economy";
The metaphor of an "economy" is a very good one for the kind of system we are considering, an open system, one in which exchanges are continually taking place, one in which there are norms, but fluctuating ones, equivalent and nonequivalent values, and one, finally, which is involved in both domestic and foreign trade.

(2) to play the same role, which is to mean the same, is perfectly compatible with being different in other respects, e.g., physically, as shapes.

Aune writes that one role or function "could in principle" be performed by linguistic expressions "with very different empirical features", giving the example of 'p⊃q' and 'Cpq'.

Both Sellars and Aune use the example of chess and "Texas chess" (played with different makes of cars on counties) to demonstrate sameness of role and difference in other grossly observable characteristics. Players can learn to perceive the relevant sameness, although it is not at all self-evident. If one's experience in a particular area includes only one kind of physical thing performing a particular role, it may take a while before the sameness of role is recognized in abstraction from the difference of physical appearance. Sellars and Aune make a good deal of the fact that a complete language must include linguistic means for expressing formal analogies between linguistic roles exemplified by empirically different forms. Sellars relates this to "theoretical discourse" and its use of models.

The essential thing about a model is that it is accompanied, so to speak, by a commentary which qualifies or limits — but not precisely nor in all respects — the analogy between the familiar objects and the entities which are being introduced by the theory.
While it is true that, without means in the language for expressing such analogies, one could not account so easily for translation and paraphrase, such a language would be impoverished in many other areas as well. After all, if the perception and use of analogies are fundamental intellectual tools (see text, p. 3) and if analogy involves more than physical resemblance, which it surely does, then any language in which one could reason at all would be one with such means. The point is only that this requirement of a language is not a requirement for semantics' sake alone, nor, perhaps, does one have to argue for it so hard.

However, when one tries to see the relation of this concept of theoretical discourse to behaviorism, its relevance and importance appear to increase. The discussion occurs in the context of this problem: what additions must be made to a public language "of which the fundamental descriptive vocabulary speaks of public properties of public objects located in Space and enduring through Time" in order to make possible talk about thoughts, sensations, feelings, etc.? Can one, in other words, remain in some sense a behaviorist while describing a language functionally in such a way as to include intentions, expectations, consequences, etc., of utterances, or must such descriptions rest from the start on the positing of thoughts and other inner episodes which are sui generis? Sellars' position is that no additions are required which diminish the intersubjectivity of language, and that this intersubjectivity is the crucial requirement for the empiricist in his commitment to restrict his evidence to overt behavior.

The first addition to the public language is semantical discourse, language for talking about linguistic expressions, presumably down to
word-level, about their meaning, truth, reference, etc. Quine would not deny that, within a language, semantical discourse is possible. For Sellars, this addition is related to the eventual possibility of being able to talk about thoughts because "semantical talk about the meaning or reference of verbal expressions has the same structure as mentalistic discourse concerning what thoughts are about." They are both "intentional" in Brentano's sense of having an object.

The second addition is, for Sellars, theoretical discourse, as mentioned above, with its use of models and hypothetico-deductive reasoning; for Aune, it is language for expressing formal analogies between linguistic roles. Given the structural, or formal, similarities between the "aboutness" (or intentionality) of semantical discourse and discourse about thoughts and other inner states, the resources of theoretical discourse make it possible to apply semantical categories to postulated "inner episodes". Their arguments use an imaginary case of a primitive, "extensional" speech-community in which people do all their reasoning and problem-solving out loud. They could have used actual psychological case studies of the development of "inner speech" in children after a stage of "egocentric speech" which is "thinking out loud". In these situations, if one were to observe intelligent behavior (i.e., the sort normally preceded by overt reasoning) not preceded by such overt verbal behavior, one might reasonably formulate a theory of thinking, using a model of speaking.

Sellars holds that the addition of theoretical discourse to a language in no way makes that language less intersubjective. He holds that the behaviouristic requirement that all concepts be introduced in terms of a basic vocabulary pertaining to overt behaviour is compatible with the idea that some behaviouristic concepts are to be introduced as theoretical concepts.
The theoretical concepts, that is, may be themselves of unobservables, without being unempirical or unacceptable to behaviorism, and he gives the physical sciences as illustrations. Because overt behavior is evidence in both cases, "concepts pertaining to . . . inner episodes . . . are as intersubjective as the concept of a positron."

Quine would object to the interlinguistic use to which theoretical discourse can, on this view, be put. Although a model may be found by a speaker of one language to translate another language on the basis of some of the similarities of function perceived in observing the behavior of native speakers, there is no way to justify that model over any other, even very different, one. For any given sentence, one cannot ask if a translation of it is "correct" apart from the particular model, or "general scheme of translation", according to which it was translated.

If the two sentence translations differ in truth-value, as they may, one appreciates the quandary of radical indeterminacy. Curiously, a defender of Quine, Gilbert Harman, chooses as an example a sentence "translated" from number theory into different formulations of set theory.

One might answer that, using a kind of sentence with more links to conduct than a sentence in mathematics, one translation might well be found to be, in testing it by using it, if not "correct" in the sense of uniquely so, then at least significantly preferable to the others. Whether one accepts that there are empirical grounds for choosing among, or ranking, different translations, or rejects this, as Quine does, depends a great deal on whether one stresses interrelatedness within language and between language and nonverbal behavior or an atomistic view of language in which all relations are seen as having been separately conditioned. Sellars' view, which allows for functional descriptions of language, both behaviorally respectable and inclusive of mental attitudes, and of functional
translation between languages, clearly stresses interrelatedness.

No one can doubt that theory occupies a central place in Quine's view of knowledge. Even the common sense knowledge that we have of ordinary physical things is "woefully underdetermined" by "surface irritations, which exhaust our clues to an external world." Theory fills in, and there is no difference in kind between common sense "theories" and esoteric scientific ones. The scientific theory is more "vivid", by which Quine means more self-conscious and reasoned, "yet in point of function and survival value ["the hypothesis of ordinary physical objects"] and the hypothesis of molecules are alike." "So much the better," he adds, "of course, for the molecules." He is not against unobservables and he is not against theoretical discourse. As a philosopher whose touchstone is modern physics, he could take no other positions on these matters, but as a behaviorist, it is worth remarking on.

It is even possible to tease out of Quine's parallel accounts of the learning of language and the development of science (Chapter I) some features close to Sellars' account. After the early stage of learning words as whole one-word sentences, the child "tends increasingly to build his new sentences from parts," but this still involves learning words in context by learning "the usage of sentences in which the word can occur." Note that "can occur" can not be identified with "does occur", but includes that and "may occur" as well. Learning this is in fact not different from learning the "function" or "role" of a word. Words with unobservable referents are partly learned by describing their referents by "the special form of analogy known as extrapolation." This is of only limited use, however, because there is no clear way of distinguishing what the theoretical objects are from what the theory says, or posits,
Theoretical discourse is therefore indispensable.

Here Quine's and Sellars' (or any non-associationist's) paths diverge. Learning a scientific theory, for Quine, is being conditioned to "associations of words with words" (or sentences with sentences, or words with sentences). The sentences of a theory may be associated with each other by "so-called logical connections" and "so-called causal ones" "but any such interconnections of sentences must finally be due to the conditioning of sentences as responses to sentences as stimuli... by the mechanism of conditioned response." Quine remarks that he has represented the evaluation of evidence for or against a theory "to be a strangely passive affair... we just try to be as sensitively responsive as possible to the ensuing interplay of chain stimulations." He suggests that perhaps even the "vaunted" standard of simplicity is "just a feeling of conviction attaching to the blind resultant of the interplay of chain stimulations in their various strengths."

Quine at times represents his associationism so baldly as to appear perverse. He does not address himself to the problems of learning a theory by being conditioned to respond to specific sentences with other specific sentences, in what would have to be a rote fashion. Is there a unique set of sentences for each branch of theory, and is being conditioned to respond with sentence no. 19 to a stimulus of sentence no. 18 what is meant by "understanding" a theory? Furthermore, on this view, the young student would understand the concepts of the theory just as well as his teacher once he was conditioned. This seems contrary to our experience of school children who can parrot assertions from a branch of science, history, mathematics, or whatever, without understanding fully what they
have "learned", and also the novice's apparent growth in understanding of concepts in his field, as evidenced by numerous operational criteria.

Quine's lack of attention to these questions can only be explained by the fact that he accepts stimulus-response learning theory largely on faith. It is one of the assumptions that he is holding fixed in this investigation. This was borne out by Quine's answer to a question after a lecture at Amherst College in March, 1965 when he blandly remarked that he didn't think his acceptance of Skinner's behaviorism was at all controversial. (His colleague at Harvard, B.F. Skinner, appears to have a similar attitude to criticism.)

The above pages have attempted to demonstrate that the particular brand of Quine's behaviorism, one which employs an associationist theory of meaning and takes an atomistic view of language, is directly related to the conclusion that "stimulus meaning" is the only empirically sound fragment of 'meaning', and that this restriction of the concept of meaning cannot help but result in the indeterminacy of translation of all but observation sentences. Apart from the inconvenience of this conclusion, which perhaps could remain merely theoretical while translation would still be undertaken in practice, we have suggested that Quine's position undermines the existence of a language which is both enriched beyond stimulus meanings and common to a group of speakers. Since the theory does not permit us to account for anyone learning such a language nor to describe it being spoken, we have neither the means nor the justification for asserting that there is such a language. From the outside of a wall which we can neither peer nor climb over, we can have nothing to say about what, if anything, is on the other side. However, this unperceived
and unperceivable enriched common language cannot "drop out of consideration" like the beetle in the box because Quine's view of meaning depends on there being something there which we cannot get at, and his view of language, particularly when he turns from interlinguistic translation to ambiguity in English, is of a language with considerable resources and common to a speech community.

Furthermore, Quine's death blow to translation with his assertion that meaning in language and truth in physics have nothing to do with one another -- his view of the linguist's enterprise as entirely different from that of any other empirical scientist -- is a consequence of his seeing the sentences of a language as independent of one another. An atomistic view of language holds that an interpretation or translation of a single sentence has no consequences apart from setting a meaning for that sentence. It follows naturally enough from a theory that language learning is an accretive process of establishing separate conditioned bonds.

Although science represents the highest values for Quine, and he recognizes the complex relationships between theory and experience in any science, his insistence on learning by conditioning makes learning a theory something rather mystifying. To insist upon this learning process, and to speak of "so-called" logical and causal connections between sentences of a theory, makes it seem entirely fortuitous why the assertions of a theory comprise the theory as they do. A scientific theory can come to seem as atomistic as a language, if the connections are all due to individual conditioned bonds.

On the other hand, if we delete associationism from Quine's view of
the complementarity of theory and experience, and recall his emphasis on the indivisibility of knowledge, then there appears no particular reason to exclude linguistics from the empirical sciences. If one views translation on the model of mapping one set of symbols on to another, then, formally, there can be a "difference of net output" without possible criteria for choosing between different mappings. If language is seen in the context of its use as a means of communication, then the linguist's problems seem no worse than those of any other scientist.

B. Analytic Truth and Meaning

A classical defense of an absolute analytic-synthetic distinction would assert that (1) every proposition is either analytic (if true; self-contradictory if false) or synthetic; (2) an analytic proposition is true solely by virtue of the meanings of the terms contained in it, while the truth or falsity of a synthetic proposition is determined exclusively by nonverbal facts. To give the meaning of a term may be, as for Morris Lazerowits, to give a "fact about verbal usage," and such facts justify the necessity of the containing propositions, but apart from "exhibiting" facts about verbal usage, necessary propositions are wholly uninformative. Synthetic propositions are informative, and their truth rests on extralinguistic facts. All synthetic propositions are seen as on an equal footing with respect to possible refutation by adverse facts. There is a clean, clear line between analytic and synthetic propositions, and one can always tell on which side of the line to place a given sentence by looking at it and asking, 'What could possibly make the true proposition expressed by this sentence not true?' If the answer involves something happening to referent(s) of the sentence, it expresses a synthetic truth. If the answer involves nothing but a change
in the meaning of a term or terms in the sentence, it expresses an analytic truth.

Quine has taken the position that such a line cannot be drawn, and such a neat division of true sentences into these mutually exclusive categories cannot be made. Any sentence can be held as true despite the evidence, any sentence can be overturned despite its form. His picture is one of a continuum of sentences, related to each other and to the nonverbal world in such a way that (1) no sentence is linked only to nonverbal experience or to verbal facts; (2) some sentences are more secure in the system than others, but none is irrevocably secure and none is dependent for its truth on an isolated experience. He has also argued that no satisfactory definition has been given for 'analytic', so that there is no way to show that any sentence is analytically true. As Harman remarks, Quine's conclusion is not merely that the distinction is a vague one, but that "nothing is analytically true." He compares this to the "witch-nonwitch distinction". It is not only that there is a substantial area of vagueness within which it is difficult to decide whether an entity is or is not a witch, but that the definitions of 'witch' (or 'analytic') which have been given have no extension whatever.

Giving up the absolute analytic-synthetic distinction creates questions about meaning and reference, meaning and experience, and sameness of meaning which were previously settled. The dichotomist's interest in meaning is only in what might be called essential meaning. Two terms are synonymous if they have the same essential meaning. He considers it no great problem to get at the essential meaning of a word. It takes a dictionary plus native speakers' intuition which can be expressed by the
formula, "'___' and not . . . is inconceivable." ("'___' is . . ." is analytic.) If he is troubled by the criterion of inconceivability, which does seem irredeemably pictorial, vague and subjective, but is hard to do without entirely, he may withdraw to facts — or better, rules — of usage. That a rule strong enough to get at essential meanings is a rule that presupposes necessity, and hence offers a circular criterion, does not bother him. The rule itself is simply a convention, a matter of contingent fact, and can be changed at any time. That there are rules with this force, and that they can be distinguished from other conventions which do not support analytic truth, he simply assumes.

Quine, however, cannot find answers to questions about meaning and sameness of meaning that satisfy him. He therefore proposes doing without these concepts. We have seen the effects of this decision in both translation and regimentation. The enterprise of translation, without an "intuitively" complete concept of meaning, has had to make do with "stimulus meaning", with the resulting indeterminacy of its products. The enterprise of regimentation, without a concept of sameness of meaning to check the regimented expression against its explicandum in natural language, has taken the standard of a narrowly defined "function in a deduction" and, where that is not adequate, the very permissive "purposes of the paraphraser".

Quine's arguments against the analytic-synthetic dichotomy, particularly in "Two Dogmas of Empiricism," have evoked considerable critical response. It will not be possible to deal with all of these critical proposals in detail, but we will consider some of them, grouped with respect to their general approach. There are five groups of criticisms, which, from the most critical to the least critical, are (1) the Ordinary Language
criticisms, (2) the Artificial Language criticisms, (3) the Empirical Survey criticisms, (4) the Gradualist criticisms, and (5) the Normative criticisms. The last of these does not lend itself to the sort of point-by-point comparison with Quine as do the other four. It is in some ways close to the Ordinary Language criticisms, and in some ways independent of the discussion in the terms in which Quine has formulated it. It is placed last, not because it is least critical, but because it stands by itself. First we will summarize the criticisms, and then go on to evaluate them.

Ordinary Language criticisms. The most important paper in this category is H.P. Grice and P.F. Strawson's "In Defense of a Dogma." Almost all of Quine's critics acknowledge some justice in his argument, and Grice and Strawson are no exception. Their adaptations of their position include an acknowledgment that not only matters of fact cause revisions in the truths of a theory, but also "at least partly" changes in the meanings of words, and a nod to the interrelations of statements in their formula that two statements are synonymous if the set of confirming or disconfirming experiences "on certain assumptions about the truth-values of other statements" confirm or disconfirm both statements "to the same degree." However, they plead that "the existence...of statements about which it is pointless to press the question whether they are analytic or synthetic, does not entail the nonexistence of statements which are clearly classifiable..."

Grice and Strawson's chief positive point is that the use of a distinction, by which they appear to mean the use of terms in which the distinction has conventionally been made, entails that there is such a distinction. Thus, in philosophical tradition and present practice, 'ana-
lytic' and 'synthetic' are used, and with considerable agreement. In ordinary usage, 'means the same' and related expressions are used, and not as equivalent to 'is true of the same objects'. Grice and Strawson accuse Quine of being impractical and unreasonable. They do not know how impractical he is willing to be. They ask

Is all talk of correct or incorrect translation of sentences of one language into sentences of another meaningless? It is hard to believe that it is.

This may even have suggested to Quine the consequences that had to be drawn from a position that he was not willing to give up for practical reasons. Grice and Strawson also point out that if there is no sameness of meaning between sentences, then there can be no meaning for a sentence. Here, too, Quine has been willing to accept unpalatable conclusions.

The chief critical point of the Grice-Strawson paper is that Quine has set impossibly high standards for the "adequate clarification" of 'analytic', and, because this clarification has not, and probably cannot, be given, has "denied its reality". Quine's outline of the problem and his requirements for clarification are summarized as follows:

1. There is a family of expressions;
2. If one member of the family could be satisfactorily explained, so could all the others;
3. Each member "is in as great need of explanation as any other";
4. An explanation cannot use any expression from the family;
5. An explanation must have the form of a formal definition, and "specify some feature common and peculiar to all cases".

The authors remark of (4) and (5) that it is "doubtful whether any such explanation can ever be given" of any expression. They suggest less
formal kinds of explanation which satisfy the noncircularity require-
ment of (1) but not the definitional requirement of (5). From the
example given of explaining 'logical impossibility', it seems to be a
kind of verificationism.

Artificial Language criticisms. Since we have dealt with Quine's
criticisms of Carnap and Martin's defense of Carnap earlier, (see text,
pp. 11-15.) we will consider this line of criticism rather briefly, and
through another spokesman, Benson Mates, in "Analytic Sentences."

It is interesting, in taking these criticisms seriatim, to see the links
connecting very different philosophical approaches. Mates, for example,
is in agreement with Grice and Strawson that it is significant that both
'analytic' and 'synonymous' are used in natural language, and that
Quine's standards for an adequate definition of 'analytic' are too high.

Taking the latter point first, Mates' outline of Quine's (and Morton
White's) standards is somewhat different, oriented, of course, more
toward Quine's criticisms of the artificial language approach to the
problem:

(1) A definition should be close to "standard or preferred usage";
(2) The syntax of the definition should conform to "ordinary" syn-
tax;
(3) A definition should not contain the word(s) being defined, nor
words not from natural language;
(4) A definition must be better understood than the definiendum.

Mates says that the addition of (4) may very well make the set of require-
ments too high to be met.

One could test for the existence of notions of 'analytic' and 'sy-
nonymous' by sets of questionnaires, follow-up questionnaires, and revi-
sions made by the subjects. For the former "notion", one would test attitudes toward sentences with respect to the relevance of evidence for truth. Mates holds that the investigator would find "a rather remarkable agreement". For the latter, one would test the interchangeability of expressions. These tests do not "define" the notions, but are "practical criteria" for them.

From this evidence of "'intuitive' notions of analyticity and synonymy," Mates goes on to urge the usefulness of artificial languages for "refining" our understanding of semantical terms. An artificial language makes the rules behind the application of the "intuitive" notions explicit, and "otherwise differs as little from the natural language as is compatible with reasonable simplicity." When an artificial language uses a term "'analytic-for-L_o'" "an analogy is being drawn" with 'analytic' in a natural language. The closer the artificial language is to the natural language, "the more adequately will 'analytic-for-L_o' explicate 'analytic'."

Empirical Survey criticisms. There is, perhaps surprisingly, considerable mutual support between some advocates of artificial languages and of empirical surveys. Arne Naess, the chief representative of "empirical semantics" whom we shall consider, takes Carnap as his philosophical guide, and Carnap considers Naess's work to "provide abundant evidence in support of the intensionalist thesis."

Briefly, the background for this is as follows: Carnap has defended the idea that semantical concepts are most fruitfully examined within constructed languages, and has proposed the "method of extension and intension". Quine, in "Two Dogmas," challenged the relevance of such studies to the natural-language difficulties with these concepts. There-
fore, in "Meaning and Synonymy in Natural Languages," Carnap proposes "to clarify the nature of the pragmatical concept of intension in natural languages, and to outline a behavioristic, operational procedure for it." He expects that this "will give a practical vindication for the semantical intension concepts." He wants to show "that the assignment of an intension is an empirical hypothesis which, like any other hypothesis in linguistics, can be tested by observations of language behavior."

Mates reflects Carnap in casting the problem in terms of "two basic approaches," the extensional and the intensional, of which the former is often (mistakenly) considered "more scientific." He points out some difficulties with the extensional approach, difficulties which Quine makes much of in "Meaning in Linguistics" and Word and Object, but which Carnap does not acknowledge in "Meaning and Synonymy in Natural Languages" when he assumes that linguists can reach "complete agreement in the determination of the extension of a given predicate in a given region."

Mates defends the use of both approaches together in determining the "ordinary use" of a word: the extensional approach in observing cases of use of the word, and the intensional approach in administering both Socratic and "anti-Socratic" (leading and open) questionnaires to get at "what [the subject] means by the given word."

Naess writes rather piquantly that in spite of the in many ways appalling crudity of the questionnaire techniques and in spite of the manifest inability of many subjects to enter into difficult linguistic or other fields, the data gathered are often apt to reveal or suggest as much to the researcher as do penetrating meditations or introspections based on data found in one's own head or gathered in an informal way.
In a paper on the "Typology of Questionnaires Adopted to the Study of Expressions with Closely Related Meanings," Naess suggests developing different questionnaires for different philosophical criteria or definitions of synonymy. He notes that the "indirect character of most (good) questionnaires and the non-operational character of the criteria or definitions" make the relation between the two subtle and "intricate".

He proposes something like the following:

<table>
<thead>
<tr>
<th>Associated philosophical concept</th>
<th>Questionnaire</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Interchangeability salva significations (intensional synonymy)</td>
<td>the subject is asked whether two given expressions mean the same to him in a particular context.</td>
</tr>
<tr>
<td>2. Inconceivability of logical inequivalence</td>
<td>the subject is asked to imagine or to judge what, if any, differences in conditions would affect the truth-value of sentences which differed only by the substitution of one expression for another. &quot;Inconceivability of a difference in acceptance&quot; is synonymy.</td>
</tr>
<tr>
<td>3. Logical equivalence</td>
<td>Is x a necessary and sufficient criterion (condition) for the truth of y? The expressions being tested may be used or mentioned.</td>
</tr>
<tr>
<td>4. Equivalent for the &quot;logic of argumentation (pro et contra dicere)&quot;</td>
<td>the subject is asked to imagine or to judge arguments which differed only by the substitution of one expression for another. Synonymy is the same-ness of pro- and contra- arguments.</td>
</tr>
<tr>
<td>5. Extensional synonymy</td>
<td>the subject is asked, 'Is this an example of '' (or ____)?' or 'What is an example of ''? or 'What is this an example of?&quot;</td>
</tr>
</tbody>
</table>
As can be seen, the questionnaire can be more or less direct, the danger of the former being that the subject is led, and of the latter that the relation of the results to the "philosophical criteria" may become very tenuous. Naess proposes the use of "metaquestionnaires" to test how the subject interprets "crucial" questions of the questionnaire. He bows in Carnap's direction with the comment that "in terms of operations, concepts of intension are not necessarily more vague or speculative than those of extension."

Perkins and Singer's paper, "Analyticity," is consonant with this approach, proposing experimental tests for determining the synonymy of two words in context (linguistic and nonlinguistic) for an individual at a time. The only criterion they consider is sameness of "testing procedures" for the subject. However, they offer proposals for refining and extending the scope and reliability of the results, and place this enterprise in the context of experimental psychology, its assumptions and procedures. In the same negative way in which Naess defends intensions, they write that the difficulties in testing for synonymy of words are "not any greater than those that confront psychologists who work on theories such as those about belief and drive."

Perkins and Singer are quite confident about the possibilities of generalization and extension, through further experiments and the use of samples, from synonymy of two words for an individual at a time to synonymy of those words for an individual to synonymy of words for an individual (presumably an over-all mapping of his vocabulary) to, ultimately, "a criterion of analyticity for a society". Naess probably also has hopes of such ultimate extrapolations, but because he is actually engaged
in this sort of research, he sees the complexities more clearly. For example, he views intrapersonal questionnaires as much less problematic than interpersonal ones, because the "same" results on separate questionnaires may be taken to indicate a "high degree of constancy as regards. . . . [the meaning assigned to ' ] within the system of speech habits of different persons", not that the meaning ("interpretation") of 'x' for one person is the same as the meaning of 'y' for another person, even where both respond that 'x' = 'y'. He prefers to test for interpersonal results in interpersonal situations, such as when two physicists question each other on their interpretations of a statement about 'mass', reformulating the sentence in such a way as to eliminate some interpretations and select others. The result is "two maps of synonymity and heteronymity relations, one map showing relations with the usage of p, the other. . . with the usage of q." The correspondence between these maps is measurable, and identically structured maps indicate interpersonal synonymy with respect to 'mass' in that context. It can be seen from this how great is the distance between starting such investigations and coming up with "a criterion of analyticity for a society".

**Gradualist criticisms.** This line of criticism is limited to showing that, although Quine is right in denying an exhaustive analytic-synthetic distinction, he is mistaken in denying that there are no analytic truths at all. These critics do not believe that they are dealing Quine a decisive blow, but merely making a small necessary correction in his position. The most important of these critics is Hilary Putnam, although Jerrold Katz, with an entirely different theoretical apparatus, also falls in this
category.

Putnam admits that the analytic-synthetic distinction is "of overwhelming unimportance" compared to an appreciation of the "monolithic character of our conceptual system" and of the many different (rather than merely two) kinds of statements. However, he writes,

> there is as gross a distinction between [a trivial analytic statement and a simple empirical statement] as between any two things in the world. . . . and no matter how long I might fail in trying to clarify the distinction, I should not be persuaded that it does not exist.

The task of a defender of the distinction, according to Putnam, goes beyond pointing to clear examples of it; he must try to clarify its nature and "rationale". This is what Putnam attempts to do.

"Analytic statements properly so called" are distinguished first by showing what they are not. They are not "framework principles", statements of high systematic import, such as the central scientific generalizations. Framework principles may deceptively appear to be just like traditional examples of analytic statements because they have definitional form and because they cannot be "jeopardized by any possible experimental results". However, framework principles are not revised merely by redefining their terms but by substituting other, incompatible principles which alter the permissible inferences in the system.

Analytic statements also are not statements with "cluster-concept" subject-words. The difficulty with these words is that any element of the cluster may be eliminated, changing the intension of the word but not its extension. They are clearly not suitable subjects for statements held to be secure from revision. Putnam has shown great ingenuity in
demonstrating that with "cluster-concept" (sometimes called "natural kind") words, such as 'lemon' or 'cat', the "discovery" can be made that any property at all does not belong in the cluster.

The standard Putnam offers for a sentence in a natural language to be an analytic statement is for it to satisfy, or come reasonably close to satisfying, the following criteria for "analytic definitions", or to be consequences of such statements:

1. The statement has the form: 'Something (Someone) is an A if and only if it (he, she) is a B,' where A is a single word.
2. The statement holds without exception, and provides us with a criterion for something's being the sort of thing to which the term A applies.
3. The criterion is the only one that is generally accepted and employed in connection with the term.
4. The term A is not a 'law-cluster' word.

Putnam then proceeds to offer a "rationale" for this distinction. The advantages of having some "fixed points" in a language are "brevity" and "intelligibility". These are clear and rather obvious and, furthermore, to hold just such analytic statements which Putnam's criteria admit as "fixed points" "can't hurt." It "can't hurt" because no other laws are involved in holding 'bachelor' interchangeable with 'unmarried male' apart from 'All bachelors are unmarried,' and also because the one-criterion words available for analytic statements are the names of synthetic, not natural, classes; classes constituted by that one defining aspect alone. This limited concession to the analytic-synthetic distinction thus has some conveniences and no real disadvantages.

Despite this, Putnam grants that there is far more danger in relying too heavily on the distinction than in denying it altogether.
ferent kinds of statements, and leads to confusion and error. Narrowly,
which is properly, interpreted, the distinction cannot accomplish much
with respect to philosophical problems apart from those directly connected
with the distinction itself: it "bakes no philosophic bread and washes
no philosophic windows."

Jerrold Katz sees the significance of his paper, "Analyticity and
Contradiction in Natural Language," "in its solution to the problem of
distinguishing analytic and synthetic truths raised by W. V. Quine in
his 'Two Dogmas of Empiricism.'" And yet he agrees with Quine's
criticisms of Carnap and joins Quine in doubting that a "full-width ana-
lytic-synthetic distinction" can be supported even if some "obvious cases"
of analytic statements can be identified. Katz attempts to offer cri-
teria which are formal, noncircular, and "beg no questions of empirical
justification," for attributing analyticity to some statements. Ana-
lytic statements are those which are "predicatively vacuous". The "philo-
sophically significant cases" do not turn out analytic, and therefore the
troubling question of "their epistemological status" is not eased by this
analysis. The aim of a "full-width" distinction is "to leave no middle
ground" between necessary and contingent truths. Those defending such a
distinction have rested all necessary truths on purely linguistic grounds
and all contingent truths on nonlinguistic facts. Katz, like Quine, holds
out no hope for doing this successfully.

Katz's definition of 'analytic' occurs within the context of his
semantic metatheory. The metatheory specifies that a semantic theory in-
cludes a dictionary, not real but "ideal", and projection rules comparable
to "what the fluent speaker has at his disposal... for applying the in-
formation in the dictionary." A dictionary entry includes (1) syntac-
tical information, (2) semantic "sense characterizations" in the form of "semantic markers" and "semantic distinguishers", and (3) optionally, synonyms as a "technique of cross-reference". (2) is most important. Semantic markers "are the elements in terms of which [systematic] semantic relations are expressed" and distinguishers "reflect what is idiosyncratic about the meaning of the word. For example, semantic markers which appear in the theory's dictionary entry for 'bachelor' are 'Human', 'Animal', and 'Male'; one of the distinguishers is 'who has never married'. Semantic markers and distinguishers are not to be thought of as words in the language, but as "theoretical constructs" expressed by words. Only for this reason does such a "theoretical definition" actually "provide an account of [the] meaning" of words; it pairs a word, not with other words as a lexical definition does, but with elements of its conceptual content.

A sentence (e.g., 'Bachelors are unmarried males,') is analytic on a particular "reading" (i.e., for one fixed sense of 'bachelor') with respect to one distinguisher of 'bachelor' if and only if all the semantic elements in the entry for the subject-word are identical to those in the entry for the predicate-word. This concept of 'analytic' as "meaning inclusion" is, Katz acknowledges, close to Kant's. He maintains that his version is not open to the criticisms that have been made by Quine and others of Kant's view because, first, "containment of meaning" is no longer merely metaphorical, and second, Katz has extended the analysis to apply to more than subject-predicate sentences.

Katz holds that his definition of analyticity is noncircular because it does not use other members of the 'analytic' family in defining 'analytic'. It is "formal" because the standard of "meaning inclusion", defined
in the semantic metatheory, can be applied within a particular semantic theory "formally", that is, by matching identical constructs.

Quine has also required that a definition of analyticity not be merely stipulative. Such a definition is "empty" with respect to our use of 'analytic' in natural language. It was for this reason that defining 'analytic' in a constructed language was rejected. Can defining 'analytic' within a semantic metatheory fare better? Katz thinks so because a particular semantic theory for a particular language, constructed according to the metatheory, can be tested for empirical adequacy. This is done by testing the "correctness" of the dictionary entries and projection rules of the theory, specifically by "comparing the claims that [the theory] makes about the semantic properties of sentences in a sample drawn from [the] infinite set of sentences of the language with the linguistic intuitions of speakers." The particular "predictions" (e.g., that a given sentence is analytic) receive their sufficient "empirical justification" by being predictions of a "highly confirmed semantic theory".

This procedure suggests that one will have to test the intuitions of native speakers with respect to the technical terms of the theory. What, otherwise, are the "claims that it makes about the semantic properties of sentences"? In Katz's more recent article in the Journal of Philosophy, he recognizes this difficulty and proposes a more indirect procedure. Subjects are asked to assign sentences to lists which contain, not the words 'analytic', 'synthetic', etc., but "sentences that are clear cases of what we would regard as analytic" (or synthetic, contradictory, etc.). This indirect procedure raises other problems, the main one being the uncertainty about what the subjects take the sentences on the lists, given as examples of "clear cases", to be "clear cases" of. Katz remarks that
the experiment must be "conducted properly". The paradigm sentences must be "sufficiently different from one another in the appropriate respects, [so that] there will be no spurious common features that might lead speakers to classify sentences on the basis of irrelevant linguistic properties. . . ."

**Normative criticisms.** This group of criticisms takes us back to the proponents of a functional concept of meaning for language-descriptions. We can speak of a "normative" approach to analyticity here in the sense that where there are functions or roles, there are norms or rules which specify what constitutes performing a function. Although the proponents of a normative approach do not form a unified group, they can be viewed as holding on to the traditional distinction at least in the sense of a distinction between regularities and rules.

Quine has considered only "semantical rules" of formal languages as possible rule-supports for analyticity, and not the kind of "rules" at issue here. Wittgenstein's vacillations on the subject show the difficulties with "rules of language" which all their supporters must face to some degree. Wittgenstein is so concerned to show the ambiguity, lack of strictness and openness of rules that one is often tempted to wonder why he calls them "rules" at all. He does not wish to speak only of the "regularities" of language, however, as Ziff does, who holds that these "regularities" are not "sources of constraint". Wittgenstein, and those influenced by him, do want to hold that these "rules of language", although they are more like "regularities", "conventions", "institutions", are sources of constraint. To speak a language is not like "operating a calculus according to definite rules" and the constraints do not
involve penalties for every illegitimate move as in a game like chess. But in a broader — and vaguer — context, speaking a language is performing a variety of actions with other speakers. The actions are interrelated, and they have consequences. One cannot do just as one pleases, and remain part of the language-speaking community.

Wittgenstein, of course, speaks of meaning as "use" and often as governed by "rules of use". Cohen, with his language-sentence/utterance-sentence distinction, is much concerned to keep "rules" separate from "use". The former give the meaning of utterances only, and the latter the meaning of sentences of a language only. Combining the two, as Wittgenstein does, may seem reasonable because it "achieves a kind of semi-adequacy in both fields" but it is illegitimate. For Cohen, an utterance is a "single event", "for ever true, or for ever false", and never ambiguous. The different causes of ambiguous language-sentences — unclear referents of indicator words, different senses of contained words, ambiguous syntactic constructions, unexpressed intentions, and supplementary beliefs, etc. — cannot account for failure to determine the meaning of an utterance-sentence. Any given utterance settles all of these possible ambiguities, and thus, its meaning, "correctly understood", is also settled.

Remembering that the conceptual equivalents of language-sentences and utterance-sentences are "culture-sentences" and "saying-sentences" (and "sayings", which abstract from the "original event of utterance and from certain peculiarities of wording" of the saying-sentence), one finds that Cohen's attitude toward Quine's gradualism with respect to the analytic and synthetic is that it fits culture-sentences very well but "fully determinate assertive sayings" not at all. The former are not
sufficiently specific to indicate on their face whether they are statements about the correct use of the subject-word or about something true of the referent of the subject-word. Changes in knowledge and opinion, from place to place and person to person, all contribute to the conception of the truth of such statements as "a varying reluctance on our part to call them in question." The latter — not all "sayings" but all "fully determinate assertive sayings" — are either analytically true or not analytically true.

The critical point is change of meaning. No sentence whose meaning can change can be analytically true, and all language-sentences and culture-sentences are subject to such meaning-change. Only utterances can have fixed meanings, and those that do can, absolutely and finally, be classified as either analytic or synthetic. Putnam takes quite a different position. In arguing that even such obvious candidates for necessary truth as 'All cats are animals' are ruled out by possible future discoveries that cats have always been merely cleverly manufactured automata, Putnam specifically disallows a "rescuing move" in terms of meaning change (e.g., the sentence was necessary at time₁ and became contingent — and false — at time₂). Within the framework of Cohen's distinctions, Putnam's argument holds for language-sentences but not for utterances. Prior to the discovery that cats are really automata, the utterance, 'All cats are animals,' is necessarily true, although the identical language-sentence is not.

Cohen's account seems to turn Quine on his head. The meaning of a language-word or -sentence "is basically a fact about human behavior" and no rules can set standards of correctness for these meanings, nor halt at any moment the continuous change within a language. Quine's one
fixed point, the setting of stimulus meanings, and his greater optimism for meanings intralinguistically, are rejected. On the other hand, the meaning of an utterance-word or -sentence is best given by a translation or paraphrase, and such specific correlations are governed by rules, although not by "uniquely relevant" rules, but, for a given utterance-sentence, by "one or other of an indefinitely large disjunction of rules for translating or paraphrasing that sentence." These are rules governing the "verbal fidelity" of a translation, its literal correctness, the "legitimacy" of the move, rather than its "brilliance". A translation from "particularly exotic languages" may alter and extend the translating language in the process. These changes within the language are "neither correct nor incorrect"; correctness is a standard in appraising a particular word or expression — new or old — in one language "as a translation of an old usage in another, or even as a paraphrase of an old usage in the same language." Precisely in those cases where Quine believes that there is nothing "to be right or wrong about" Cohen believes that rules for appraising correctness are applicable.

It is not surprising that Cohen finds a place in his scheme of things for an interlinguistic vehicle of timeless meanings on the conceptual level. These are "sayings". They are held to have decisive advantages over propositions while serving all the worthwhile purposes of propositions. They are not limited to indicative assertions, not identified with "meanings", not subsistent entities. In abstracting from "peculiarities" of wording, one does not abstract from wording altogether, although some differences of language do not prevent sameness of sayings under proper conditions. Cohen proposes that Quine accept "sayings" as the meanings represented by logical formulae in regimentation. He asserts
that "there must be some determinate way of correlating the formulas
of ... a calculus with the utterances of ordinary speech for which
they may stand proxy, and clearly this is best achieved by assigning
meanings to the formulas." He brushes aside Quine's disclaimer that
the regimented expression "means the same" as its explicandum by taking
Quine's standard of "function" as an expression of a functional concept
of meaning. He denies that there are "eternal sentences", and as-
serts that to achieve the kind of sentence with fixed truth-value that
Quine seeks, one must turn from language-sentences to utterances of one
person during a very short period, abstracting from peculiarities of
language and from "authorship". The results of this process are "sayings".

Cohen's particular apparatus is distinctly his, but any functional
approach to language will be inherently interlinguistic. Sellars and
Aune can easily speak within their framework of "linguistic roles" of a
role being performed by "more than one set of sign designs." The role
or function of a linguistic element is not to be identified with that
element, but is something that the element can be used to do. To specify
the role is to separate one aspect of the element from others. It is not
a great step after that to find that different linguistic elements share
that one aspect, i.e., have the same role.

Norms, or rules, are what link particular utterances to their impli-
cations, truth-conditions, inference relations, intersubstitutable ex-
pressions under some conditions, relevant responses, and so on. It is
misleading to speak only of the use of expressions because speakers are
not free to use expressions as they please. The game/language analogy
may also be misleading in certain respects, but it is held nevertheless to
reveal some significant features of language.
Do any of the above criticisms of Quine’s radical position on analyticity go far enough to make a difference to the effects of that position on indeterminacy of translation and the procedure of regimentation? An effective criticism would have to accomplish the following:

1. Define 'analytic' noncircularly;
2. the definition must be explanatory with respect to the use of 'analytic' and related expressions in natural language;
3. the definition must be empirically justifiable;
4. the definition must be in terms of meanings, and exclude the effect of "intrusive" shared information;
5. every statement (sentence) must be classifiable as analytic or not analytic; the distinction must be absolute;
6. the definition must be interlinguistically valid.

If defense is needed of these requirements for an effective criticism, it would be that (1) is a requirement for any definition; (2) is a restriction on arbitrariness: we do not simply want a stipulative definition, but a definition which is demonstrably related to actual practice, even if it is a critical improvement on actual practice; (3) is connected to (2): because of (2) we want the definition to be reached by sound procedures. Requirements (h), (5) and (6) are intrinsic to the particular term to be defined here. A definition of 'analytic' which did not distinguish validation by linguistic facts alone from validation by nonlinguistic facts would violate the general requirement of (2) by overlooking an essential traditional feature of analyticity. A definition which did not effect an absolute distinction between the analytic and the synthetic would not serve the purpose for which the distinction has been made: to be able to classify
and locate the source of truth of all true statements in the language, particularly the central theoretical-scientific and philosophical ones. A definition which was good for one language only would not affect the question of translatability.

The ordinary language criticisms, represented by Grice and Strawson, meet the noncircularity requirement only at the expense of the requirement, implicit in (1), of a formal definition. This is in harmony with Wittgenstein's insistence that many words cannot be "defined" by giving the necessary and sufficient conditions for their application and the common features of their referents. Of the types of "informal explanation", whose availability is the argument against declaring the expressions of the analyticity family to represent "pseudo-notions," the only illustration given involves distinguishing between logical and physical impossibility by a distinction between not understanding and not believing what is alleged. In the case where the subject judges both assertions to be false, but asks himself under what conditions they could be true, he "know[s] what to prepare for" in the case of physical impossibility but not in the case of logical impossibility. This is nothing more novel than the criterion of inconceivability, and the position is, in effect, the classical dichotomist one. (See text, pp. 116-18.) This is not a counterproposal to Quine's criticisms but a reiteration of the view he is criticizing.

With respect to (2) and (3), Grice and Strawson assume that there is agreement "to a very considerable extent" in the use of 'analytic' and related technical terms in philosophical usage and in the use of 'means the same' and related ordinary language expressions in ordinary
usage. It is on this assumption that they rest their resistance to Quine's approach. "Instead of examining the actual use that we make of the notion of meaning the same, the philosopher measures it by some perhaps inappropriate standard (in this case some standard of clarity), and because it falls short of this standard...denies its reality..." However, Grice and Strawson do not "examine the actual use" of the relevant terms, and do not attempt to measure the extent of the assumed agreement in their application. Like other ordinary language philosophers, they are content to rely on their intuitions as native speakers. (Cf. Cavell, text, p. 92.)

Apart from their unquestioning acceptance of the introspection of native speakers as empirically sound for linguistic questions, the position of Grice and Strawson has another serious flaw. Even if it were well established that there is wide agreement in the use of certain terms, that fact alone would not suffice to indicate the basis of agreement. It would not tell us whether a concept of analyticity or synonymy supported the agreement, or something else, or nothing. This is what Mates calls "an armchair version of the extensional method", and it has all the weaknesses of the approach of Quine's self-restricted linguist.

The ordinary language approach does not satisfy (a) because the ordinary use of 'means the same' does not eliminate "shared information". If one is content to rely on actual, unreformed use, then one must be satisfied with a somewhat untidy result. Lacking a systematic empirical method for examining actual use in some depth, this approach cannot elucidate either the ordinary or the philosophical uses of these terms.

Grice and Strawson concede to Quine that the distinction is not
absolute, that there are statements "about which it is pointless to press the question whether they are analytic or synthetic." They also make no provision for an interlinguistic standard. They are content to deal with English.

The artificial language critics and the empirical survey critics are stronger together than either is separately. As Quine has rightly observed, an approach to analyticity through constructed languages suffers from arbitrariness, which no claim of "analogy" nor of 'analytic-for-L₀' "indicating" that 'analytic' (in English) is being explicated can evade on its own. (See text, pp. 12-13.) One can always ask for justification of the analogy, and this justification must come from outside the constructed language. It is just such support that empirical surveys can provide, by "plugging" the definitions of the artificial language in to natural language in use. The alliance is of mutual benefit because any survey needs hypotheses, precise criteria, and a theory of the relationships among its terms; these can be offered by a formal system. It may be the realization of these benefits that explains the actual mutual support among some members of these two superficially so different approaches.

With respect to (4), it is not difficult for a formal system to define 'analytic' in terms restricted to linguistic relationships. As Harman points out, however, outside of a formal system, one must distinguish postulates, or stipulative definitions, which may not be true at all from true statements which are "true by (linguistic) convention". The assignment of meaning by the rules or conventions of a language "does not guarantee truth". What is a fairly simple matter for a
constructed language, then, is a complex one for a natural language, and there is no a priori answer to whether a delineation of statements true on linguistic grounds can be made by any combination of questionnaires.

On the question of an absolute distinction, here also it can be drawn formally, but remains an open question for natural languages. One would think it likely, however, that speakers of a natural language will be "undecided" about many cases of sentences, that empirical research will yield a large intermediate area of neither clearly 'analytic' nor clearly 'synthetic' statements.

The definitions of one formal system, applied in surveys to different languages, would satisfy the requirement of interlinguistic validity. But as in any discussion of linguistic "universals", whether such a single set of definitions would be applicable to all natural languages cannot be determined in advance of attempts to apply them.

The slight sketch of Naess's work should show that the results of research in what is not philosophy but a branch of sociology will be undramatic, inconclusive, to be summarized in terms of less-than-perfect correlations. In these characteristics, they are like all scientific investigations (particularly in the social sciences) but unlike the universal generalizations of philosophy. What is involved is "a whole strategy of research" which will, in time, redefine the concept under investigation. There is no guarantee that the redefined concept will bear a close resemblance to traditional analyticity. It may be the only justifiable way to examine synonymy in a natural language, but it may turn out that Quine was right, after all, in deploring all traditional definitions of 'analytic' and 'synonymous'. As we have seen, all the important
requirements for a definition, (4), (5) and (6), remain open questions under a commitment to this approach.

Putnam, as a "gradualist", does not really consider himself in opposition to Quine. The criteria he establishes for some analytic statements make explicit defensible standards for attributing analyticity to those few, trivial, clear cases which have played such a role in philosophical discussions, if not in life. However, as the name I have given this group implies, Putnam agrees with Quine's gradualism, his denial of an absolute distinction between the analytic and the synthetic. His modification of Quine changes nothing basic in that position.

In evaluating Katz's position, we can look at a response of Quine's which is a good example of failure of communication between philosophers. Quine begins by saying that his complaint with analyticity has been that it is "insufficiently empirical". "A notion having to do with language seems peculiarly unpromising if its relation to observable behavior is obscure..." He notes with satisfaction that Katz agrees with him on the need for a "behavioral criterion". He then suggests shearing off the "apparatus" of Katz's semantic metatheory "as inessential to the central issue." He includes "by-pass[ing]" the system of "semantic markers". Quine considers Katz's central proposal to be the indirect empirical test outlined in his Journal of Philosophy article, the "lists" for classifying sentences under "clear cases" of different kinds of sentences, and criticizes it as not interlinguistic. Then he concludes by saying that Katz's lists would measure only "degrees of analyticity" rather than effect an absolute distinction, and none of the "really interesting" cases (to science and philosophy) would come out analytic.
This is amusing because Katz rests his whole case for providing a noncircular, explanatory, empirically justifiable, wholly linguistically validated definition of "analytic" (requirements (1), (2), (3) and (4)) on the fact that the definition occurs within the semantic meta-
theory. The definition is probably interlinguistically valid as well, although the "lists" mentioned in the article Quine devotes his atten-
tion to do not make this so clear. Katz grants that his definition of "analytic" applies only to "predicatively vacuous" sentences; he does not claim to be drawing an absolute distinction. However, every-
thing depends on the metatheory. If one rejects it, on whatever grounds, Katz's definition amounts to nothing; if one accepts it, Katz has done better than most other critics. But one cannot, as Quine suggests, "by-pass" the semantic markers and the "apparatus that surrounds his account of analyticity."

It seems more reasonable to say that because of insurmountable difficulties with the theory, particularly with the semantic markers, Katz's effort is unacceptable. Katz holds, on the one hand, that "meaning inclusion" is not merely metaphorical. One "path" of a dictionary entry contains the concepts in the meaning of one sense of a word. The readings for the subject-word and predicate-word of an analytic sentence are "formally identical". This makes matching semantic markers in paths of dictionary entries seem like matching truth-table columns or inscrip-
tions for identity. And yet, on the other hand, Katz insists that seman-
tic markers are not words, although we use words to "label" the "constructs"; they are abstract objects, ideas. Furthermore, for those who don't like "ideational theories", Katz grants that these ideas are not neces-
sarily consciously present in one's experience. Rather, they are theoretical unobservables, "constructs" or "posits" like unobservables in physics. With this explication, the concreteness of "meaning inclusion" pales, and one wonders about the "formal" features whose matching the definition provides for, when these turn out to be formal features of abstract entities.

This account takes the assignment of meaning to a sentence to be the result of a compositional process, and the meaning of a word to be a conjunction of properties. There is reason to think that a decision to apply a word in a given case is not the automatic result of having completed a check-list of properties. It is a simplified account of word-meaning, and in accepting it, one must accept a great deal of "apparatus" as well as the transformational grammar assumptions about innate ideas. Katz's arguments are minimal, and perhaps the prudent course is at least to defer acceptance of what, at present, appears a simple product in a theoretically burdensome wrapping.

It is difficult to bring the normative criticisms directly to bear on Quine's requirements for a definition of 'analytic'. Quine did not concern himself in "Two Dogmas" with any rule-supported approach other than the "semantical rules" of the formalists. Another difficulty is that the proponents of this approach are not really offering an alternative definition of 'analytic'. Their contribution to this discussion is an emphasis on the existence of constraints in speaking a language rather than on an attempt to locate the analytic-making ones. Cohen appears to leave wide open the selection of "a workable procedure for the identification of analytic-making synonymies and contradictions."
One can nevertheless see that the normative approach will not satisfy requirement (4). Just as a strength of this approach in language-description is that it views speech in the broader context of actions, intentions, and dispositions to respond to utterances and their implications, it is a weakness in coming to terms with analyticity that it does not, in principle, separate language from the rest of the world. By broadening 'meaning' beyond extension and intension, the functional approach can make finer distinctions than an extensional approach and can make those distinctions more objective and observable than an intensional approach. However, it is not in a good position to separate purely linguistic truths from other truths.

Despite the differences, there are some interesting points of contact between this approach and Quine's. The most obvious is Quine's use of "function" in regimentation. Another is that the paradigm case of regimentation is one in which the person who utters a sentence is the one to regiment that sentence. This is a recognition of the "fixed meaning" of an utterance. The sentence is "fully determinate" for the person who utters it; he knows what he means. Another is "primary synonymy", the "true" and "genuine" synonymy between "sufficiently long segments of discourse." Since its specification may even include "stage directions", this comes close to a functional language-description.

To see why the functional approach cannot represent a solution to the problem Quine has set for himself, let us look at these points of contact and the difficulties they suggest to Quine. The lexicographer cannot deal directly with "primary synonymy" because he cannot specify and catalogue "the infinitely numerous genuine pairs of long synonyms."
But the lexicographer must go on to specify and catalogue, and so he finds himself dealing only in "quasi synonyms". In the later writings, his task is depicted even more pessimistically. The paradigm case of regimentation only leads Quine to conclude that there are no standards by which one person can reject another's regimented expression, not that, for an utterance in context, a meaning (or paraphrase, or translation) can be given. Finally, pace Cohen, "function" in the restricted sense in which Quine uses it is very different from the function of an expression in communication. The ways in which it is different are critically important for Quine. The function of an expression in a deduction can be formally identified and specified. General rules can be given from which inferences follow mechanically. The function of an expression in communication is not generally nor precisely specifiable at all.

Alston finds himself "reluctantly forced to conclude" that there may be no way "to generally define 'linguistic act'". This does not lead him to abandon the concept, but to accept a piecemeal strategy of analyzing particular linguistic acts. To compromise with generality and exactness is certainly not Quine's way. It is hard to see how the tension in his aims between Quine-the-logician and Quine-the-empiricist can be resolved with equal justice to both Quines.

At the end of section A., I tried to suggest that Quine's associationist behaviorism not only limits him to stimulus meaning and, consequently, leads to indeterminacy of translation, but that it does not allow for the kind of language that he assumes all along nor for scientific theory. What was proposed in section A. was that perfectly sound standards of evidence support a functional concept of meaning, which gives Quine a language as enriched as he requires and theory as well,
and also evades dire consequences such as indeterminacy of translation. We were dealing with behaviorism and empiricism, and yet also with a preferred concept of meaning.

In this section, purportedly limited to questions of meaning, we have come up again against considerations of empirical support. Perhaps there is no reason to deplore this blurring of the tidy categories we originally drew. Quine also sees them as different sides of one problem. He investigates the problem of empirical support, however, in much less detail and depth than the problem of defining analyticity. I have been critical of him for his dogmatic acceptance of a crude behaviorism and his equation of that position with "empiricism." He cannot be accused of complacency or carelessness for his arguments against the analytic-synthetic distinction. They are far better than the arguments used against him. None offered, if I am correct in interpreting them, meets Quine's challenge.

Another way of saying this is that Quine-the-logician was the dominant author of Word and Object. If it were not for his manifest and explicit objective of finding empirical supports for "notion[s] having to do with language", the bias in favor of extensional logic would be merely a fact for the reviewer to record. As it is, the inadequacies of his empiricism do grave injustice to his own aims, and there is some reason to think that revisions there would allow a concept of meaning which also could not stand up to his stringent requirements. Perhaps the stringency of the requirements needs to be called into question. The alternatives may have been wrongly perceived as either meeting those requirements or doing without meaning. The utility of a concept of meaning may require of philosophy something both more practical and more difficult; help in making a functional concept of meaning more precise and operational.
Abbreviations used in footnotes

Word and Object: W&O
From a Logical Point of View: FPV
Introduction to Mathematical Philosophy: IMP
Principia Mathematica: PM

FOOTNOTES TO CHAPTER I

6. W&O, p. 82.
12. W&O, p. 9; see also pp. 114-5.
13. One would think, in the light of the book as a whole, that indeterminacy is already rearing its ugly head; but in this connection, it doesn't bother Quine at all.
19. See also Chomsky, "A Review of B.F. Skinner's Verbal Behavior," op. cit., p. 572. In response to criticisms by J.A. Fodor, Charles Osgood, proponent of a "mediating response" theory, argues that the difficulty in demonstrating the "functional equivalence" of responses is not an "inherent inadequacy" of his theory. "Two words would have the same meaning, or two word strings be paraphrases, if and only if they produced precisely the same simultaneous pattern of r.'s. However, this is a situation rarely found..." J.A. Fodor, "Could Meaning be an r.'?" Journal of Verbal Learning and Verbal Behavior, 4 (1965), pp. 73-81; Charles E. Osgood, "Meaning Cannot be an r.'" ibid., 5 (1966), pp. 402-7. This is reminiscent of Goodman's approach in "On Likeness of Meaning," Analysis, 10 (1949). The definition is clear and strict but has almost no instantiations. The vast majority of words and phrases turn out nonsynonymous, with no way of distinguishing among them.

20. W&O, pp. 26, 27, etc.


25. W&O, p. 34.


27. LPoV, p. 42.


32. W&O, p. 75.

33. W&O, p. 79.

34. "Two Dogmas of Empiricism," in LPoV, p. 41.

35. W&O, p. 73.
36. LPoV, p. 37.

37. Jerrold Katz, in a recent paper, rejects this approach. In the first place, he regards Quine's delineation of "logical truths" as too narrow, including only the tautologies of English rather than the "logical truths of quantification theory." He is apparently overlooking the lack of a decision procedure for theorems of quantification theory. In the second place, and more interestingly, he claims that one must "know quite a bit about the semantics of certain types of English constructions" in order to "generate[e] the logical truths of English by substituting words or expressions from English for predicate variables in the direct, simple-minded way." He holds that "such translations must be mediated by a representation of the meaning of sentences and must thus be indirect." "Some Remarks on Quine on Analyticity," The Journal of Philosophy, LXIV (1967), pp. 36, 38; illustrations on p. 37.

38. LPoV, p. 27.


40. LPoV, p. 25.

41. LPoV, p. 24.


43. Benson Mates, who is quite optimistic about interchangeability as a criterion of synonymy for both extensional and extensional-plus-modal languages, draws the line only at fully enriched languages with intentional contexts and indirect discourse. He does not actually say that there are no synonyms in such a language, but that "nothing short of synonymity will guarantee interchangeability in a language of this type." The implied conclusion is that there is no such "synonymity". "Synonymity," in Leonard Linsky, ed., Semantics and the Philosophy of Language (Univ. of Illinois Press, 1952), p. 121.


46. Martin, op. cit., p. 46.

47. The problem of establishing standards for an expression being an explication of another expression is one that faces Quine also. He does not always use a more explanatory standard than Carnap and Martin.

49. LPoV, p. 35.
50. Ibid.
52. LPoV, pp. 23-4; Meaning and Necessity, pp. 9-10.
53. LPoV, p. 36. Carnap, in "The Concept of Intension for a Robot" in Meaning and Synonymy in Natural Languages, attempts to take the "mental or behavioral" factors into account. This attempt depends on knowing a great deal about the "internal structure" of the robot (pp. 241, 247) and contains the further questionable assumption that because a property is "observable" it more or less points itself out. (p. 245) The isolation of properties presents a problem for both the extension and intension of terms, as will be discussed below.
54. LPoV, p. 42; WP, pp. 11, 12-3.
55. LPoV, pp. 43-4.
56. A somewhat revised version of a 1951 lecture, in LPoV.
57. LPoV, p. 60.
58. LPoV, p. 62.
59. LPoV, p. 63.
60. LPoV, p. 61.
61. The Diversity of Meaning, op. cit., p. 91.
62. LPoV, p. 61.
63. LPoV, p. 62.
64. At most, this is a claim that language is "regulative" of experience. J.W. Swanson, "Linguistic Relativity and Translation," Philosophy and Phenomenological Research, 22 (1961), pp. 138-9.
65. B.L. Whorf, Language, Thought and Reality (MIT Press, Cambridge, 1964), pp. 213 (general statement), 215-6 (examples); 59 (general statement), 59-60 (examples); 138 (general statement), 140, 141, 143, 144-5, 146 (examples).
66. LPoV, p. 62.
67. LPoV, p. 62.
68. LPoV, p. 63.
69. Whorf, op. cit., e.g., pp. 209-11.
70. LPoV, p. 63.
71. Ibid.
72. W&O, p. 28.
73. W&O, p. 66.
74. W&O, p. 34.
76. Ibid., p. 21.
78. W&O, p. 38.
80. W&O, p. 36.
82. Ibid.
83. W&O, p. 68.
84. W&O, pp. 9, 13-14.
86. LPoV, pp. 58-9.
87. LPoV, p. 58.
88. Ibid.
89. W&O, p. 53.
91. W&O, p. 35.
92. W&O, p. 72.
93. Meaning and Necessity, p. 236.


99. By the correlation discovered by G.K. Zipf between the length of a word and its frequency of use.


101. W&O, p. 70.


103. W&O, p. 70.


105. Ibid.

106. W&O, p. 73.


108. For example, W&O, pp. 22, 24.


111. W&O, p. 76.


114. See references in note 65.

FOOTNOTES TO CHAPTER II

3. W&O, Chapter IV, passim.
5. Ibid.

10. W&O, p. 158. See also J.W. Swanson, "On the Calculus Ratiocinator," Inquiry, 8 (1965). The philosophical use of formalization is held to be "more than a mere sprucing up and formalizing of something already contained, only a bit messily, in ordinary language." (p. 318) Regimentation is probably closest to Swanson's type (h) "ideal language," the effect of which is to "lift out the skeletal (i.e. the logical) structure of a discipline, to make explicit all the undefined terminology of the system, and to show how the consequences follow from the first principles and how the various definitions are interrelated." (p. 321)


15. W&O, p. 158.


17. Chomsky, whose example this is, believes that neither is ambiguous, and that they illustrate the nonequivalence of an active-voice sentence
17. (cont.) and its related passive-voice sentence on the intuitive semantic level. He believes that (1) refers to any two languages and (2) to the same two. Although there is some presumption in favor of this interpretation, there is still ample room for ambiguity. Syntactic Structures (Janna Linguarum, Mouton, The Hague, 1957), pp. 100-1.


23. W&O, p. 160; see also "The Scope and Language of Science," in The Ways of Paradox, p. 232. Restricting predicates is viewed there as the intrusion of "ideology".


28. Ibid.; see also W&O, pp. 242-3.

29. W&O, p. 204.

30. Ibid.

31. Ibid.


33. Except to say that they "scout the whole notion of proposition" since it is a necessarily interlinguistic concept, pp. 205-6.

34. W&O, pp. 258ff.

35. W&O, pp. 221, 228.


40. LPoV, p. 27.
41. LPoV, p. 25.
44. IMP, p. 167; "Reply to Criticisms," in Schilpp, op. cit., p. 690.
47. IMP, p. 176.
52. PM, p. 67.
54. IMP, p. 169.
55. IMP, p. 171.
57. PM, p. 67.
58. IMP, p. 178, "On Denoting," pp. 481, 482.
59. IMP, p. 179.
60. PM, p. 66.
61. IMP, p. 174.
62. PM, p. 66.
63. IMP, pp. 178-9.


66. IMP, p. 170.

67. IMP, p. 168.

68. "Russell's Theory of Descriptions," p. 188.

69. Ibid., p. 189.

70. Ibid., p. 207.

71. PM, p. 69.


73. PM, p. 68; "On Denoting," p. 182.

74. IMP, p. 177.

75. PM, p. 68.


77. Ibid., p. 175.

78. Ibid., p. 177.

79. Ibid.


82. Ibid., p. 182.

83. Ibid., p. 182.

84. PM, p. 67; IMP, p. 175.

85. IMP, p. 175.

86. Ibid.
87. In so many places here we will deal chiefly with "On What There Is," in LPOV, Methods of Logic, revised ed. (Holt, New York, 1959), and the relevant passages in W&O.

88. W&O, p. 184; Methods of Logic, p. 223.

89. W&O, p. 184.

90. Methods of Logic, p. 224; see W&O, p. 192, n. 1, for a reminder on how to interpret this.


92. Methods of Logic, p. 197.

93. LPOV, p. 4.

94. Methods of Logic, p. 201, emphasis supplied.

95. IMP, p. 178.


98. W&O, p. 163.


103. IMP, p. 165; see text, p. 168.

104. IMP, p. 170.

105. Mathematical Logic, p. 150.


108. Ibid.

109. Ibid.

110. Ibid.

111. W&O, p. 96.
114. W&O, p. 179.
115. Ibid.
117. LPoV, pp. 7-8.
119. LPoV, p. 6; emphasis supplied.
120. See text, pp. 42-3.
121. Quine has not concerned himself with the problem of "semi-grammatical" sentences and degrees of grammaticalness, as Chomsky, Katz, and Ziff have; see Fodor and Katz, The Structure of Language, for these papers. For Quine's discussion of "significance", see "The Problem of Meaning in Linguistics," LPoV, pp. 48-51.
123. LPoV, pp. 8-12.
125. Methods of Logic, pp. 218-9, emphasis supplied.
128. Ibid. (1) (x)(if x = a and \ldots x \ldots then \ldots a \ldots).
(2) If (\exists x)(x = a and \ldots x \ldots) then \ldots a \ldots.
(3) If \ldots a \ldots then (\exists x)(x = a and \ldots x \ldots).
(4) \ldots a \ldots \equiv (\exists x)(x = a and \ldots x \ldots).
129. Ibid.
134. Ibid.; also n. 5.
137. See Methods of Logic, p. 223.
139. Ibid.
140. W&O, p. 186.
143. W&O, p. 177.
144. Methods of Logic, pp. 219-20.
145. W&O, p. 204.
146. W&O, p. 189.
147. See Epstein, op. cit., p. 676, for examples of possible, if unres-

restricted, "translations".
148. W&O, p. 204; see text, pp. 39-40.
150. W&O, p. 158.
154. Ibid.
156. W&O, p. 79.


164. See, for example, *Syntactic Structures*, *op. cit.*, p. 103.

165. "The Scope and Language of Science," in *The Ways of Paradox*, pp. 226-32; *Mathematical Logic*, pp. 120-1; *I60*, sec. 431, "Two Dogmas of Empiricism*" *IPO*, pp. 30-1. Chomsky relates, in a letter to me (November 9, 1967), that he went through an early stage of thinking that Goodman's inscriptionsal approach to "sameness of meaning" might be applicable to linguistics. (Journal of Symbolic Logic, 18 (1953)) He writes that he "mentioned Goodman [in *Syntactic Structures*, pp. 103-4, n. 10] because of his attempt to incorporate some part of [the theory of meaning] in the theory of reference, an attempt which I felt was analogous to my attempt to show that part of what had been called 'theory of meaning' was reconstructible within syntax... I am now much less persuaded than I was in 1957 that Goodman's approach to problems of meaning has merit [for linguistics]."

166. *Syntactic Structures* was published in 1957, marking the "birth" of transformational grammar, although Zellig Harris's work earlier in the fifties anticipated some aspects of the new approach. It took some more time for possible consequences for philosophy to be appreciated. Zeno Vendler, in *Linguistics in Philosophy* (Cornell Univ. Press, Ithaca, 1967), is one of the more optimistic philosophers in his view of the fruitfulness of collaboration between the disciplines. Chomsky, in a paper delivered at the New York University Institute of Philosophy (April, 1968), declared Vendler overoptimistic. He said that it would be only a "lucky accident" if the expressions which interest philosophers on philosophical grounds coincided with those of interest to linguists on linguistic grounds.

167. See footnote 37. of Chapter I (p. 150) for Katz's comment on this method of "direct" translation.

168. *I60*, p. 159.

169. *I60*, p. 205. One might want to reject taking the logic as a model for natural language, arguing that it is instead a part of that


171. W&O, p. 60. An early hint of indeterminacy occurs in "Logic and the Reification of Universals," first written in 1939, and amended in 1947 and 1950, where Quine remarks that in "a really alien language" one may not be able "to determine... any firm analogue of quantification." LPoV, p. 107.


173. Ibid.


FOOTNOTES TO CHAPTER III


3. E.g., Bruce Ames, Knowledge, Mind, and Nature (Random House, New York, 1967), pp. 201-2, 278 (n. 20); Max Black, orally in class, Cornell University, 1968; Steven Davis, op. cit.

4. W&O, p. 66. For Quine's use of "intuitive", see p. 36m.

5. LPoV, p. 2kl.

6. Ibid.


11. Ibid.

12. Ibid.


14. For the role of "analogy" in language learning, see W&O, p. 9f for its role in translation, pp. 70-1.

15. W&O, p. 27, emphasis supplied.


17. The Diversity of Meaning, pp. 93-4.


The Diversity of Meaning, p. 93.

J.A. Fodor, "Could Meaning Be an rm?" op. cit., p. 78.

Aune, op. cit., pp. 101-2; Cohen, op. cit., p. 70; Vygotsky, op. cit., p. 69.

Cohen, op. cit., p. 57.

There is undoubtedly a problem in picking out the "distinctive sort of behavior" that goes with the use of particular expressions, comparable to the problem of picking out the correct features of a thing for an associationist theory. William Todd, "The Theory of Meaning and Some Related Theories of the Learning of Language," Inquiry, 8 (1965), pp. 365, 374. Perhaps this broader approach is more development-oriented: a word can be used before it is thoroughly understood. But can a word be used at all before a stimulus-response bond is formed, according to an associationist theory?

There is no reason to grant that all physical-thing words are learned before any others, because it seems clearly untrue.

The ambiguity of what x is, the possibility of describing x under different categories, exists whether x is a physical thing or an action.


Ibid., pp. 77, 83.

Ibid., p. 86.

Ibid., p. 93.

Ibid., pp. 84-5.

Ibid., p. 102, n. 29.

Models and Metaphors, op. cit., p. 61.

Ibid., p. 58.

Ibid., p. 61.

Ibid., p. 60.

Ibid., p. 59.

2. Ibid., p. 62.

3. Ibid., p. 62 and n. 22. Black takes the view that 'The ...' sentences imply that 'there is a ...' and criticizes Strawson, p. 63.


5. Ibid., p. 94.


7. Ibid., pp. 85, 86.

8. Ibid., p. 86.

9. Ibid.

50. V.C. Chappell, ed., op. cit.

51. Cavell, op. cit., p. 78.

52. Ibid.

53. Ibid., p. 111.

54. Ibid., p. 112.

55. Cohen, op. cit., p. 79.

56. Ibid., pp. 4-5.

57. Ibid., p. 13.

58. Ibid., pp. 31-2.

59. Ibid., pp. 161 ff.

60. Ibid., pp. 29, 115-6.

61. Ibid., p. 152.

62. Ibid., p. 33.


64. Cohen, op. cit., p. 31.
66. Ibid., p. 57.
67. Ibid.
68. Ibid., p. 58.
69. Ibid.
70. Ibid., p. 60.
71. Ibid., p. 61.
72. Ibid.
73. Ibid., p. 62.
74. Ibid.
75. Ibid., p. 64.
76. Ibid., p. 79.
77. Ibid., p. 66.
78. Ibid., pp. 79-80.
83. W&O, p. 47; see text, p. 87.
86. LPoV, p. 60; W&O, p. 28m.
89. W&O, p. 28.
There are references to "empirically unconditioned variation in one's conceptual scheme", "man's conceptual sovereignty", and to the individual "diversity of connections" compatible with the same verbal behavior, W&O, pp. 26, 5, 13, 8. It is not clear to what extent these are references to creative theory formulating and individual differences and to what extent they are references to the conceptual differences among languages. If the latter is what is intended by the "empirically unconditioned variation in one's conceptual scheme," Quine would be hard pressed to account for this within his theory except by innate mechanisms.

103. Ibid., p. 340.
104. Ibid., pp. 343-4.
107. Ibid., p. 209.
109. Ibid., p. 178.
110. Ibid., pp. 179-80. Cf. Cohen: this includes both "utterances supple-
mentary of context" and "functions" of words. Ams's first addition
is the means to refer to and characterize "linguistic roles".

111. Ibid.


115. Sellars, ibid.

116. Ibid., p. 185.

117. Ibid., p. 189. No one would call a physicist unempirical; Q.E.D.


119. Ibid., pp. 113, 117.

120. W&O, pp. 72, 22.

121. W&O, p. 22.


124. W&O, p. 16.

125. W&O, p. 11.

126. W&O, p. 19. See also "Two Dogmas" in LPoV on the "germaneness" rela-
tion and on "recalcitrant experience", pp. 42-4.

127. Skinner was quoted in The New York Times (March 17, 1968) as saying
of Chomsky's review of Verbal Behavior, "I read a bit of it... and saw that he missed the point, so I never read the rest. I never
answer any of my critics. I generally don't even read them. There
are better things to do with my time than clear up their misunder-


129. W&O, p. 73.

p. 48.

131. Max Black, "Necessary Statements and Rules", Models and Metaphors,
pp. 74-8.
132. Lazerowitz uses the necessary/contingent distinction, but rules out the possibility of synthetic a priori propositions so that it does not differ from an analytic/synthetic distinction. See Lewis White Beck, "On the Meta-Semantics of the Problem of the Synthetic a priori," *Mind*, 66 (1957), p. 230.


135. A and B are both contingent propositions. It is not the case that 'Necessarily (A\(\rightarrow\)B)' entails either '(A\(\rightarrow\)Necessarily B)' or '(A \(\rightarrow\) Impossible -B)'. Let A be the statement of a linguistic rule (e.g., ' Bachelor' means unmarried male' is a linguistic rule) and let B be the associated statement that a sentence expresses a necessary proposition (e.g., 'Bachelors are unmarried males' expresses a necessary proposition). The dichotomist thinks that he avoids the circularity of his position by insisting that he is not illegitimately asserting that, if A, then necessarily B.


137. Ibid., p. 157.

138. Ibid., p. 156.

139. Ibid., p. 158.

140. Ibid., pp. 142-3.

141. Ibid., p. 143.

142. Ibid., p. 145.

143. Ibid., p. 146.

144. Ibid., p. 147.

145. Ibid., pp. 147-8.

146. Ibid., p. 148.

147. Ibid., pp. 149-51.


149. Ibid., pp. 529-30.

150. "The Analytic and the Synthetic — An Untenable Dualism," in Linsky, *Semantics and the Philosophy of Language*, op. cit. White's position is virtually identical to Quine's in "Two Dogmas," and apparently they were in close contact during the evolution of these criticisms; see W&O, p. 67n7. White's paper even appeared a little earlier than Quine's, but nevertheless the renown of "Two Dogmas" pushed it into the background.

152. Mates does not acknowledge that he has chosen, from a number of possibilities, particular criteria for 'analytic' and 'synonymous', and that his experimental results might be quite different with different tests.


154. Ibid.


160. Ibid., p. 237.


162. Ibid., p. 71.


164. "On the Verification of Statements About Ordinary Language," pp. 69, 73.


166. Ibid., p. 483.

167. Ibid., p. 484.


169. Ibid., p. 492.

170. Ibid., p. 494.


172. Ibid., pp. 490-1.

173. Ibid.
174. Ibid., pp. 493-5.
175. Ibid., p. 492.
176. Ibid., pp. 494-5.
178. Ibid., pp. 258-61.
180. Ibid., p. 361.
181. Ibid.
182. Ibid., p. 368.
183. Ibid., p. 375.
184. Ibid., pp. 374-7.
185. Ibid., pp. 378-80.
186. "It Ain't Necessarily So," pp. 659-60; lecture on "Natural Kinds" to Graduate Philosophy Club, Cornell University, February, 1968.
188. Ibid., pp. 392-3.
189. Ibid., p. 396.
190. Ibid., p. 362.
194. Ibid., p. 543.
197. Ibid., pp. 494-5.
198. Ibid.
199. Ibid., p. 497.
200. Ibid., p. 496.
206. Ibid., p. 542.
207. Ibid.
209. Ibid.
210. Ibid., p. 51.
212. Philosophical Investigations, par. 81.
213. Ibid., e.g., pars. 43, 556-67.
215. Ibid., p. 149.
216. Ibid., p. 162.
217. Ibid., p. 183.
218. Ibid., p. 182, quoted from Quine, Methods of Logic, op. cit., p. xiii.
219. Ibid., p. 183.
220. Putnam speaks of necessary, rather than analytic, truth here because the examples do not involve "one-criterion" words. "It Ain't Necessarily So," p. 659.
221. Ibid., p. 671.
223. Ibid., p. 149.
225. Ibid.
226. Ibid., p. 154; emphasis supplied.
227. Ibid., p. 73.
228. Cohen, op. cit., p. 162.
229. Ibid., p. 164. See examples given.
230. Ibid., p. 219.
231. Ibid., pp. 249-50; of text, pp. 144, 146.
232. Ibid., p. 252.
237. Grice and Strawson, op. cit., pp. 150-2. If one is not sympathetic, one can always contrive a case in which the three-year-old who is asserted to be an 'adult' does (or can be imagined to) possess some attributes of adulthood, perhaps all except age. Furthermore, one can ask what it means not to be able to imagine something. This is never made really clear in the appeal to the criterion of inconceivability.
239. Harman points out that one important difference between 'means the same' in ordinary and in philosophical use is that the former is equivalent to 'has a similar meaning' which lacks the property of transitivity of 'means the same' in its philosophical use. "Quine on Meaning and Existence," I, pp. 149-50. Grice and Strawson may only be claiming that there are two areas of agreement: within the philosophical tradition on 'analytic' and within ordinary discourse on 'means the same'. However, in that case, they cannot use ordinary discourse to elucidate the philosophical problem.


248. Ibid., p. 52.

249. Ibid., p. 53.

250. Ibid.

251. Ibid.

252. Ibid., pp. 53-4.


254. Ibid., p. 42.


258. The Diversity of Meaning, p. 184.

259. LFoV, pp. 57-9.

260. LFoV, p. 59.


BIBLIOGRAPHY


"On Likeness of Meaning." Analysis, 10 (1949-50).


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"On Understanding 'Understanding Utterances'." Reprinted in *Fodor and Katz, eds., The Structure of Language*.