ON SOME ASPECTS OF THE
CONCEPT OF TRUTH

by

Daniel Isaacson

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Preface

My endeavor in this thesis is to investigate certain fundamental issues in the philosophy of language. These include, in particular, 'how does language mean?' and 'how do we frame our conception of reality?' These two questions are closely related, in that an answer to each involves, essentially, reflections on the concept of truth. My general approach is anti-realist, in the sense Dummett has given to this term. To a far greater extent than the actual number of citations of his works may indicate my indebtedness, this thesis owes its conceptual framework to Dummett's construal of the opposition between realism and anti-realism.

Following Dummett, I model anti-realism on Brouwer's intuitionist construction of mathematics. My concern is primarily with empirical discourse. My general claim is that a thorough-going empiricist conception of language theory entails some form of anti-realism, including adherence to logic akin to that of mathematical intuitionism. I argue that verificationism went some way toward this conception of anti-realist semantics, but incoherently, for being founded upon a realist notion of truth. Quine's conception of language gives promise of consistent empiricism. His program for radical translation may be seen as verificationism, gone holistic; in the process, truth takes on a properly anti-realist character. I fault Quine for his advocacy of classical logic, in the light of his essential anti-realism. Davidson's program for semantics can be viewed as making explicit the role of truth in Quine's conception. In so doing, he shows how a Tarskian theory of truth can be empirical.
His account of meaning by way of truth theory makes explicit the role of truth in an anti-realist conception of truth and meaning.

The help of many people has been invaluable to whatever progress I may have achieved on the topics of this thesis. I am grateful to them, not only for their stimulation to my thinking on these topics and for contributions to the effort of producing this volume, but also, and especially, for the happiness of friendship.

Burton Dreben took me on as a student ten years ago, and I feel strongly that both the ideas and the excitement which he imparted in three years as my tutor, and in occasional conversations since, lie behind very much of what I am attempting to do here (even though I take it his sympathy for mathematical intuitionism is no greater than Quine’s). To P.F. Strawson I owe the impetus for embarking upon the thesis topic here presented. His patient rejection (during supervision of my first term in graduate study) of my ill-supported claim that there is more to the notion of truth than is revealed in its performative character set me to thinking along lines whose development I here submit. A.J. Ayer, my thesis supervisor since Trinity Term, 1968, has provided a sympathetically critical testing ground for my inchoate ideas, enthusiastically stimulating their development. His encouragement has been invaluable. By the great kindness of giving me use of his college study as a place to write and encouraging me in the development and expression of my ideas, Peter Hacker made this thesis feasible, at a time when it had seemed all but overwhelming.

Many others have, by discussion and encouragement, contributed to my efforts. They include Gordon Baker, Oswaldo Chateaubriand,
Donald Davidson, Michael Dummett, Gareth Evans, Gilbert Harman, Saul Kripke, Charles Parsons, Kenneth Wachter, Scott Weinstein. From each I have learned a great deal, and to each I am grateful.

Friends, and friends of friends, have pitched in during these final days to help with the arduous work of production on this volume. Their proofreading, Xeroxing, collating, and providing transport were indispensable. Roberto Minio-Paluello did all these things, and more. Our discussion of points to do with intuitionist mathematics saved me from blunders. He prepared the fully detailed bibliographic references from scrappy lists consisting mostly of no more than the brief citations given in the body of the text. I am grateful to him, and to Janet Berg, George Fleet, Samuel Guttenplan, Peter Hoy, Godfrey Keller, Alex Nicoll, Sally Turner for their invaluable and cheerful help.

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Abstract

Two aspects of truth constitute the subject of investigation in this thesis. These two aspects arise in the dependence of truth on language and fact. A statement is true or false, as the case may be, jointly in virtue of what it means and of how things are. This double dependence of truth on meaning and reality establishes prima facie interconnections between these notions, which I am here concerned to analyse.

Consideration of these interconnections with respect to individual sentences suggests that truth is dependent on meaning. After all, we cannot begin to assess a statement as to its veracity unless we first understand it, that is to say, grasp its meaning. This claim is unexceptionable, but only from a vantage point which precludes a general understanding of the concepts involved. It quite leaves out of account the evident consideration that it is only in the context of a language that a collocation of symbols or sounds is endowed with sense. While it must be that, to determine the truth or falsity of a statement, we have first to know its meaning, it does not follow that the concept of truth is to be analysed in terms of meaning. And indeed, at the level where we consider meaning in terms of the systematic functioning of language, we find that truth underlies meaning. In §1.2, I consider four arguments which aim to show that an answer to the question "how is a statement endowed with meaning?" is to be couched in terms of truth.

The fundamental idea, going back to Frege, and echoed by Wittgenstein in the Tractatus, is that we understand a statement when we know what must
be the case in order for it to be true; we then know what it says or asserts. On this view, the meaning of a sentence is to be identified with its truth conditions. This argument from understanding is complemented by what I term 'the argument from translation'. Quine sets the problem of meaning in the context of translating the speech of a community radically alien from ours, in the sense that there is no bilingual, or chain of bilinguals, whom we can call upon to translate for us. Left to our own devices, we break into the alien conversation by effecting correlation of sentences true in their language with true sentences of ours. The process begins with simple sentences, where the occasion makes evident the subject of discourse, and the simplicity of what is said allows us to take sincerity as a guarantee of truth. With sentences which are only mediately concerned with the perceptible present, the underdetermination of theory (what is true) leads to the indeterminacy of translation. The arguments from translation and understanding are complementary. The enterprise of radical translation provides a genetic account justifying the claim that understanding consists in the capacity to recognize truth when it obtains. Conversely, if we can take understanding a statement as knowing in what circumstances it is true, the methodology of translation is immediate.

These two arguments seem to me crucial in the proper analysis of an interconnection between truth and meaning. I consider and fault as inadequate Davidson's attempt to utilize in this regard less substantive considerations based on purely formal properties of truth and meaning.

I then turn attention to the second aspect of truth, its dependence on fact. Two different conceptions of truth are developed, corresponding
to Dummett's distinction between realism and anti-realism. I present
the issue in terms of contrasting expressions of modesty. The realist
is impressed by man's relatively insignificant niche in the universe
considered as a whole, and takes it that vast ranges of fact lie outside
the ken of human cognition. Statements are rendered determinately
true or false by these facts. The anti-realist is impressed rather by
a different sort of human limitation: man is limited to his rational and
perceptual faculties in saying how it is with the world around him. Thus
he cannot even frame a conception of fact utterly inaccessible to human
determination.

While the issue has to do with the character of truth, (roughly, objectivity
vs. subjectivity), the grounds on which the positions are to be held turn
on considerations as to the character of meaning. In virtue of what are
statements describing situations endowed with sense? At least this is
the way the anti-realist views these matters. As I discuss, the realist
attempts to justify his position not in terms of how statements have
meaning, but from ontological considerations. This ploy cannot succeed,
as it merely invites a query as to how we attach sense to the existential
assertions.

I describe something of the contrast between classical and intuitionist
mathematics, and focus on the issue of existence of discontinuous real
functions. The realist position is analysed as resting upon an illicit
appeal to pictures, with the justification for that move supplied by the
principle of excluded middle. But no justification for the principle is
forthcoming.
In Chapter 2 I focus attention on the verificationism of the logical positivists. Verificationism is seen to be a form of anti-realist semantics since meaning of a statement is given in terms of capacity to determine whether the statement obtains. But as practised by the positivists, the doctrine constitutes a potential counter example to the general scheme delimited in Chapter 1. As I discuss, verificationism was espoused jointly with a classical (realist) conception of truth. Carnap's professed grounds for adherence to the classical conception of truth is Tarski's analysis of truth. I argue, however, that mere conformity of a concept to convention T, or even taking the very form of a Tarskian deductive theory, is insufficient to determine the realist/anti-realist character of the concept so characterized. The argument is that Convention T and Tarskian theories are readily interpretable intuitionistically.

In §2.3, I consider how positivists came to advocate these disparate conceptions. The point is that verificationism arose not as a contribution to semantics, but as a polemical tool in the program to excise metaphysics from philosophy. The starting point was a thoroughly realist adherence to the law of excluded middle. Every form of words which makes a statement is thereby true or false. Pseudo-statements are forms which appear to be statements, but fail to be either true or false.

How is this condition transposed into the criterion of verifiability? I describe a two step argument. At the first step a statement, which is true or false, is declared to be so either in virtue of its meaning alone, or in virtue of experience (analytic/a posteriori dichotomy). This principle
Hempel labels the "fundamental tenet of modern empiricism." He takes positivism to be advancing a further thesis when it is claimed that every cognitively meaningful statement is, if not analytic or contradictory, "capable, at least in principle, of experiential test". This second step in the argument was never adequately developed by positivism, and already in the 1930s Hempel and Carnap were prepared to give up verificationism in favor of a weaker condition of translatability into an empiricist language. I consider briefly the difficulties with this notion.

Attention is focused on an argument by Schlick in which he seeks to achieve this second step. The argument trades on an equivocation between sentence and word meaning. A sentence can "have something to do with experience" by virtue of its predicates being observational in character. But this condition fails to guarantee sentence confirmability unless one has an account, as the mathematical intuitionists do and the positivists did not, of how the sentence forming devices relate the observations determinative of applicability for those component predicates to observations determinative of assertability for the sentences in which they figure as components.

Having considered how realist truth and anti-realist semantics came to be jointly and unwarrantedly maintained, I then demonstrate in the next section an incoherence arising from the combining of constructivist semantics with a classical conception of truth. The point is to exhibit sentences which, on a realist conception, are possessed of a determinate truth value, but are nonetheless absolutely undecidable. I consider some examples by R. Swinburne aiming to achieve something like this. I find
his arguments badly flawed, but viewed differently, his examples can
be made suitably recalcitrant to the positivist conception. The crucial
point is that unlimited universal quantification over moments of time
can create absolute undecidability. I take it this is a point already
noted by Dummett in his consideration of the example "A city will never
be built here."

The final section of Chapter 2 turns to an assessment of the legacy of
verificationism for anti-realist semantics. Merely eliminating the
incoherence demonstrated in the previous section by adopting an appropriately
constructivist conception of truth leaves a range of problems already familiar
to verificationism. I consider in particular the difficulties over identity
of sense between the same form of words in application to the remote and
recent past. I suggest that a notion of analogical uses of language, as
opposed to verificationist notion of analogical (in principle) evidential
experience, may prove viable.

Chapter 3 is concerned with Davidson's program for semantics of
natural languages, as given by Tarskian truth theories. My claim is
that this program ultimately represents a form of anti-realist semantics.
But first it is necessary to argue against Davidson's espousal of a weak form
of realism. He attempts to extract an (etiolated) version of correspondence
out of a Tarskian theory. My argument there extends the point as to the
philosophical neutrality of Tarski's analysis of truth and ties it to what
is wrong with attempts to found a realist conception of truth in ontological
considerations.

I then assess grounds for identifying semantics (theory of meaning -
not in Quine's sense) with theories of truth. Having earlier faulted
Davidson's purely structural arguments for the identification as inadequate to establish the point, I now consider the program in terms of Quine's account of radical translation. On this view of these matters, the identification of semantics with truth theories takes on a certain transparency. The empirical nature of the enterprise of constructing a theory of meaning for a natural language is also a direct consequence of this construction. Davidson is explicit in the consonance of his position with Quine's, even if he has wanted to get to this point by other arguments. And he is also correct when he says that "in suggesting that an acceptable theory of radical translation take the form of a recursive characterization of truth, I go beyond anything explicit in Quine." Still, it is possible to overdo the consonance between Quine's account of radical translation and a truth theory. The operation described by Quine is not the testing of a theory of meaning, it is the theory of meaning. And what is described in Chapter II of Word and Object is the process by which we test a theory of truth for the alien language. This distinction underlies the contrast between theories of meaning based on so-called 'absolute' theory of truth and ones in terms of 'relativized' truth.

I conclude § 3.3 with criticism of Davidson's finiteness condition on theories of truth. The last section of Chapter 3 concerns the sort of work in empirical linguistics which will need to be accomplished if the program Davidson envisages is to be realized.

The final chapter is devoted to consideration of Quine's views on the matters here of concern. I begin with consideration of his attack on verificationism, as over the "Two Dogmas of Empiricism." In view of my account of the development of verificationism from the analytic/
synthetic (a posteriori) dichotomy sketched in Chapter 2, it is puzzling to find Quine declare the two 'dogmas' at root identical. And that puzzlement is compounded by Quine's appeal to holism to block these dogmas, for he attributes an appreciation of the effects of holism to Carnap. Yet Carnap, while giving up verificationism on considerations from holism, continues as a staunch adherent of the analytic/synthetic distinction. But as passages from the Logical Syntax of Language make clear, it is a relativized notion of analyticity, the relativism a product of the holistic underdetermination of theory. It is thus tempting to consider whether this view of Carnap's is not the same doctrine, in transposed terminology, as Quine's indeterminacy of translation. The fit is strikingly exact. But in the end there is a critical difference, which rests upon a fundamental disagreement as to the proper conception of language.

Carnap's relativized analyticity turns on supposing a language specified, or created, by selection of its meaning postulates. For Quine, a language can be nothing other than a natural language (even if on occasion a regimented natural language). A language is embodied in the complex dispositions to verbal and other behavior manifest in a community of human beings. The point of contrast between Quine's and Carnap's conceptions is that between a formal and a formalized language. For Carnap, meanings are selected by L-rules, and grasped by human minds in accepting the L-rules. Quine's thoroughly empiricist view of language and meaning rejects this entire picture.

In §4.4 I argue that Quine's view of individual sentences as holistically dependent on theory renders truth immanent to human perceptions, and
so anti-realist in character. At the same time, the constraint of theory by observation sentences avoids the incoherence of unalloyed coherence theory.

The final section of Chapter 4 is a consideration of Quine's view of the status of logical truths. I argue that his general commitment to an anti-realist conception of truth makes intuitionist logic the natural notion of logic in his scheme. And his specific arguments as to the radical translation of the sentential connectives fail to support classical logic, which he supposes they do, and rather count for the correctness of intuitionism. This ought occasion little surprise, as the classical, transcendent notion of truth must, by its very character, prove incommensurable with any humanly determinable notion of truth (assertability). The role of logic as merely one theory among many (though distinguished by extreme generality) seems to me also a point of consonance between Quine and Brouwer.
CHAPTER 1

TRUTH, MEANING AND REALITY

§1. Introduction

Two aspects of the concept of truth, and their interaction, constitute the focus of concern in this dissertation. These two aspects reflect a basic dualism characteristic of truth. This dualism finds common expression in the idea that a statement is true jointly in virtue of what it means and of how things are. Or to borrow a metaphor from mathematics, truth is a resultant of two vector components, one linguistic, the other factual. The two aspects of truth to be extracted from this widely accepted picture are connections with meaning, and connections with reality. But useful as it is, this suggestive picture does not so much represent as misrepresent the conceptual interplay of these notions. In particular, it strongly suggests that meaning is prior to truth. A theme pervasive in this dissertation is that an account of meaning is to be rendered in terms of an account of truth. This claim is not directly incompatible with the idea that the truth of a sentence is dependent on its meaning. The claim concerns what will enable us to achieve an account of meaning as a general phenomenon; it is not always to be expected, even if it is sometimes the case, that an encompassing theory will merely generalize the particular account which seems appropriate for individual instances subsumed under it. In the matter of truth and meaning there is all the difference between focusing on a single sentence and considering an entire language. It is nonsense to take a particular collocation of symbols as a sentence without knowing the language to which
it belongs. And by the same token it would be absurd to raise the issue of what that expression means and whether it is true. A language is something beyond and distinct from the totality of sentences in it which get used; rather, it is the device which generates those instances, the capacity which allows comprehension of as yet unencountered concatenations of words. These reflections indicate that the connection of truth and meaning with a language is primary, the interaction of those notions at sentential level secondary.

It might be supposed that this line of consideration is repeatable, with consequence that the notions of truth and meaning and their connection with reality are abstracted beyond particular languages. To do so would constitute a mistake, opposite but comparable to supposing the issue properly set at the level of sentences. We do want a general account of the concepts involved. But the generality must be that the account as it stands will apply to any given language. Those who might succumb to the temptation to seek even greater abstraction may do so on the view that language is no match for reality. The existence of language is an accident of biology (evolution), while necessarily there is a reality, even if what is real is not necessary. Propositions are invoked to accommodate these sentiments. It is in them that meaning resides (they are meanings); and when they match reality, they are true propositions. Appeal to propositions occurs sometimes within the framework I am here attempting to establish for the consideration of truth, meaning and reality. Propositions are invoked to explain how a particular linguistic system is endowed with meaning, how its sentences succeed in being true. And what propositions are is explained as what sentences of two different languages mean when they mean the same thing. But if it is allowed, as this explanation hints, that our only access to propositions is through language, a thoroughly
empiricist conception of language may discredit this whole notion of abstract propositions. Much of the discussion in this dissertation may be read as elaborating this point.

In the following section I will present a general discussion of the interconnection between truth and meaning. As previously noted, the claim will be that truth underlies meaning, in that meaning is to be accounted for in terms of an account of truth. The third and final section of this chapter concerns the notion that truth is a reflection of reality. Two conceptions suggest themselves, differing by the emphasis accorded reality on the one hand and its reflection on the other. The poles of this contrast receive the labels 'realism', 'anti-realism'. What can decide between these conceptions of truth? The answer is to be sought in the dependent connection of meaning with truth. What is the character of meaning which animates our linguistic behavior? How do we succeed in attaching sense to the statements we utter? In what does sense consist? Given that truth underlies meaning, answers to these questions bear upon the issue of the character of truth. In favoring constructivist answers, this dissertation is partial to an anti-realist conception of truth.

The three remaining chapters each focus on a particular account of meaning: Chapter 2 on the logical positivists' verificationism, Chapter 3 on Davidson's truth definition based semantics, Chapter 4 on Quine's account of meaning in terms of radical translation. The objective which unifies these considerations is to extract from each of the three accounts a contribution to the anti-realist scheme of truth and meaning. As achieving such a contribution was not the avowed objective of these philosophers, a certain amount of constructive exposition and interpretation of their views is required. The result is not as
implausible or unfair as might be supposed. While all three positions involve espousal of an ostensibly realist (classical) conception of truth, doing so is in each case justified by appeal to Tarski's analysis of truth. In Chapter 2 I argue that Tarskian theories of truth are, in terms of formal structure, quite neutral as between a realist or anti-realist conception of truth. This point is insufficient, of course, to establish my construal of the philosophical views considered, but it removes an impediment to it. The verificationist tenet that the sense of a statement is given in terms of capacities to determine whether the situation it describes does obtain is on the face of it anti-realist semantics. But though adherence to Tarski left them free to do so, and the interconnection between truth and meaning, as set out here, dictated it, the positivists did not embrace an anti-realist conception of truth. Statements involving quantification over all time point the contradiction in this position. Davidson's program for semantics by its very form illustrates the thesis that an account of meaning is to be rendered by an account of truth. There is an attempt, in "True to the Facts", to parlay a Tarskian theory of truth into an etiolated realist notion of correspondence. The argument fails. The program remains intact - not surprising, as the notion of truth on which it is founded turns out anti-realist. In order that the truth theories be applicable to natural rather than formal languages, Davidson modifies Tarski's techniques. The modification introduces anti-realism. The test of adequacy in terms of convention T ceases to be a logico-deductive exercise as on Tarski's account, and becomes instead an empirical enterprise.

1. Tarski himself allows that "we should like our definition to do justice to the intuitions which adhere to the classical Aristotelian conception of truth" ("The Semantic Conception Truth", p. 53 f), and evidently he takes it his analysis does this.
Meanings of sentences in the language to which the truth theory applies are established in the course of that enterprise. And they are established in terms of an anti-realist notion of truth, something like 'assertable on good evidence'.

This technique is closely akin to Quine's account of meaning in terms of radical translation. The basic ploy in radically translating is to match true sentences of their language with true sentences of ours. Thus immediately a connection between truth and meaning is set out. The notion of truth which we have here to operate with is, to varying degrees, theory dependent. Duhemian underdetermination of theory transposes to Quine's doctrine of indeterminacy of translation. This notion that sentences, especially those remote from observation, may be assessed as to truth only in the context of theory, constitutes, I argue, an anti-realist account of truth. And it is a particularly attractive version of anti-realism in allowing for enough fact (observations 'at the periphery') to avoid the embarrassment of incoherent coherence theory (a danger when we give up the simple notion of truths individually determined by facts). The dissertation concludes with discussion of logical truth in the context of Quine's anti-realism.

§ 2. Truth and meaning

Settling the issue whether a given sentence is true cannot be managed unless we have already grasped its meaning. Nonetheless there are grounds, to be set out in this section, for taking truth as primary with respect to meaning. This point is advanced in the context of claiming that the units of meaning are languages, rather than sentences. Before
proceeding to these considerations, I want to remark on the welcomness of matters turning out thus: 'meaning' is an intrinsically obscurer and more problematic notion than 'truth'. This sentiment is expressed by Quine, in the remark, "It is a striking fact that these notions [including truth], despite the paradoxes which we associate with them, are so very much less foggy and mysterious than the notions belonging to the theory of meaning." ("Notes on the Theory of Reference", p. 137). Now it is sometimes supposed that such a difference is due to the clarifying contribution of Tarski's construction of truth theories. But the case can be made out without appeal to any particular analysis of truth. Compare what is to be demanded of a theory of truth with the demands on an account of meaning. The point is that there is something clear and simple to say about truth in this regard, not so for meaning. We can explain the conditions of application of the predicate 'is true' to any given sentence in terms which have nothing to do with the concept of truth (or any other overtly semantic notion). Examples are of the familiar sort: "'Galileo died in the year of Newton's birth' is true just in case Galileo died in the year of Newton's birth." It is easy to overemphasize the uninformativeness of these pronouncements. Of course, we cannot understand such a statement unless we understand the statement to which the application of the concept truth is being explained. This is the phenomenon already noted of the priority of meaning over truth at sentential level. But assuming we understand "Galileo died in the year of Newton's birth" - and the locution "just in case" - this mini-theory fully accounts for truth as applied in this extremely limited domain of one sentence. It is a theme to be developed later that much of importance in these matters (in
particular, realism vs, anti-realism) is packed into the understanding of "just in case", or as it is often expressed "if and only if". The point does not affect directly the present discussion. That we must already operate tacitly with what may be analysed as a conception of truth does not vitiate these mini-theories as constituting the form of particular application of a truth predicate. What do the totality of all such mini-theories for the statements of a given language tell us about truth relative to the entire language? Without further reflection or abstraction, we know nothing of truth beyond a non-uniform account of its particular applications. This totality in no way constitutes an analysis of truth. But if we reflect on the character of each instance, we do realize something general about truth - what Quine has termed its 'disquotation' property. Applying the truth predicate to a quoted sentence is in particular cases nothing different from using the sentence. This observation provides an overall constraint on an account of truth at the level of a language, abstracted from its application to particular sentences. Attribution of truth is nothing more than being self-conscious about our application of language. This observation suggests that general reflections on truth may tell us something general about our language. What now of the relative dubiety of meaning in comparison with truth?

Suppose on our model of mini-theories for truth we try explaining the application of the concept meaning to a particular statement as follows: "'Galileo died in the year of Newton's birth' means that Galileo died in the year of Newton's birth." Such a statement is uninformative in the same way the corresponding truth account is. But it is in a crucial
feature problematic where that for truth is not. The use of the 'that'
construction (in which the words "Galileo died in the year of Newton's
birth" are embedded as they occur on the right hand side) has the effect
that though unquoted, this form of words is not there used in its
ordinary employment. So understanding the particular sentence to which
the account is to apply is itself insufficient to guarantee understanding of
the account. What is required is a grasp of the use of that sentence
within indirect discourse. Such grasp is additional to the capacity to
employ the sentence in its usual application. Sometimes these considera-
tions are taken to constitute an argument for meanings, or propositions.
"Means" becomes a two term relation between a statement and a meaning.
It is to be doubted whether we can usefully employ the notion of meaning
in this substantival mode. But the pressure to do so illustrates problem-
atic features of meaning as against truth.

I will now consider four arguments which aim to demonstrate that an
account of meaning in terms of truth can be achieved. The first two
proceed in terms of mainly formal considerations. They might be termed
the argument from analogy and the argument from structure. Davidson
seems inclined to justify his program on the basis of these two arguments
alone. Managing this would be attractive, as they involve minimal
philosophical commitment or analysis. The next two involve more substantive
considerations. They will be referred to as the arguments from under-
standing and from translation. I take Frege and Wittgenstein in the
Tractatus to be analysing the understanding of a sentence in terms of
grasping the condition under which it is true. The treatment of these
matters in the context of translation is the technique favored by Quine.
Before going on to the consideration of these arguments, let me present some very simple observations which demonstrate the necessity of operating in the context of an entire language when we attempt an account of meaning. The argument can be put in terms of what is accomplished, and what is not accomplished by use of a traveler's phrasebook. It might seem that we can deal with the translation (account of meaning) of a single, or just a few sentences. It is informative, useful (in the proper circumstances) and correct to be told "'La plume de ma tante est sur la table' means (in French) that my aunt's pen is on the table."

Traveler's phrasebooks impart such bits of information. Two reflections on this case show it the 'exception which proves the rule', in the sense of a putative counterexample whose failure helps explain and lends plausibility to the thesis which it was to undermine. The first point is that the phrasebook-wielding traveler does not understand the language some of whose sentences (even of considerable syntactic complexity and richness of vocabulary) he bandies about with such apparent fluency. The illusion of fluency collapses at his evident incomprehension of the native's conversational follow-up, incomprehension even when the reply plays only simple changes on the eliciting remark. (This point is not vitiated by the admission that the boundary between non-fluent speaker and phrasebook-limited non-speaker is not a sharp one.) The second consideration in this case is to reflect on how the phrasebook was compiled. If we are to trust ourselves to the thing at all, we must have grounds for supposing that the author possessed a fluency in that language. Suppose not. Without loss of generality we take it he did not obtain those sentential translations from a competent bilingual (otherwise we are back at the case of the fluent author). So how did he obtain the translations for his boc
Perhaps he encountered a native who uttered assertively "La plume de ma tante est sur la table" in circumstances which justify the claim that his aunt's pen is on the table. But even if it is absolutely clear that the pen on the table belongs to the informant's aunt and is the focus of attention as the informant utters assertively "La plume de ma tante....", the myriad other equally likely possibilities as to what he might be saying are vastly heteronymous. E.g. "You should buy one of these", "What a waste of the world's resources to make pens like this", "This pen used to belong to my grandfather", etc., etc. with literally infinite variety. Clearly the would-be minimal linguist has not the least hope of getting anywhere with this enterprise until he segments the utterance into syntactically minimal components, gathers evidence for plausible renderings of them, and establishes how they are put together to form sentences. The attempt to translate one particular sentence finds him embarked on the construction of a rudimentary lexicon grammar, in short treating the sentence as embedded in a whole language, and seeking after the meaning of the particular sentence through mastery of the systematic workings of the language in which it is embedded. Take care of the language and the sentences will take care of themselves.

I turn now to consider arguments for construing this problem of meaning in terms of truth. The first of the two which limit consideration to reflections on form is that from analogy. The argument is due to Davidson. In "Truth and Meaning" he takes it that we may formulate as a sort of adequacy condition on an account of meaning that it produce all instances of "s means that p", where p is replaced by a sentence, s by a name of it. Let us call this convention M. Grounds have been adduced
for finding the instances of convention M problematic, at least in comparison with their counterpart instances of convention T. Davidson remarks on the difficulty that "in wrestling with the logic of the apparently non-extensional 'means that' we will encounter problems as hard as, or perhaps identical with, the problems our theory is out to solve" ("Truth and Meaning", p. 309). His solution (as he then allowed, "the only way I know to deal with this difficulty") seeks to exploit the structural similarity between instances of conventions M and T, as both connecting a description of a sentence with that sentence (or a translation of it). Davidson supposes that "the theory [characterized by convention M] will have done its work if it provides, for every sentence s in the language under study, a matching sentence (to replace 'p') that, in some way yet to be made clear, 'gives the meaning' of s." He proposes a "bold step" in which we "try treating the position occupied by 'p' extensionally: to implement this, sweep away the obscure 'means that', provide the sentence that replaces 'p' with a proper sentential connective, and supply the description that replaces 's' with its own predicate. The plausible result is 's is T if and only if p'" (loc.cit.). What grounds have we to expect that in boldly leaping from the intensional to the extensional, what is characteristic of truth is preserved? Why is 'if and only if' the "proper sentential connective"?, i.e. how do we know we have made the right leap? Answers to these questions are not readily extractible from Davidson's text. In "Semantics for Natural Languages" (p. 184) we encounter a briefer and if anything less plausible argument to similar effect (though this time in the opposite direction, from T to M): "Since the words 'is true if and only if' are invariant [in the instances of convention T],
we may interpret them if we please as meaning 'means that'.' Why not, if we please, interpret "is true if and only if" as meaning "causes it to be the case that". Nothing in Davidson's methodology seems to bar our way to this curious theory of causality, in which states of affairs are caused by the sentences which describe them. Though Davidson has not disavowed these arguments in print, remarks of his at various colloquia, including already the Irvine conference in the philosophy of language (summer, 1971), indicate he no longer lays much stock by them. The failure of these arguments does not entirely discredit analogical considerations. But it does leave a need for justification of any attempt to exploit the evident similarity in structure between conventions T and M as the ground for establishing that an account of meaning is obtainable from a theory satisfying (T). Such justification may be forthcoming in what I term the structural argument. It is plausibly to be viewed as a refinement of the analogical. What is compared is not only (T) and (M), but also the structure of Tarskian theories developed in conformity with (T), and desiderata on an account of meaning more detailed than that expressed by (M). What must be demanded is a systematic theory which will account for the phenomenon that with mastery of a finite stock of semantic elements and a capacity for constructing sentences out of them by various grammatical devices, we find ourselves capable of understanding any one of the potential infinity of sentences so constructible. That is, we want an answer to the question, how does word meaning contribute systematically to sentence meaning? Put this way, we are reminded of Tarski's construal of the problem of truth. Progress there was achieved in taking the question not as 'in what does the truth
of a sentence consist?", but rather as: 'how is the truth value of a sentence a function of the semantic role of its sub-sentential components?'. What may plausibly be claimed is that the two structures, how the parts of a sentence contribute to its meaning on the one hand, and to its truth on the other, are really one and the same structure. Davidson, after the analogical leaps earlier faulted, allows that this is the point achieved: "a theory of meaning for a language L shows 'how the meanings of sentences depend upon the meanings of words' if it contains a recursive definition of truth-in-L." (op.cit., p. 310). David Wiggins persuasively expresses this identification of structure utilized in constructing both theory of meaning and of truth in his paper "On sentence sense, word sense and difference of word sense. Towards a philosophical theory of dictionaries" (p. 19):

The truth-condition must have been produced by the operation of a systematic, general and uniform procedure competent to analyse any sentence of the language into semantic components drawn from a finite list of such components (i.e. a vocabulary or dictionary). And the procedure must account for the semantic structure of the sentence by showing how it could be generated by a finite number of semantically interpreted modifications or steps from one of the finite number of semantically basic sentence forms of the language.

The integration in structural terms of semantics with an account of sub-sentential contribution to truth goes back to Frege. His grounds for espousing this viewpoint are deeper, however, than mere structural similarity, involving essentially his theory of sense and reference. The doctrine follows from his notion that the reference of a complex expression is a function of the reference of its components, conjoined with the
characterization of a sentence as a complex denoting term having as reference its truth value. While I do not think that every account of meaning in terms of contribution must proceed in terms of the Fregean apparatus, the structural similarity approach does not seem to me substantial enough to promise or explain the success of such an account. Noting the similarity of structure underlying the systematic functioning of truth and meaning in application to a particular language is highly suggestive. But that similarity constitutes a phenomenon in need of explanation, rather than itself serving as explanation of the interconnection between truth and meaning. We cannot rest content with purely formal consideration of these matters. To obtain any philosophically significant conclusion requires some philosophically significant input. And so we must turn to consider the second pair of arguments, arguments which I take to be more substantive in character than the two so far dealt with.

The argument from understanding is based upon the observation that we manifest comprehension of a statement in our capacity to recognize under what circumstances it is true. The conception that "the general form of explanation of the sense of a statement consists in laying down the conditions under which it is true" (Dummett's formulation in "Truth", p. 99) I take to originate with Frege. It is based upon his construal of semantics in terms of sense and reference (with the sense of a term the "mode of presentation" of its reference ("Sense and Reference", p. 57)), along with the doctrine that sentences are denoting expressions referring to their own truth value. It follows that the sense of a statement is the mode of presentation of or means of recognizing its truth value. Frege
gives compact expression to these notions in §32 of Grundgesetze der Arithmetik (pp. 89-90 of Furth's translation):

Every such name of a truth-value expresses a sense, a thought. Namely, by our stipulations it is determined under what conditions the name denotes the True. The sense of this name - the thought - is the thought that these conditions are fulfilled. ... The names, whether simple or themselves composite, of which the name of a truth-value consists, contribute to the expression of the thought, and this contribution of the individual [component] is its sense. If a name is part of the name of a truth-value, then the sense of the former name is part of the thought expressed by the latter name.

Frege's conception of semantics lies at the root of very much and quite disparate work in philosophy of language. Wittgenstein echoes that conception in the Tractatus, as at 4.024: "To understand a proposition means to know what is the case if it is true." And Davidson, after tortuous and not very convincing progression through the arguments here earlier considered allows (albeit somewhat grudgingly) that "there is no need to suppress, of course, the obvious connection between a definition of truth of the kind Tarski has shown how to construct, and the concept of meaning. It is this: the definition works by giving necessary and sufficient conditions for the truth of every sentence, and to give truth conditions is a way of giving the meaning of a sentence." (op.cit., p. 310). Davidson's remark concludes in a holistic vein: "To know the semantic concept of truth for a language is to know what it is for a sentence - any sentence - to be true, and this amounts, in one good sense we can give to the phrase, to understanding the language."

The arguments so far considered prove cogent indifferently as the
conception of truth dealt with is taken to be of realist or anti-realist character. I want now to consider the argument from translation, based on Quine's construal of the problem of meaning. As I argue in Chapter 4, Quine's thoroughly empiricist construal of language theory carries in its train (even if unnoticed by Quine) commitment to an anti-realist conception of truth. The program of radical translation is complex, and I leave consideration of detail to Chapter 4. But I want here just to highlight the connection between translation and truth. The basic point is that radical translation proceeds by matching native utterances true in given circumstances with our description of the situation. Understanding is well along, the putative manual of translation a success, when we find that on our handling of a given native occasion sentence we and he agree on the occasions when it is true to say it. For sentences not prompted by occasion there would seem difficulties with this picture, and there is also the problem of native error. These matters lead not to discrediting the account or the enterprise, but to the realization of indeterminacy of translation. Discussion of this phenomenon will be reserved for later. What can be extracted from even a meagre account of translational semantics is a sharp sense in which truth is the fundamental notion which underlies meaning. We begin the enterprise of radical translation, knowing nothing of native meanings, by establishing simple (observation) sentences he holds true. From a basis of knowing which of his utterances are true, we come to establish what he means. This matter, and including the relevance of indeterminacy to it, is further considered in Chapter 4.

I conclude this discussion with the observation that the arguments from
translation and understanding are connected and corroborative. Radical
translation is a format into which the whole problem of meaning fits,
for as Quine would have it, radical translation begins at home. The
account in the end is seen to apply as much to fellow English speakers
as the exotic native. It is all a matter of degree. If so, then the
enterprise of radical translation (as just noted tied directly to determin-
ing the truth of utterances ) provides a genetic account justifying the
claim that understanding consists in the capacity to recognize truth
when it obtains. Conversely, if we can take understanding a statement
as knowing in what circumstances it is true, the methodology of radical
translation is immediate.

The arguments here sketched are in no way conclusive, but they are,
I feel, suggestive. Subsequent chapters will consider in some detail
various approaches to truth and meaning suggested by them.

§3. Realism vs. anti-realism: variations in the conception of truth

I turn now to consider truth in its connection with reality. Very
roughly, the issue here can be seen as the traditional problem of objec-
tivity vs. subjectivity. Let me present the matter in terms of modesty.
We may suppose everyone agreed that it is seemly man should espouse
a modest assessment of his role in the larger scheme of things. But
it is not immediately obvious which constitutes the modest viewpoint.
We may be impressed by man's contingent and relatively insignificant
niche in the universe, taken as a whole. Science reveals to us that the
universe existed long before man arrived in it, and man's science and
technology contribute a certain probability to his departing. In such
eventuality the universe continues (to suppose otherwise would be the height of immodesty). Nor indeed are such cataclysmic and vast scaled considerations the sole means of impressing ourselves with man's inessential role in the state of things. Just on earth and at present there must be realms of fact of which we are now, and may forever remain, ignorant - facts to do with situations too remote ever to fall within human purview, perhaps just too uninteresting to be enquired about and so figure in anyone's experience. What have these reflections to do with truth? Any statement describing correctly such an inaccessible situation is true. Perhaps to avoid presupposing an agent who does the stating we should express this doctrine not in terms of statements but of context-independent sentences. There are some who find this move insufficiently modest as regards man and the universe. Context-independent sentences have meaning in virtue of being part of a language, and languages exist in virtue of human development. So the truth of a statement, in virtue of its dependence on meaning, depends on man, even when the statement concerns some situation entirely remote from human action. Bernard Williams seems to give expression to this view in "Wittgenstein and Idealism", pp. 93-95. He there presents an argument which he takes to show, on the assumption that every sentence "has the meaning we give it" that "we can get the truth of any true 'S' dependent on human existence etc.; that is, prove unrestricted idealism." (As might be expected, the crucial step in the argument is "If 'S' did not have a meaning 'S' would not be true.") Two disparate responses suggest themselves, one trivial, the other significant. The trivial one is to query the validity of Williams' final step, where he says "that is, prove unrestricted idealism." Even if the truth of any true 'S'
depends on human existence, it does not follow that the situation correctly described by 'S' is dependent on human existence. Supposing that it does may result from undue reliance on the evident truisms codified in convention T. If a certain situation obtains just in case a certain sentence (of a particular language) is true, and the truth of the sentence depends on human existence, it seems ineluctably to follow that that situation obtains just in case humans exist. But the fallacy is evident. The correctness (or even existence) of instances of convention T depends on human existence. And thereby the validity of the whole argument is so dependent. The argument is blocked without the assumption of a language, and so people whose language it is. Nothing compels us to suppose that should human beings have failed, or cease, to exist, matters of non-human fact would be affected. But to be fair to Williams, his concern is with a more substantial point; "more radical, and ... meant to be more radical, than the banal thought in standing back from a sentence describing a non-human event, that if there were no human events there would be no such sentence." (loc. cit.). This leads to my second response. What is here of concern is conception of language, including how meaning attaches to statements. We may allow that truth is a notion applicable only in the context of language, and that language evolved with human development. Even so, language has an independent life of its own, once brought to creation. Relative to such an abstractly considered structure, truth matches reality in its general indifference to human affairs (when human affairs are not the issue). A language viewed synchronically is not the creation of those who speak it, but rather a tool found ready to hand. On the conception of modesty I have
sketched, every sentence in this language which expresses a complete thought (i.e. contains no indefinite reference unfixed by context) is determinately true or false, in virtue of the facts, and entirely independent of human agency.

To views such as this I apply the sobriquet "realism", with attendant adjectival and adverbial forms. The use of these words in this dissertation is mainly a matter of technical, or at any rate specialized nomenclature, saved from unbridled arbitrariness by applicability of some overtones from their unspecialized connotation. This vague appropriateness, however, is shared by particular philosophical uses of the term "realism" which emphatically I do not intend, viz. "1. Scholastic doctrine that universals or general ideas have objective existence (cf. 'nominalism', 'conceptualism'), 2. Belief that matter as object of existence has real existence (cf. 'idealism'), (The Concise Oxford English Dictionary. 4th edition (1958), p. 1010). Rather, I intend it in the more recent sense given currency by Dummett, e.g. in "The Reality of the Past", where it consists, in part, in the doctrine "that every statement is either true or false, ... independently of our knowledge" (p. 239). The contrastive notion would be rather 'constructivism' than 'idealism' or 'nominalism'. As Kreisel expresses the issue (speaking primarily with reference to philosophy of mathematics), it is a matter "not of the existence of objects, but the objectivity of facts." Not only is realism modest, conceptions of this tendency would seem to embody the sort of common sense exemplified in Thomas Carlyle's response to Margaret Fuller's announcement that she accepted the universe ("Egad, madam, you'd better"). Let me now do what I can for modesty and common sense of quite opposite persuasion.
The anti-realist is impressed by the truism that man is limited to his sensory and rational faculties in determining how it is with the world around him. Talk of situations independent of human perceptions and conceptualizations is an unwarranted and perforce unsuccessful attempt to transcend human limitations. It is immodest (chutzpah). We can only make sense of the supposition or assertion that a certain situation obtains if we grasp what it is for experience to show it to be so. This conception owes much to L. E. J. Brouwer. His paper "Consciousness, Philosophy, and Mathematics" contains the following stark, and seeming extreme statement of principle: "There are no non-experienced truths" (p. 1243). Brouwer's concerns focus on mathematics, for the most part. Mine in this dissertation will concern mainly empirical discourse. Broadly speaking, my enterprise fits within a program of attempting "to transfer to ordinary statements what the intuitionists say about mathematical statements."

The phenomenon that meaning inheres in language may excite wonder, impress us even as a mystery. The explicit insight that such a program gives promise of understanding this phenomenon is due to Dummett (cf. "Truth", p. 109), although arguably this conception tacitly informs the whole of Wittgenstein's work from 1929 onwards (a point alluded to in the final sentence of Dummett's essay op. cit.).

What plausibility accrues to Brouwer's anti-realist conception of truth considered in the domain of mathematics is not immediately transferable to ordinary discourse. The powerful intuitive picture which motivates the realist outlook is most compelling in application to empirical matters, while problematic when applied to mathematics. Direct observation, or at least the simplest account of our immediate sensory experience, reveals
to us the existence of material objects, subsisting on their own, de-
terminately endowed with properties, which we may or may not, as
circumstances allow, succeed in discovering. No such simple look-
and-see experience can suggest to us a realm of independently existent
mathematical objects (e.g. numbers, sets of them, etc.) creating in
their various inter-relations a domain of objective facts, reflected in
language by objectively true or false eternal sentences. To be sure,
arguments are advanced to establish such a platonist conception of
mathematics, and the apparent objectivity of mathematics (in the sense
that there seems to be a right and a wrong to it) may be taken as a
motivation for finding such arguments. But the antinomies did something
to shake uncritical confidence in mathematical objectivity. And even if we
are still inclined to suppose a platonic realm of numbers, there is no simple
and compelling picture, as there is for material objects, of how we
interact with these things, thereby forced to concede some statements true,
others false.

No matter how realist we would be about mathematics, we must concede
that mathematical truths are discovered by individual acts of mind. On
Brouwer's view we lack entirely any justification for supposing there is
more to mathematics than these mental processes. If so, then the
mathematician, by his activity of mind, does not discover, but rather creates
mathematical truths. Such a conception constitutes not only a philosophy
of mathematics, it leads to a new mathematics. Mathematical entities
must be constructed by acts of intuition. All mathematical truth flows
from such construction, and the introspective realization of internal relations
between such constructed entities (proof) is itself an act of intuition. In
platonistically conceived mathematics, logic is the device by which we construct proofs. Classical proofs allow us to infer certain properties of an otherwise inaccessible realm of objects from a few basic truths (axioms) which somehow suggest themselves as evident. The realist conception finds expression in a faith that logical deduction, involving in particular the principle of excluded middle, constitutes an organon for discovering truths. If we take it that every statement is determinately true or false, then we know in advance of any experience that for any statement 'p', the statement 'p or not p' is true. Taking such a statement as the starting point of a deduction, we might then establish the falsity of 'not p', e.g. by showing that its supposition leads to untenable consequences. Having thus shown that 'p' is not false, we are, on the realist conception, in possession of conclusive grounds for asserting that 'p' is true. But clearly we have no direct perception of that truth, and our faith that it is a truth rests entirely on the general conception that every statement is true or false. For the anti-realist, we quite lack grounds for this general assertion. The requirement that we are justified in the assertion of a truth just in case we possess a direct perception of it leads Brouwer to the view that "logic is not a reliable instrument to discover truths and cannot deduce truths which would not be accessible in another way as well" (loc. cit.).

While this is not the place to discuss in detail divergence between classical and intuitionist mathematics, a few remarks will be useful. As we might expect, roughly the divergence is greatest in those domains where the classical theorems are most remote from perceptual experience. The requirement that mathematical entities be generated by acts of intuition
does not produce a structure for arithmetic appreciably different from that pictured on the classical conception. This is not surprising, as the process of counting is a perfectly constructive, humanly performable act. Indeed, if we restrict our attention to finite arithmetic, the classical and intuitionist theories coincide completely. By finite arithmetic is meant what can be expressed in terms of numerals, free variables, the standard arithmetical notions (addition, multiplication, successor and sentential connectives,) but no use of quantifiers (or equivalently, only bounded quantification). For this domain (what is often called primitive recursive arithmetic) the principle \( p \lor \neg p \) is intuitionistically valid, since numerical equations and sentential combinations of them are decidable. Thus, by reflection on particular features of this range of mathematics, the intuitionist comes to accept in this case the principle that every statement is true or false. Skolem explored the mathematical power of quantifier-free arithmetic in his 1923 paper "Begründung der elementaren Arithmetik durch die rekurrierende Denkweise ohne Anwendung scheinbarer Veränderlichen mit unendlichem Ausdehnungsbereich." He assessed this work in a subsequent publication: "individually of Brouwer and without knowing his writings, I set forth similar ideas, confining myself, to be sure, to elementary arithmetic" ("Über die Grundlagendiskussionen in der Mathematik", p. 13). This assessment somewhat misses the point and substance of the intuitionist conception. It is correct that anything demonstrable in Skolem's system is intuitionistically correct, but even for arithmetic intuitionism does not restrict itself to what can be expressed by these quite limited means. Skolem's explicit construction of primitive recursive arithmetic is consonant
with Brouwer's requirement of logic-free direct apprehension of truth.

But decidability has the effect that in this domain the classical (realist) viewpoint is intuitionistically justified, even if the classical mathematician (unlike Skolem) feels no need to justify it. What is characteristic of the anti-realist conception will emerge in the domain of arithmetic only when we get beyond numerical equations and free variable formulae and confront unbounded quantification.

On the realist conception the meaning of statements involving unbounded quantification is explained in terms of the totality of natural numbers. A universally quantified statement '∀x Fx' is true just in case every natural number has the property F. The existentially quantified statement '∃x Fx' is true just in case at least one is an F. That such statements are determinately true or false follows from the point that the property F is determinate in its application to any natural number whatever (it is a fact, which we may or may not discover) and the notion that the natural numbers constitute a determinate and complete totality. From that totality the property F generates another, consisting in answers to the question: 'does F apply to it?', for every natural number. It is determinate what that set consists in; if all the elements in it are 'yes', the corresponding universal quantification is true, and at least one 'yes' somewhere in the lot renders an existential statement true. Sometimes such explanation of quantifier semantics are given in terms of a metaphor of surveying. The set of 'yes-no' answers for a given F is obtained by 'surveying' the totality of natural numbers, setting down the answer in each case. On the realist conception this explanation remains only a metaphor. For Brouwer, and anti-realists
generally, it is much nearer the literal explanation. The requirement that all mathematical entities are humanly constructed through acts of intuition means that we can have no easy recourse to the totality of natural numbers, as occurs in the realist account. The process of counting natural numbers (and so e.g. placing into a collection the numbers which have been counted) is indefinitely extensible - however far we go, we can always go further. But this process will never result in our counting \( \omega \). Nonetheless, this does not have the effect of rendering impossible the expression of unbounded quantifications within intuitionist arithmetic. For we have the possibility of achieving, by an act of intuition, a perception of the applicability of a particular property \( F \) to the natural numbers as individually intuited. This perception does not require that the totality of natural numbers itself be intuited as a single and complete entity. To illustrate, consider a statement derivable in quantifier-free arithmetic and which contains free variables, e.g. '\( x+y = y+x \)' . The derivation constitutes a schema such that whatever two natural numbers we might choose to consider, we can apply the steps of the derivation with respect to them. In this way we come to intuit directly the truth of e.g. \( a+b = b+a \), for particular \( a \) and \( b \). By reflection on the character of the free-variable proof, we realize that it provides for us a uniform procedure whereby a demonstration of the general property for any particular numbers is immediately obtainable. In this way we are justified intuitionistically in asserting the universal closure of the free variable formula so proved, e.g. ' \( \forall x \forall y (x+y = y+x) \)' . So far, of course, we remain within the bounds of Skolem arithmetic. But it can happen that we are intuitionistically justified in asserting the negation of a universally quantified arithmetic statement, e.g. ' \( \neg \forall x (x+x = x) \)' , and such
assertions are not expressible in quantifier-free arithmetic. The statement \( \neg(x + x = x) \) asserts the falsehood \( \forall x \neg(x + x = x) \). We can of course establish such statements within the system, e.g. by \( \neg(1+1=1) \).

The impossibility of expressing negation of universally quantified formulae in Skolem arithmetic resolves a situation which at first glance might have seemed puzzling: the expressibility of e.g. Fermat's last theorem in that system (since it is purely universal) even though the system is decidable. The point of course is that the decidability of Skolem arithmetic comes to nothing more in full arithmetic than \( \forall x_1 \ldots x_n (Fx_1 \ldots x_n \lor \neg Fx_1 \ldots x_n) \) for quantifier-free \( 'F' \). We cannot find either a formula in quantifier-free arithmetic with just the expressive content of an existentially quantified formula, e.g. \( \exists x (x+x=x) \). But intuitionism allows such statements, where the existential quantifier ranges over some species of constructed objects. As might be expected from what has already been said, the intuitionist interpretation of such a statement is in terms of conditions under which we intuit or perceive its truth. For \( \exists x Fx \) that means being able to pick out an object and to demonstrate (by the canons of intuitionist provability, of course) that it has the property \( F \). While no statements of Skolem arithmetic will bear such an interpretation, a purely existential arithmetic statement so interpreted will be assertable just in case there is a corresponding true closed formula of Skolem arithmetic, e.g.

\( \exists x (x+x=x) \) is assertable in virtue of our knowing that for 0 we can demonstrate \( 0+0=0 \). Skolem arithmetic thus provides a reasonably good account of Brouwer's anti-realist conception up through purely universal and existential statements. Where we might expect inadequacies to appear is in the treatment of quantifier alternations. Even here the quantifier-
free system can go some way, for it is equivalent, as earlier remarked, to one in which bounded quantifications are allowed, and many theorems can be recast with that explicit limitation. The prime decomposition theorem (for every natural number there is a unique factorization into primes) can be proved in a free variable system since the search for prime factors for a given number \( n \) is an algorithmic process bounded by the greatest integer not greater than \( \sqrt{n} \). The reduction, as in this case, is itself a bit of mathematics, and not guaranteed to be obtainable for any statement of arithmetic. The twin prime conjecture constitutes an \( \forall \exists \) statement where we presently lack the mathematics to effect the reduction, since we know of no bound on how far beyond a given number we must search to find a pair of consecutive primes. The lack of a reduction is not due to the problem itself being unsolved; the situation could be as with Goldbach's conjecture (every even number is the sum of two primes), where \( n \) provides a simple bound on the representation as a sum. There are arithmetical statements which can be seen to be, even on their intuitionist interpretation, irreducible to Skolem arithmetic. Consider a function which is recursive but not primitive recursive, e.g. the Ackermann generalized exponential. Let \( T_3(z, x_1, x_2, x_3, y) \) and \( U(y) \) be as defined by Kleene in his arithmetization of recursion theory (cf. §56 of Introduction to Metamathematics) and let \( e \) be the Gödel number of a set of Herbrand-Gödel equations which calculates \( f \). There is no way to express in Skolem arithmetic what it is to establish '\( \forall x_1 \forall x_2 \forall x_3 \exists y \exists v(T_3(e, x_1, x_2, x_3, y) \& U(y) = v) \). This result is demonstrated by showing \( f \) not primitive recursive, from which it follows that there is no \textbf{uniform} bound on \( y \) and \( v \) as a function of \( x_1, x_2, x_3 \).
We have noted that realist and intuitionist arithmetic coincide completely for the theory of equations and bounded quantifiers. For unbounded quantification the intuitionist interpretation might appear 'weaker' than that attendant on a realist conception. At least from the realist viewpoint the requirement of perception through direct constructions seems additional to truth. And as standardly formalized in terms of a system of logic and the Peano postulates, intuitionist arithmetic constitutes a subsystem of the classical one (every statement intuitionistically assertable is classically true). Due to results of Glivenko and Gödel, however, there is a sense in which the containment relation goes the other way. There are ways of translating classical theorems such that anything the realist-minded mathematician feels justified in asserting is intuitionistically correct as well. These translations, e.g. \( \neg \forall x \neg F(x) \) in place of \( \exists x F(x) \), are acceptable from the realist point of view. The conclusion sometimes drawn from these results is that classical mathematics has not succeeded, through its utilization of realist logic and the Peano postulates, in exploiting the full power of its realist conception of the infinite totality of natural numbers. In any case, nothing is being claimed in classical arithmetic which the intuitionist cannot accommodate by suitable reinterpretation.

The divergences become rather more irreconcilable when we come to analysis. Crucial differences emerge already before quantification is the issue. On the classical conception, an individual real number constitutes a determinate, completed infinite structure. Giving up this picture might seem to preclude a theory of the continuum, for if we require infinite decimal expansions to proceed according to a pre-determinate rule (as e.g. all rationals, algebraic irrationals, and some transcendental, notably
e and π) there can be only denumerably many reals. While one might manage on this basis enough calculus of functions to serve experimental science (all measurements, after all, are rational numbers), one would in no way do justice to the intuitive notion of continuity so fundamental to our concept of analysis. Brouwer had the insight that a much less restrictive and mathematically richer notion of real number is consonant with the requirement that all mathematical entities be constructed through humanly performable acts of intuition. A real number may be taken as an infinitely proceeding sequence of 0s and 1s. We may allow cases where the nth value of the sequence is not determined by rule in advance; for it is a humanly performable act to choose a 0 or a 1 at each step. What the intuitionist conception dictates is just that no property of the number generated (either by choices or in accord with a specified rule) turn on the supposition that the generation process be completed. From this condition follows the so called "Fan Theorem", according to which for any function \( \mathcal{G} \) of a real variable we can compute from its definition a natural number \( N \) such that the value of \( \mathcal{G} \) at a real number \( r \) is determined from the first \( N \) elements of \( r \), i.e. if \( r_1 \) and \( r_2 \) agree in their first \( N \) places then \( \mathcal{G}(r_1) = \mathcal{G}(r_2) \). In consequence of the fan theorem, any real-valued function which is defined everywhere on a closed interval of the continuum is uniformly continuous on that interval. This statement is false in classical mathematics, but any realist-minded argument to refute it will violate the fan theorem, and so trade upon taking arbitrary real numbers as determinate, completed entities. Consider such 'obvious' counter-example as a step function. With \( r_\circ \) the point at which the function 'jumps up', there is for any \( n \) an \( r_n \) such that \( r_\circ \) and \( r_n \) agree in the first \( n \) places but differ in value;
no matter how large \( n \), we may obtain such an \( r_n \) by getting sufficiently close to \( r_\omega \). This talk of 'we' and 'getting close' is common enough in the exposition of classical mathematics, but the use must be metaphorical - 'we' could never construct such a function in the first place. It might be supposed that by taking \( r_0 \), the point of discontinuity, as particularly well behaved, for example, an integer, such a function could be constructed. In this case we should then know where the function changed value; elsewhere the function is completely constructive since it is a constant function.

But there is the rub. Which constant function is it for any given (on the literal intuitionist sense) \( r \) in the interval of definition? For an infinitely proceeding sequence it is not in general decidable whether it generates a real which is identical with, less than or greater than a given real. The discontinuity of the step function means that, simple to the point of triviality though it appears, such a putative function is not calculable.

The fan theorem and uniform continuity are points of substantial disagreement between classical and intuitionist mathematician. These theorems involve quantifiers, but the disagreement has already to do with the most basic properties of real numbers. Characteristic of this difference in conception is the point just appealed to, that order and equality on the continuum are not decidable. Accordingly, the intuitionist will take as entirely unwarranted the general assertion ' \( \forall x \forall y (x = y \lor x < y) \)' , where \( x \) and \( y \) range over the reals, and there will be instances for specific real numbers \( r_1, r_2 \) such that ' \( r_1 = r_2 \lor (r_1 < r_2) \)' lacks justification.

With this excursus on the content of intuitionist mathematics concluded, I want now to reflect on this sketchy account in order to characterize more exactly than in previous remarks the contrast between
realism and anti-realism. These doctrines were initially characterized as opposed conceptions of truth. The issue was said to concern the objectivity of fact. Yet it may appear from the foregoing account that the point in contention between realist and anti-realist mathematician concerns rather the existence and nature of objects: do natural numbers exist apart from humanly performable acts of counting?, does the totality of natural numbers exist as a completed object?, do the points of the continuum exist apart from our constructions?, are there discontinuous functions (e.g. the step functions)? It may be allowed that while there is a conception of truth which I have characterized as 'realism', this conception is better considered as a reflection of ontological doctrines. In a certain way such a description is correct, but to conceive matters in these terms misleadingly characterizes the dispute between realist and anti-realist. From the anti-realist point of view the mistake which the realist commits comes not in affirming the existence of certain entities, but in supposing that he can frame a conception of 'the way things are' which is quite independent of human capacities for perception. The anti-realist account of truth turns on inquiring after how we obtain a conception of the way things are. And the answer must be that we manage this precisely in grasping the meaning of the language by which we describe situations. So the issue comes down to how language is endowed with meaning.

This turn in the discussion may appear to introduce an asymmetry between the realist and anti-realist conceptions of truth. But it must be the claim of the anti-realist that this seeming asymmetry is illusory, and if insisted upon, a serious philosophical mistake. The realist notion that truth is determinate, however limited human access to it might be,
seemed to follow from the intuitively plausible and modest point that many objects exist quite independently of human affairs. Whatever such an object is, its various properties and relations to other objects are part and parcel of its existence (this conception, of course, applies to all properties and has nothing to do with essential attributes). The sentences which attribute these properties and describe its relations are true. So the independent existence of objects carries in its train the determinate, realist character of truth. Where is the mistake in such an argument? The objection is not that it trades upon some formal fallacy. But it is objected that the major and minor premises fail to be given adequate justification. And a fully explicit justification would invoke a conception of language which the anti-realist disputes. The mistake which misleads consists not so much in this conception of language (though it is to be rejected) as in the failure to perceive that the issue turns on it. To reduce the problem of how we conceive 'the way things are' to there being things is merely to invite the questions of how we conceive there being things. Of course one answer might be in terms of our immediate experience of objects. To the extent that such an answer is tenable, it is consonant with the constructivist tenor of anti-realism. But the realist conception of truth is meant to transcend just such a limitation. To suppose that we can make sense of objects inaccessible to us is to suppose that language can serve us in our attempt to assert that there are, or could be, such objects. And this is to suppose that language is endowed with sense in such a way that we can use a sentence quite independently of any human capacity to determine that the situation is as described, or capacity to reorganize experience which would contribute to such a determination.
The minor premise of the realist argument trades essentially on this conception as well. The notion of 'the properties of an object', even for an object explicitly constructed, such as a natural number, itself requires an account. To the extent any such account would validate the use to which the realist would put this notion, the previously noted conception of meaning will need to be invoked. Consider by way of illustration the Gödel number (on some arithmetization of recursive theory) of a particular set of defining equations for the Ackermann generalized exponential. This is a particular natural number (call it 'e') which I can easily construct and exhibit (most readily and perspicuously in terms of its prime decomposition). Does it have the property that

\[ \forall x_1 \forall x_2 \forall x_3 \exists z \exists y \left( U(z) = y \text{ and } y \geq 3 \right) \]

The realist supposes that on our conception of an object, we conceive that all its properties inhere in it, though many of those properties we do not conceive of at all. All other properties are characteristic in failing to apply to the given object. An object in this way creates a dichotomous classification of all properties. So it is that for any property we may happen to conceive of (e.g. is the Gödel number of the Ackermann generalized exponential, under a certain arithmetization of recursion theory), this property is on one or the other side of the dichotomy, though we may lack any conception of how to decide which. The attribution of a given property to a given object is thus true or false. Frege expresses this conception in §56 of Grundgesetze der Arithmetik, Vol. ii:

A definition of a concept (of a possible predicate) must be complete; it must unambiguously determine, as regards any object, whether or not it falls under the concept (whether or not the predicate is truly assertible of it). Thus there must not be any object as
regards which the definition leaves in doubt whether it falls under the concept; though for us men, with our defective knowledge, the question may not always be decidable. ... The law of excluded middle is really just another form of the requirement that the concept should have a sharp boundary. Any object $\Delta$ that you choose to take either falls under the concept $\varphi$ or does not fall under it; *tertium non datur*.

No one, of course, would wish to dispute that to raise the issue of whether the number $e$ has a certain property, we must understand the concepts involved, e.g. for the case mentioned above, know the meaning of the complex predicate $T_3$ and the function $U$ and comprehend statements formed by application of existential and universal quantifiers. But what is at issue is how we come to understand the concepts. Frege's discussion of "definition of a concept" in the passage just quoted has to do with conditions on what it is for linguistic expressions to be endowed with meaning. What the anti-realist will find puzzling in Frege's account is that while for "us", "with our defective knowledge, the question [whether a given object falls under a particular defined concept] may not always be decidable", nonetheless (our) definition manages to "determine as regards any object whether or not it falls under the concept." How can a definition, which we ourselves frame, take on such independence as to possess capacities quite in excess of our own? In a certain way, of course, this is a silly, or unfair question, for the definition is not 'doing' anything. There is some sort of immutable congruence between it and a non-linguistic entity, the property it defines. Still, there is a difficulty then in allowing that we frame the definition, or grasp the meaning of an expression. How is it we know what concept we have defined or understood when the very notion of concept is in terms of excluded middle, and human capacities are such
as often to find us caught out between attribution and denial? And if we do not know what concept it is, then surely we have failed in our act of definition or understanding.

There is one clear situation in which defective knowledge and/or other human infirmities puts us in an unfavorable position when compared with what is determined by a definition. Such a case is illustrated by the concept of prime number, defined in terms of checking the result of dividing \( n \) by each prime not greater than \( \sqrt{n} \). For very large \( n \) we may lack the patience, paper or time to perform the algorithm; nonetheless the concept remains determinate in its applicability or non-applicability to \( n \) through the 'in-principle' possibility of determination. But clearly Frege had some much more radical cleavage in mind, for not every concept admits such in-principle human determination, and Frege demands conformity with excluded middle of every concept we suppose defined.

The example complex predicate of earlier put forward for consideration contains elements where this divergence will not be apparent, namely in the predicates \( T_3 \) and \( U(z) = v \), which are both primitive recursive and so expressible in terms just of the basic arithmetic operations and bounded quantifiers. With the unbounded quantifications which govern the free variables of these expressions we come to the important difference of conception. The contrasting interpretation of the quantifiers in classical and intuitionist mathematics has already been sketched. The classical view in that sketch seemed rather to be conceived as a reflection of ontology; I want now to reconsider the contrast in line with my current point that any use of language must admit of an account of how we come to grasp its meaning.
Suppose we have a decidable two place arithmetical relation $\psi(x, y)$. Then for any particular natural number $n$ we may determine whether $\psi(17, n)$. How do I understand the property of 17 that the answer to any such question, for arbitrary $n$, is 'yes'? For the intuitionist it consists in envisaging the possibility (without, of course, any requirement that it be actual) that $\tau$ should succeed in attaching to each procedure by which I generate a given natural number $n$ a positive determination that $\psi(17, n)$. For the realist the answer will leave me entirely out of account (except for the disingenuous (?) use of metaphor): ' $\forall x \psi(17, x)'$ means that every natural number bears the relation $\psi$ to 17. But how can I envisage a situation being a certain way if I am not in effect envisaging determining or perceiving it to be that way? The realist reply just given does not seem to be even a prima facie answer to this question. If the realist takes such a point seriously he is apt to reply with an appeal to analogy: I know from experience what it is e.g. for every number less than 48 to bear the relation $\psi$ to 17. So I can envisage what it is for every element in some collection of numbers to have this property. Well, the collection might just happen to be all the natural numbers. It is possible to hear this explanation as not entirely unlike what the intuitionist is saying. The explanations are compatible if my grasp is relevantly similar to that for finite sets. But with that sense of similarity as yet unexplained, the account is thereby incomplete.

Lurking in the background of such a realist explanation is the principle of excluded middle, and it might be thought employable at this juncture to explain the notion of similarity between finite and infinite sets being a certain way. The decidability of sentences involving only finite collections
of accessible objects acquaints us directly with excluded middle. In these cases, as it happens, we do not have to rely on the principle, as direct perception reveals each statement to be determinately true or false. But when we get beyond such domains of discourse, excluded middle comes into its own as a feature characteristic of truth. For what previously we could justify for ourselves, we now can rely on logic to provide the grounds. Evidently this view is but a slight transposition of the previous account from analogy, though different enough not to allow an anti-realistically acceptable understanding of it. The humanly determinable and indeterminable case are said to be 'like' in both instancing excluded middle. But this move brings us no nearer to an appreciation of how statements inaccessible to immediate experience are nonetheless true or false.

As remarked, the contrast between classical and intuitionist mathematics is most pronounced and characteristic in the development of analysis. I want now to consider a particular realist counter-attack on the intuitionist theory of functions of a real variable, as a means to illuminating the general issues debated in the preceding pages. This discussion will concern step functions, declared not well-defined on the basis of Brouwer's uniform continuity theorem. On certain uses of language, and with certain pictures in mind, step functions must strike us as simple, well-behaved, entirely unproblematic. It is easy to pick out particular ones by description, e.g. the function defined on the closed interval of the reals \([0, 2]\) which takes the value 1 for argument from 0 up to but not including 1, and takes the value 2 from 1 to 2. We picture the function as having this graph:
Various assertions about this function seem evidently true, e.g. it has a derivative with a constant value 0, except at 1, where the derivative fails to be defined. The intuitionist, of course, does not deny the existence of pictures of the sort drawn above - but he does deny that this is a picture of a real valued function of a real variable. To suppose that it is is to take as literal what is only analogy or metaphor. We can correlate a point on the page incident on one line (e.g. the x-axis) with a point on another line (the graph of \( f(x) \)) by intersecting that line with a straight edge through the original point and parallel to the f(x)-axis. But we seriously delude ourselves if we suppose this operation to explain what it is for a real valued function of a real variable to 'take a value for given argument.' No one does actually suppose his words given sense in this way, but there is, the anti-realist will claim, tacit appeal to such a picture in the confidence that we know what we are talking about when we pick out a particular step function by description, and then go on to predicate truly of it. The notion of 'taking' some particular arbitrary real as argument is rendered intelligible by the image of a pin prick on a line on the page; correlating one pin prick with another is a perfectly intelligible operation, and it stands proxy to 'determining the value of the function at a given argument.'

For classical and intuitionist mathematics alike, real numbers are
officially understood as convergent sequences of rationals, or equivalently, as infinitely proceeding sequences of digits. The intuitionist takes this conception seriously, insofar as every further attribute of reals is to be understood in terms of it. The reals have ordering properties. For two convergent sequences of rationals, i.e. reals, a and b, \(a < b\) just in case there are natural numbers \(N\) and \(k\) such that, for \(n > N\), \(b_n - a_n > \frac{1}{k}\) (where \(a_n\) is the \(n\)th term of the sequence \(a\)). Infinitely proceeding sequences cannot be surveyed, being infinite structures, so it is not in general decidable whether \(a = b\) or \(a < b\) or \(b < a\). This result is not some peculiar feature of the intuitionist notion of choice sequence; the point applies already with completely lawlike reals. For example, the values of \(\pi\) are generated by an algorithm; we may define another algorithm \(\pi'\) which is like \(\pi\) except that if we come to the first string of ten consecutive fives in the calculation of \(\pi\), the values for \(\pi'\) at those points is 4. How do we form the conception, as the realist does, that whether we can in a given case make the determination or not, any two infinitely proceeding sequences (or convergent sequences of rationals) either are (or define) the same real, or one is less than the other? The picture of the line does much for such conception - it is perceptibly determinate whether we prick the line twice in the same place, or in two different places, and if the latter case, which point is leftmost. The issue then is how real numbers are to be identified with points on the line. A choice of unit length and simple geometric constructions give us an identification between rationals and linear points. Continuity is a feature bound up in our conception of a line, so that if a sequence of points of the line are clustering arbitrarily closely, we suppose them
to cluster about a point rather than a gap. So every convergent sequence of rationals is identifiable with a point on the line. And conversely, an arbitrary point can be squeezed between rationals by first dividing in half the integral unit in which it lies, and then halving the half of the unit in which the point occurs, and so on.

An intuitionist reply to this argument begins with a point which the realist readily concedes. The notion of straight line must represent an idealization, abstracted from collocations of graphite adhering to surfaces of paper, edges of precision machined blocks of metal, or whatever. No physical point will constitute a particular real number, covering as it will a whole neighborhood of the line. And the atomicity of matter, to say nothing of quantum effects, preclude continuous lines in matter. Even the notion of a ray of light constitutes an abstraction from the stream of protons in which it consists. The realist fails to draw the consequence of these evident points. Insofar as we would use the notion of line to represent the continuum of reals, that notion must come from our prior theory of real numbers, rather than from our experience of physical lines. What enables us to formulate the suitably abstract and idealized conception of line is our notion of a real number. The argument of the previous paragraph to justify the identification of points of a line with real numbers is intuitionistically acceptable – in particular the construction in which a given point is converged upon by successively halving rational intervals is precisely the much utilized theory of canonical real number generators. What the argument may be taken to establish is that given the theory of reals, an account of the distance between two arbitrary points is forthcoming from the notion of unit distance. It is quite unjustified to conclude from any such
an argument that order between real numbers is determinate because we can visualize them as points on a line.

The realist conception trades quite generally and explicitly on the suggestiveness of pictures. It is somehow thought that to concede they are merely pictures leaves the conception intact. Justification for this attitude of unconcern itself turns on the utilization of a picture, for it is somehow supposed that there are properties of entities about (e.g. of real numbers) and suggestive pictures suggest them. So even if the picture fails to convey determinate notions if taken literally, it enables us to grasp through analogical leap a notion not directly accessible in terms of immediate human experience, but nonetheless there to be grasped. The encompassing picture justifying this conception is of objects as points in a plane and properties or concepts as regions of the plane. Whenever we draw a closed curve on a piece of paper we thereby mark out a region of the sheet. We succeed as long as the curve closes - we do not need to know what region it is we are enclosing to succeed in doing so. The regions are there in virtue of the paper being a plane surface, and the points belong just to the regions which cover that point and no other. "There must not be any object as regards which the definition leaves in doubt whether it falls under the concept...We may express this metaphorically as follows: the concept must have a sharp boundary. If we represent concepts in extension by areas on a plane, this is admittedly a picture that may be used only with caution, but here it can do us good service." (Frege, Grundgesetze der Arithmetik, Vol. ii, §56). Wittgenstein, in the Tractatus, follows Frege in employing this metaphor; "An analogy to illustrate the concept of truth: imagine a black spot on white paper, you can describe the shape
of the spot by saying, for each point on the sheet, whether it is black or white. To the fact that a point is black there corresponds a positive fact, and to the fact that a point is white (not black), a negative fact." (4.063). It seems to me this image has been used with insufficient caution, that it provides the ostensible justification of the realist conception of truth, and that this conception is not consonant with considering properly the question of how language is for us endowed with meaning. When that question is properly considered, anti-realist truth follows.

Quite some way back, when first introducing the discussion of Brouwer, I remarked that his anti-realist conception might be thought at its most plausible when applied to mathematical discourse, and less so as concerns empirical matters. The point was a reflection on the realist construal of the issue of objectivity of truth in terms of ontology: statements about subsistent entities are determinately true or false. And it seemed that common sense would allow denying subsistence to mathematical entities while balking at the same as regards physical objects. I have three comments to offer in this regard. The first is to note that in the course of the preceding discussion grounds have been adduced for taking the issue about truth to be not a reflection of ontology but an issue about the nature of meaning. The second point is that at least until recent times all informed philosophical opinion was subservient to some form or another of phenomenalism. This observation has little to do with what might prove acceptable to common sense (the terms in which I expressed the issue of plausibility), but does suggest that the constructivism of Brouwer's anti-realist conception is not new to philosophy. Berkeley's dictum "esse est percipi", and Mill's notion of physical objects as "permanent possibilities of sensation"
seem not uncongenial with Brouwer's notion of entities constructed by acts of human intuition. But while these earlier conceptions may be congenial with anti-realism as here characterized, they are not properly considered precursors of it. The concerns which lead to phenomenal views were almost exclusively to do with epistemology. The notion that these investigations had anything to do with the concept of truth seems not to have occurred at all. And that they might be bound up with issues to do with meaning was realized only dimly, if at all. The dim realization concerned the connection of meaning of a term or predicate with the conditions for its application. No conception of meaning as a systematic phenomenon was manifested in this "impossible term-by-term empiricism of Locke and Hume" (Quine, "Two Dogmas", p. 42). Frege first realized the myopia of this limited notion of meaning, and declared (§62 of Die Grundlagen der Arithmetik) "it is only in the context of a proposition that words have any meaning." Quine applauds Frege's insight, but urges that "even in taking the statement as unit we have drawn the grid too finely" (loc. cit.). Quine's holism, in which language is taken as unit of meaning, is not so far removed, as I shall argue in Chapter 4, from Brouwer's conception of meaning and truth. My third comment as to the applicability of Brouwer's conception to empirical as well as mathematical discourse concerns his own view of the matter. While the main concern of his published papers is with mathematics, there are enough comments to indicate he considered the conception applicable in wider domains. The concluding line of "Consciousness, Philosophy and Mathematics" speaks of research in foundations of mathematics as having "revealing and liberating consequences, also in non-mathematical domains of thought." Brouwer
does allow "the practical validity of the whole of classical logic for an extensive group of simple every day phenomena" (op. cit., p. 1247). This will include experience of immediately perceptible objects. He takes this fact to provide in part a genetic explanation for the widespread impression that classical logic is universally applicable and a priori in character. The following passage from "On the Significance of the Principle of Excluded Middle in Mathematics, especially in Function Theory" (p. 336) expresses this view. He also there explicitly maintains the limited applicability of logic in empirical domains, and particularly cites the problem of statements about the past. (I will discuss the meaning of past tense statements in the course of my considerations on verificationism.):

...Numerous objects and mechanisms of the world of perception, can be mastered if we think of them as (possibly partly unknown) finite discrete systems that for specific known parts are bound by specific laws of temporal concatenation. Hence the laws of theoretical logic, including the principle of excluded middle, are applicable to these objects and mechanisms in relation to the respective complexes of facts and events, even though here a complete empirical corroboration of the inferences drawn is usually materially excluded a priori, and there cannot be any question of even partial corroboration in the case of (juridical and other) inferences about the past.

He goes on in this passage to set out as a condition of correct applicability of classical logic to certain discourse "the projection of a finite discrete system upon the objects in question." And he supposes that "our partial ignorance of the representing finite systems" contributed to the view that classical logic is correct a priori. I shall argue, particularly in the final section of Chapter 4, that Brouwer's conception, and in particular intuitionist logic, is apposite to a thorough-going empiricist conception of language.
CHAPTER 2

VERIFICATIONISM

§1. Introduction

Verificationism and the logical positivists who made it their chosen doctrine and weapon have been so attacked, left for dead and vilified into the bargain that I should be pleased were it possible to announce as the aim of this chapter not the burying of verificationism but its praise. And so to a degree I can. For it is one goal of this thesis to explore and develop an anti-realist philosophy of language, to a point at which assessment of feasibility and recognition of the important issues begins to be possible. Brouwer's mathematical intuitionism provides an analogical model for this enterprise - but it must be only analogy when we attempt to deal with truth and meaning within the domain of empirical discourse. The positivists' doctrine of verificationism and their development of it constitutes a constructivist semantics of at least broadly anti-realist character and intended to encompass the range of empirical language use. So to the extent that anti-realism is praiseworthy, which I hope to show is considerable, verificationism may be praised as precursor, if not exemplar. And examplar it is not. Therein consists a serious, indeed possibly fatal flaw. For with the apparatus in hand by which realist and anti-realist approaches to language may be distinguished as issuing from a conception of truth, we will see that verificationism is an unwitting but quite explicit attempt to be both realist and anti-realist at once. And far from showing
thereby that the two conceptions are compatible, perhaps even consisting of no more than variation in terminology, the elements of both draw together in palpable contradiction, demonstrating the impossibility of verificationism as its originators conceived it. This particular contradiction may be used as an argument to the effect that in espousing verificationism the logical positivists ought to have been anti-realist, as we are understanding this term. And so it seems they should have. But many of the other difficulties which disrupted and discredited their program would remain, though in at least some cases transposed to an analogous but also possibly more tractable problem. I have in mind here such points as the understanding attaching to statements concerning the remote past, the treatment of counterfactual conditions, and the status of the principle of verification. While these difficulties are not matters for which positivism deserves praise, they do put us in mind of points which must be dealt with in any attempt to construct an anti-realist account of meaning in empirical discourse, and so prove instructive to the enterprise of this thesis. Whether also destructive we shall have carefully to consider.

§2. The Janus-faced character of verificationism

We proceed now to consideration as to the plausibility of construing verificationism as a form of anti-realist semantics. Initially, let us take the doctrine as expressed in the slogan often repeated by adherents, 1

1. For this exact form of words see: Moritz Schlick, "Meaning and Verification", and also Hans Reichenbach, Experience and Prediction, p. 49.
commentators and detractors: "The meaning of a proposition is the method of its verification." We recall (from Chapter 1) that anti-realist semantics consists in the analysis of meaning in terms of truth conditions (a trait shared alike by realist semantics) where truth must be delimited in terms of speakers' capacity for recognizing it (a conception sharply at variance with that of the realist). In these skeletal terms, without enquiring too closely into what is meant by 'verification' or 'capacity for recognizing', the principle of verification constitutes a formulation of anti-realism. As it happens, however, this convergence is accidental, for while the logical positivists (essentially following Wittgenstein in the Tractatus) adhered to the first stage of analysis, which identifies (declarative) sentence meaning with truth conditions, their conception of truth was a thoroughly classical, realist one. In any case, it came to be thoroughly classical, a point clear from Carnap's remark in "Truth and Confirmation" (adapted from "Wahrheit und Bewährung": "The neglect of the distinction between truth and knowledge of truth (verification, confirmation) is widespread and has led to serious confusions", (p. 120). What Carnap here warns his readers against is a conception somewhat akin to our notion of anti-realism, at least in the feature that it entails an alteration in underlying logic, and in particular abandonment of tertium non datur: "...the term 'true' was used in the sense of the entirely different concept 'confirmed'. But this leads to considerable deviations from the common usage of language. Thus one would find it necessary to abandon, e.g. the principle of the excluded middle. The principle maintains for every statement that either it or its negation is true. But as to the vast majority of statements neither they nor their
negations are confirmed or scientifically accepted." (p. 119) Carnap locates the motive for this regrettable deviation in the despair occasioned by the semantic paradoxes, as e.g. The Liar, at employing the concept of truth consistently (loc. cit.). Carnap's own motivation for resisting, in face of the paradoxes, abandonment of truth in favor of confirmation with attendant alteration of logic consists in fidelity to common usage of language, as he indicates in the passage previously quoted. And for Carnap the vindication of that stand is in Tarski's development of formal semantics, for there Tarski: "... succeeded in establishing an unobjectionable definition of truth which explicates adequately the meaning of this word in common language" (subject of course to certain restrictions to handle the contradictions). Carnap concludes: "Hence the word 'true' should properly no longer be used in the sense of 'confirmed' ".

What is the force of this line of argument as an attack on the anti-realist conception of truth? Further, is it actually anti-realism which is attacked? The (negative) motivation considered by Carnap is not the path to anti-realist truth sketched in Chapter 1, but of course there can be a number of routes to the same position. Particularly if the realist/anti-realist classification is even roughly dichotomous, disenchantment (for whatever reason - syntactic, or more deeply based as by reflection on the nature of sentence meaning) with the classical conception of truth might well be the rationale for embracing an anti-realist one. Analogous considerations apply in respect of mathematical intuitionism: there is support for the position on its own merits based on a demonstration of the power and richness of a particular understanding of mathematical language, but there is also the polemical point that classical mathematics
in search of 'foundations' had got itself into the morass of paradox. But in any case, Carnap's characterization of the position as a rejection of the (classical) conception of truth which entails a revision of underlying logic, including abandonment of excluded middle, is strikingly close to the crucial effects of anti-realism, and is enough, I think, to identify his target as such. What then of Carnap's motive for resisting that conception? He wishes to respect 'common usage of language', which according to him has it that every statement or its negation is true. The observation itself as to usage may well impress for its accuracy. And the second premise, that "for the vast majority of statements, neither they nor their negations are confirmed or scientifically accepted" seems unexceptionable. From which then the conclusion follows: (if we are to respect common usage), then 'true' cannot be understood as 'confirmed'. The anti-realist seems then to be left in the dilemma of accommodating the conception which gives rise to that usage, or riding rough shod over it (positivists themselves have been known to course over the vagaries of ordinary language when elucidation seemed the prize). Dummett, in the concluding lines of his essay "The Reality of the Past", describes gracefully something of this latter approach: "Of course, like everyone else, I feel a strong undertow towards the realist view: but, then, there are certain errors of thought to which the human mind seems naturally prone." At this juncture, however, I think we are not forced quite to the expedient of demeaning our pre-philosophical tendencies; there is a way of both having our anti-realism and accounting for the bivalence of truth reflected in common usage of language. The second step of Carnap's argument constitutes recognition that if 'true' is identified with
'confirmed', alteration in logic will result. As we have already discussed, one difference will be the failure of logical equivalence between a statement and its double negation (though not strictly the same phenomenon as failure of excluded middle, it is closely related. In particular, both principles are direct reflections of bivalence, though statements of the form \( A \rightarrow \neg A \) are not necessarily equivalent to \( \neg A \rightarrow \neg (A \neg A) \).) And indeed while there are unassertable instances of \( A \rightarrow \neg A \) of just the sort Carnap cites in his argument, \( \neg \neg (A \rightarrow \neg A) \) is always assertable, i.e. logically valid. On the anti-realist interpretation which validates \( \neg \neg (A \rightarrow \neg A) \) while \( A \rightarrow \neg A \) can fail, it bears the following interpretation: (*) Every statement or its negation is open to confirmation. Or less colloquially but more directly from the formula: it is absurd that we should never be able to establish a statement or establish its negation. Now this principle is of general interest in the context of this chapter because it seems plausibly construed as an expression of the principle of verification. As such of course it would command the assent of Carnap. And its validity on the anti-realist conception constitutes a demonstration of the proper connection between anti-realism and the principle of verification. Of this more elsewhere. Relevantly to the present argument, I want to utilize this principle by way of accounting (from the vantage point of anti-realism, according to which excluded middle fails as a principle logic) for the admitted phenomenon that on ordinary usage every statement or its negation is true. The point is that what in the popular imagination passes for the principle of excluded middle is adequately served by (*). Or to take it from a slightly different angle, the validity of (*) accounts for (or explains) why it is generally supposed
that every statement or its negation is true. Admittedly by such account we seem to impute a widespread confusion, or in any case failure to distinguish clearly between 'is confirmed' and 'is confirmable'. But more plausibly than attributing to common usage a philosophical mistake, we might attribute the lack of a philosophical theory. So we do not suppose the man on the street imbued with any particular view as to the proper analysis of truth in terms of confirmation. But at the same time it does not seem to me implausible that one who affirms that every statement or its negation is true would accept the suggestion, if put to him, that it comes to the same thing to say: every statement or its negation is confirmable. At least if he would balk at such he must be supposing that there are statements which are true, but not open to confirmation. But this in any case is not a situation which Carnap in his capacity as verificationist can well countenance. What then if it is not an ostensible adherent of verificationism who advances the argument now under consideration? We are then forced, I think, to allow that it might go through, that is to say, we could not disabuse its adherent of it without first getting him to give up background principles. To extract the lesson of this argument, we see that a minimal condition on acceptability of anti-realism is the principle of confirmability (*).

Carnap does not present these considerations from common usage as decisive arguments against the identification of 'true' with 'confirmed', and I certainly do not myself hold any view to the effect that philosophical theory must do justice to, or account for pre-philosophical linguistic behavior. Nonetheless, I should feel considerable discomfort in
advancing a semantic theory for a language which attributed sentence meaning systematically at variance with that of native speakers - hence the importance I attach to this argument of Carnap's, and the interest in determining a minimal condition on acceptance of an anti-realist semantics.

I want now to consider the argument in the passage quoted from "Truth and Confirmation" which Carnap does take as establishing that "the term 'true' should properly no longer be used in the sense of 'confirmed' ", a result made welcome, but not in his view dictated by his considerations as to common usage we have lately explored. That argument consists essentially in citing Tarski's analysis of truth. I think it entirely unjustified to draw from that analysis the conclusion Carnap does.

Before coming on to my demonstration of this, let me comment on a feature of Carnap's assessment of Tarski's accomplishment, which if not one crucial to the argument now to be considered, seems curious. I mean his claim that Tarski's definition of truth "explicates adequately the meaning of this word in common language" (my emphasis). The first section of Tarski's "Wahrheitsbegriff" entitled "the concept of true sentence in everyday or colloquial language" concludes with the following (italicized) remark:

The very possibility of a consistent use of the expression 'true sentence' which is in harmony with the laws of logic and the spirit of everyday language seems to be very questionable, and consequently the same doubt attaches to the possibility of constructing a correct definition of this expression. (p. 165)

And indeed Carnap himself in his paper "Meaning Postulates" remarks:
Our explication ... will refer to semantical language systems, not to natural languages. It shares this character with most of the explications of philosophically important concepts given in modern logic, e.g. Tarski's explication of truth. It seems to me that the problems of explicating concepts of this kind for natural languages are of an entirely different nature.

(Meaning and Necessity, p. 223)

One can offer the explanation as to why Carnap would have wanted to see Tarski's analysis as explicating the common conception of truth in the context of the passage cited, namely that he has begun his considerations by appealing to common usage adherence to the principle of excluded middle; to have his appeal to Tarski dovetail with those remarks requires that Tarski's work be applicable to common usage. But even if the whole argument were recast e.g. with the considerations as to the status of excluded middle put in terms of systems for the rational reconstruction of science or the like, the appeal to Tarski cannot achieve what Carnap would have it, for Tarskian theories are neutral as between a realist notion of truth and anti-realist ones such as confirmed, justifiably assertable, or whatever.

Consideration to this effect focuses on Tarski's quite unexceptionable condition of adequacy for any theory of truth, dubbed by him 'convention T' (Logic, Semantics, Metamathematics, p. 187). To set it out something needs first to be said as to the distinction between object language and meta-language, and the syntactic character of truth in the context of formalized languages. So let me here rehearse as briefly as I can an excessively familiar story. Whatever difficulties (due to context dependence, ambiguity and the like) there may be in the case of natural languages to taking truth
as a predicate of sentences, there aren't such for formalized languages. Predication of sentences requires of course that we be able to refer to the sentence (as an object) in distinction to using it (to make a statement). The device of reference is naming and so we shall require in our analysis of truth a supply of names applicable to all the sentences of the language to which that analysis is meant to be applicable. Guaranteeing that supply is not quite a trivial problem: roughly it is solved already by the ordinary language device of quotation; with more promise of a systematic theory there is the technique of describing the concatenation process by which the sentences are strung together from basic symbols, and closely related is the device of Gödel numbers (due independently to Tarski - who failed to realize its tremendous potential for metamathematical investigation, \(^1\) which Gödel did). The precise character of sentence names need not detain us; chiefly what is required for this discussion is that truth has the syntactic character of a one place predicate \(T(x)\), where the variable place may be filled by sentence names.

Now the crucial characteristic of any candidate purporting to be a truth predicate must be that (★): it applies to a given sentence just in case things are as that sentence asserts them to be. There is, I take it, something incontestably correct in this observation; indeed, advocates of the so-called redundancy theory of truth might be construed as maintaining that this is all which can be said by way of general theorizing about truth. Still, there is something quite unsatisfactory about it; the explicans must strike us as highly artificial for it succeeds in being a sentence only by the artifice of

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1. Gödel numbers and concatenation names differ as to important theoretical properties - cf. Niemi, "On the Existence of a Modal Antinomy".
employing 'things' as grammatical subjects in a context which renders the
reference of that all too general term entirely obscure. (★) is an attempt
to characterize in a single sentence truth as it applies to (the infinity of)
all sentences. If instead we content ourselves with the attempt to express
the insight inchoate in (★) as it applies to a single sentence the result is
quite unproblematic:

( * ) T ('Snow is white') if and only if (just in case) snow is white.

In each particular case the condition of 'things being as that sentence asserts
them to be' is simply expressed by the sentence itself. The infinite totality
of all such statements, one for each sentence constitutes the expression of
(★) in unproblematic terms. This infinity of sentences is generated by
substitution in the schema:

(T) X is true if and only if p

where in place of 'X' goes the name of a sentence, and in place of p the
sentence itself. This is Tarski's Convention T:

Now at last we are able to put into a precise form the conditions
under which we will consider the usage and the definition of the term
"true" as adequate from the material point of view: we wish to use
the term "true" in such a way that all equivalences of the form (T)
can be asserted, and we shall call a definition of truth "adequate" if
all these equivalences follow from it.

"The Semantic Conception of Truth" (p. 55) (Tarski's emphasis)

There is a further wrinkle to this story occasioned by the need to avoid the
pitfalls of inveterate liars. Formalizing the liar paradox demonstrates
directly that no consistent theory can be framed in a given language $\mathcal{L}$ which
yields as deductive consequences every instance of convention T as p ranges
over all the sentences of $\mathcal{L}$. Tarski's way out is to frame the theory of truth for a given language $\mathcal{L}$ in a semantically richer language $\mathcal{L}'$, a meta-language for $\mathcal{L}$. An obvious condition for the inter-relationship between and $\mathcal{L}$ is that $\mathcal{L}'$ contains names for the sentences of $\mathcal{L}$ and means of saying the same thing as each sentence (i.e. translation of $\mathcal{L}$ into $\mathcal{L}'$).

The instances of Convention T are then to be taken as sentences of $\mathcal{L}'$ with $X$ the name in $\mathcal{L}'$ of a sentence in $\mathcal{L}$ and $p$ the translation of that sentence into $\mathcal{L}'$.

Let me here enter two comments on the interconnection between principle (★) and schema (T). The first concerns the redundancy theory, and is thus somewhat parenthetical in the present context. The second has to do with the apparent realism embodied in principle (★). I think it is sometimes taken that the unravelling of (★) into the (infinitely many) strands of (T), each individually so clear and unproblematic explains, clarifies, and indeed establishes the redundancy thesis about truth. But it is crucial to realize that the redundancy thesis is not adequately served by (the totality of instances of) schema (T). These instances do perfectly well, of course, when dealing with truth as applied to finite collections of specified sentences. But as Ramsey, the man generally credited with first explicitly proposing the redundancy thesis points out: "in the ... case in which the proposition is described and not given explicitly we have perhaps more of a problem, for we get statements from which we cannot in ordinary language eliminate the words 'true' and 'false' " ("Facts and Propositions", p. 143). In such cases, as "Everything he will say is true", what is required for the elimination of "true" is a single sentence with the full force of (★), but minus its obscurity. Convention (T) will not do because it is not a sentence, but
merely an organon for generating sentences. Tarski has called the instances of T "partial definitions of truth", and remarks that "the general definition has to be, in a certain sense, a logical conjunction of all these partial definitions." ("The Semantic Conception of Truth", p. 55). But language, the artifact of finite creatures, generates as units finite sentences; so there is no literal conjunction of the totality of instances of convention T. However, as one learns early in the study of formal logic, language is equipped with a device which achieves the expressive power of infinite conjunction: universal quantification. But it is conjunctions of a particular sort, namely the conjunction of statements of some specified form varied by application to the elements of a specified domain of objects (the domain of quantification). And it is by no means a simple matter to apply that model to the present case, for the quantification over sentences would have to govern at once both the use and the mention of the sentence, as in: \( \forall p (\text{if he says } 'p', \text{ then } p) \), presumably the non-redundant form of \( \forall p (\text{if he says } 'p', \text{ then } 'p' \text{ is true}) \). It must be something of this difficulty which exercises Ramsey on p. 143 of "Facts and Propositions." The hint of a solution there presented suggests quantifying over the syntactic components by which the multiplicity of statements in the range of the quantifier \( \forall p \) is obtained. What he actually instances are atomic statements of the form 'a R b': "'He is always right' could be expressed by 'For all a, R, b, if he asserts a R b, then a R b'". But it is claimed that "when all forms of proposition are included the analysis is more complicated but not essentially different." Clearly dealing with "all forms of proposition"

1. The mathematical theory of infinitary languages is not to the point here.
can only be accomplished by reflecting the recursive operations by which they are generated from a finite stock of basic forms. It is tempting to suppose that what Ramsey had in mind here is something like Tarski’s analysis of truth. ¹ If so these remarks would bear striking testimony to Moore’s comment about Ramsey in a preface to the posthumously collected papers, "but sometimes I feel that he fails to explain things as clearly as he could have done, simply because he does not see that any explanation is needed: he does not realize that what to him seems perfectly clear and straightforward may to others, less gifted, offer many puzzles." But whether Ramsey did indeed envisage an account of truth essentially like that Tarski came to provide, a matter in itself of course of no philosophical importance, I do think that once the would-be redundancy theorist accepts the inadequacy of (A) as an account of truth and appeals to (T) as elucidating what he meant by (A), the pressure of demands on the account of truth will force exploitation of recursive syntactic devices in the manner characteristic of Tarski’s analysis.

My second comment on connections between (A) and (T) concerns, as announced a few pages back, the realism embodied in (A); as such it has to do more directly than did the first with the focus of current consideration: Carnap’s claim that Tarskian theories of truth capture, and indeed validate the classical (realist) conception of truth. That conception consists in the notion that the truth of a sentence (given what it means) is entirely determined by reality, by how it is with the world (however inaccessible

¹ Tarski’s definition of a truth predicate solves the problem of use/mention, whereas the passage from Ramsey just quoted is still confused in this matter. Cf. Quine: "The truth predicate is a device of disquotation." (Philosophy of Logic, p. 12).
to human experience the feature of the world dealt with in that particular sentence may be). Principle (\(\star\)) may be construed as expressing this conception, when 'things', whose reference we earlier found problematic, is taken as referring to reality, the state of the world. Whether these latter terms are actually more specific and readily understood than that in the original formulation might be questioned. But they do at least point more unequivocally in the direction of a notion such as "configuration of the objects referred to in a given sentence", and suggest the idea of that configuration being objective and independent of our perceptions. Discussion in the philosophical literature of such configurations often is couched in terms of "states of affairs." "The cat is on the mat", uttered assertively on a particular occasion and with reference to some particular cat and mat is determined as to truth value by the relationship between those objects; that is, whether they are configured as asserted. This configuration is the state of affairs which obtains with respect to those objects. The truth of a sentence is a matter of whether the state of affairs which obtains for the objects referred to by it is the sort described by the sentence. If it is, the sentence is true. Such, in crudest outline, is the genesis of the correspondence theory of truth. It may be seen as an appealingly natural development of the intuitive thought behind realism: something "out there" determines a sentence as true, or as the case may be, false. This something is not just the objects referred to, since true and false statements may both be about the same things, it is the state of affairs in which they are configured. For brevity, and conforming generally both to ordinary and philosophical usage, we call a state of affairs which obtains a 'fact'. Truth is then to be analyzed as a two place relation of correspondence
between fully specific sentences and facts. Such an account of course requires an ontology of facts in addition to objects, and there are those imbued with Ockhamite parsimony or nominalist scruples who would wish to get on without them. But even with a taste to accommodate as many and as bizarre objects as possible, notorious problems stand in the way of rendering an account of facts. These begin to be apparent as soon as one asks after the facts corresponding to statements of greater complexity than the favorite "cat on the mat" example: what fact renders true a negative statement? What are the objects that constitute a fact in the case of those corresponding to disjunctive statements? What facts correspond to conditionals? How are facts individuated? This is not the place to rehearse the misadventures of attempted answers to these challenges. But in any case the reconstrual in terms of (T) of the account of truth by (★) is meant to bypass these difficulties, if not to solve them. And Tarski remarks, in reference to such formulae as: "The truth of a sentence consists in its agreement with (or correspondence to) reality" and "A sentence is true if it designates an existing state of affairs": "However all these formulations can lead to various misunderstandings, for none of them is sufficiently precise and clear." And he concludes "it is up to us to look for a more precise expression of our intuitions." ("The Semantic Conception of Truth", p. 54) Tarski's answer is (T). I will present an argument, shortly, to show that, whatever charms may be possessed by theories conforming to convention T, they are not, in virtue of so conforming, realist in character. Thus it must be that the undoubted gain in clarity in moving from (★) and its cognates in terms of facts, etc. to (T) was effected at
the expense of accounting for what is peculiarly realist about (A).

The argument as presented will not rule it out that theories in conformity with (T) do embody realism. Tarski does not, to the best of my knowledge, go back to consider explicitly the interpretability of the earlier (unclear) classical conceptions in the theories he constructs; such lack of discussion perhaps is unsurprising in view of Tarski's (unwarranted) faith that conformity with (T) itself guarantees the classical conception. There is something of such a discussion by Davidson in his "True to the Facts": "In this paper I defend a version of the correspondence theory. ... The semantic concept of truth, as first systematically expounded by Tarski, will play a crucial role in the defense" (p. 748). Davidson's basic ploy is to find in Tarski's device of satisfaction an explication of the old notion of correspondence. There is initial plausibility in this idea since the explication of truth in terms of correspondence and that in terms of satisfaction have the same syntactic, we might even say logical, form: both correspondence and satisfaction are two place relations, one of which places is to be filled by a sentence designator, and when the other is existentially quantified the resulting one place predicate is co-extensive with (constitutes an explicans for) truth. (This point would seem to answer Frege's objection: "Correspondence is a relation. This is contradicted, however, by the use of the word 'true', which is not a relation-word and contains no reference to anything else to which something must correspond." "The Thought : A Logical Inquiry", p. 19) There is little reason to expect in such coincidence a solution to the problems of an ontological account of facts earlier adumbrated, a charge to which Davidson is sensitive when he allows that "the relation, satisfaction is
not ... exactly what intuition expected of correspondence; and the functions or sequences that satisfy may not seem much like facts.

Indeed, Davidson is ready to fault facts for what is surely the only reason for invoking them: "the failure of correspondence theories of truth based on the notion of fact traces back to a common source: the desire to include in the entity to which a true sentence corresponds not only the objects the sentence is "about" ... but also whatever it is the sentence says about them." Nonetheless, Davidson deems the account of truth in terms of satisfaction to constitute something of the essence of correspondence theory because "the property of being true has been explained, and non-trivially, in terms of a relation between language and something else." But I do not think this condition is enough to capture what was taken to be explanatory about correspondence as an account of truth, for that "something else" to which Davidson's account would non-trivially relate a true sentence is not that in virtue of which the sentence is true; a sequence of objects certainly does not 'make' a statement true. Now it may well be that Davidson would reject this desideratum on an account of truth, indeed his earlier quoted remark on the reasons for failure of correspondence theories heretofore strongly suggests this. But if so, then his modelling of correspondence within Tarskian theories of truth can not serve to vindicate Tarski's claim, utilized as we see by Carnap, to have explicated the classical (realist) conception of truth. As it happens, I do not think Davidson has managed even to show Tarskian theories where truth is defined in terms of satisfaction to be forms of correspondence in his minimal sense. For while on such accounts we may take a closed sentence as true just in case
there is some sequence which satisfies it, this condition is equivalent
to satisfaction of that sentence by every sequence. And while the
existential condition seems to provide a "something else", relation to
which non-trivially explains the truth of a sentence, I see no way of
extracting such an explanatory object from the equivalent universally
quantified condition. It does not seem correct to construe Tarski's
analysis of truth as providing an explanation of the truth of a sentence
in terms of its relation to something else, linguistic or otherwise.

The line of explicative analysis leading from principles like (★) to
(T) has been taken by Tarski, and others such as Carnap following him,
to guarantee that truth theories, in conforming with (T), are theories of
the classical conception of truth. I now present grounds to doubt any
such guarantee.

Let us suppose that we are presented with the theory ℳ (couched in
the language ℒ′) of a one place predicate T(x), whose arguments range
over sentences of a given language ℒ, a sublanguage of ℒ′, and which is
such that every sentence of the form 'T(n) if and only if p' where p
is a sentence of ℒ and n is its name in ℒ′ is a consequence of ℳ. Can
we conclude that a realist conception of truth is characteristic for ℒ
and that T captures that conception? Let me present first the outline of
an argument, in terms of the purely syntactic character of ℳ, that we
cannot.

Taking ℒ as presented by generation from a formal grammar, we
may take ℒ′ as endowed with arithmetic, by which to formalize the syntax
of ℒ, the primitive one-place predicate T, and enough weak set theory
to form denumerable sequences of individuals from the domain of quantification of $\mathcal{L}$ (actually finite sequences without limit of length would do well enough). For axioms we take, in addition to those for arithmetic, the recursive clauses which (ostensibly) characterize $T$ as truth, e.g. $T(\ 'p \lor q' )$ if and only if $T( 'p' )$ or $T( 'q' )$, and we need of course a background logic. As is well known, if that is full classical logic then every instance of $(T)$ can be derived. But what I want to claim is that if we inspect these derivations we will see that we have not utilized in them any principles of logic which are not valid on an intuitionist interpretation, basically that is, no arguments use essentially the principle of excluded middle, or classical argument by cases. Further, every one of the axioms will bear anti-realist interpretation - certainly all the principles of arithmetic, and also more crucially to the argument the axioms characterizing $T$, where now it is interpreted in a suitably constructivist manner, as e.g. "is demonstrated", "is confirmed." To illustrate with the axiom governing application of 'T' to disjunctive sentences: a disjunction is demonstrated just in case the first disjunct is demonstrated or the second disjunct is demonstrated. This principle is characteristic of the constructivist canon of demonstration. Unsurprisingly then, the instances of convention $T$, with 'if and only if' understood anti-realistically, are all correct with $T$ the entirely unclassical notion of truth in terms of demonstration. To see this directly, we take the two halves of the biconditional in turns; remembering that 'if $p$ then $q$' is to be construed as "from a demonstration of $p$ we may effectively obtain a demonstration of $q"$: well, if we have a demonstration of $p$, then pointing to it constitutes a demonstration that $p$ is demonstrated, i.e. $p \Rightarrow T( 'p' )$. And if we can demonstrate that there is a
demonstration of \( p \), we have thereby actually demonstrated \( p \). Hence
\[
T('p') \rightarrow p.
\]

It is evident that this last argument as to the anti-realist correctness of convention \( T \) is entirely general and does not depend on any account of how the instances of \( (T) \) are generated. Thus we may conclude that mere conformity to convention \( T \) in no way shows that the predicate \( T \) which so conforms is classical truth as opposed to constructivist notions of demonstration.

Looking back to the preceding argument in terms of constructive interpretability of \( \mathcal{J} \) and the logic requisite to generating \( (T) \) from it establishes the stronger claim that even if the instances of \( (T) \) are known to arise from the theory which Tarski constructed, we still lack grounds on the basis of that knowledge alone for concluding the predicate characterized to be classical truth. The situation with regard to realist vs. anti-realist interpretability of \( \mathcal{J} \) is exactly like that of formal arithmetic: in any derivation not employing excluded middle, the premises, steps of logical inference and the conclusion can be accepted by both classical and intuitionist mathematicians - each of course understanding them in his own way.

Some of Tarski's "Polemical Remarks" from "The Semantic Conception of Truth" may be construed indirectly as related to the preceding discussion. He considers the following objection: "In formulating the definition we necessarily use sentential connectives. ... However, it is well known that the meaning of sentential connectives is explained in logic with the help of the words 'true' and 'false' ... Hence the definition of truth invokes a
vicious circle." (p. 66) Tarski's immediate response is that "if this objection were valid, no formally correct definition of truth would be possible," (p. 67) He is confident, however, that "the situation is not so bad" as this; the ground for his confidence lies in appeal to the "strictly deductive development of logic". He allows that such development "is often preceded by certain statements explaining the conditions under which sentences of the form "if p, then q" etc., are considered true or false. (Such explanations are often given schematically, by means of the so-called truth-tables.) However, these statements are outside of the system of logic, and should not be regarded as definitions of the terms involved." (my emphasis, p. 67) So Tarski here takes the view that meaning of the logical connectives is not given in terms of the notion of truth and by appeal to which certain general principles of logic are shown valid, but rather the connectives take their meaning from the formal systems governing their deductive properties. Indeed, he goes so far as to claim that "these statements [explaining the conditions under which statements of given logical form are true or false in terms of truth and falsity of components] do not influence the deductive development of logic in any way" (my emphasis). This extraordinary faith in the axiomatic method rather invites Russell's wry assessment of it as having "all the advantages of theft over honest toil." And more to the point, the serious question is how one is to be guided in the choice of formal deductive principles, if not constrained in that choice by the aim of deducing certain principles antecedently realized to be valid on the basis of a conception of truth and meaning? Supposing a cogent answer forthcoming to this question, the objection Tarski is specifically considering may be handled; the picture is of logic coming
from on high in the guise of ready made formal systems, and once possessed of these we go on to all manner of deductive theories, including say, geometry, physics, and among them semantics: "the moment we find ourselves within the deductive system of logic - or any discipline based upon logic, e.g., of semantics - we either treat sentential connectives as undefined terms, or else we define them by means of other sentential connectives, but never by means of semantic terms like 'true' or 'false'."

Now I find this conception all but incoherent, but even were such appeal to the virtues of the axiomatic method here legitimate, the difficulty I have raised cannot be so dealt with. This difficulty is one familiar in the construction of non-logical axiom systems, meant to characterize e.g. such notions as natural number and the arithmetic operations on them - the appropriate axiom system will often fail to be categorical, in the sense that what is characterized by it is unique. So Dedekind's "Peano postulates" of arithmetic fit the (intended) $\omega$ structure of the natural numbers, but also uncountably many other $\omega + (\omega^* \div \omega) \cdot \eta$ structures. And in the case of $\mathcal{T}$ theories, viewed as a purely formal deductive apparatus, we have seen that they may concern the classical conception of truth (Tarski's intended interpretation), but equally they may be taken as an account of the conception of truth appropriate to anti-realism.

I now consider an argument purporting to show that $T$-theories are theories of (classical) truth which turns, not upon intensional consideration as to the analysis of problematic principles such as $(\bigstar)$ in terms of $(T)$, but proceeds by direct reflection on the extension of '$T(x)' as fixed by the totality of instances of convention $T$. The idea is that the extension of $T$ must be precisely the true sentences of $\mathcal{L}$ since $T$ applies to a sentence when it is true.
and does not when it is false - such is the effect of \((T)\) - instances and the way "if and only if" operates. The preceding sentence constitutes an entirely valid argument - valid from both classical and intuitionist viewpoint. But what would be quite illegitimate would be to take it as showing \(T\) faithful to one or another of these two viewpoints; in particular it will show the extension of 'Tx' to be classical truth just in case "if and only if" is construed classically. What then if "if and only if" is taken intuitionistically? We must be careful how we describe the situation since intuitionistic demonstrability is not a strictly extensional notion as classical truth is. For simplicity, let us suppose we are dealing with arithmetic, and take it that intuitionist assertability is properly captured by some notion of recursive realizability. Then I think we may expect very roughly the following situation: if for a closed formula \(\varnothing\) and a natural number \(n\) we can prove that \(n\) realizes \(\varnothing\), then we can prove \(T(\varnothing')\). And conversely, from a proof of \(T(\varnothing')\) we can extract a natural number \(n\) which (probably) realizes \(\varnothing\).

Here I conclude for now my discussion of the realist/anti-realist character of the concept of truth as embodied in Tarski's analysis, and take this discussion to have shown Carnap unjustified in concluding from what he takes as Tarski's success in "establishing an unobjectionable definition of truth" that hence the term 'true' should be understood realistically. I now discuss briefly the positivists' pre-Tarski conception of truth.

As with so much else of their philosophical viewpoint, the positivists initially took their account of truth from Wittgenstein's *Tractatus*. That account constitutes a thoroughly realist correspondence theory; the logical
atomism of the Tractatus provides for the appealing analysis of correspondence as a relation of 'picturing' between sentence and state of affairs. Crucial to the story is a dichotomy between language (seen as a kind of picturing device) and reality, with truth determined by agreement or disagreement of the two, and knowledge by comparison; thus at 2.223: "In order to tell whether a picture is true or false we must compare it with reality." It was Neurath, it seems, who first began to be uncomfortable with this account. Carnap soon joined him; they were both dissatisfied by the notion of comparing statements with reality. The alternative seemed to be comparing statements with other statements, and they found themselves embracing a kind of coherence theory in which the truth of a statement consists in its agreement or consistency with the range of basic statements (protocolsätze) generally accepted. Schlick continued to champion the legitimacy of comparing statements with reality (cf. "The Foundation of Knowledge"), (even while branding the issue of the existence of objective particulars (cf. "Positivism and Realism") in the form "is there an external world?" a pseudoproblem from metaphysics); he remarks in replying to Hempel, who had joined the fray on the side of Carnap and Neurath with his paper "On the Logical Positivists' Theory of Truth": "I have been accused of maintaining that statements can be compared with facts. I plead guilty. I have maintained this. But I protest against my punishment: I refuse to sit in the seat of the metaphysicians" ("Facts and Propositions", p. 65). Indeed Carnap's and Neurath's scruples as to the Tractatus conception were basically concerned with its metaphysical character, and Hempel put the matter bluntly: "None of those who support a cleavage between statements and reality is able to give a precise account
of how a comparison between statements and facts may possibly be accomplished, and how we may possibly ascertain the structure of facts. Therefore, that cleavage is nothing but the result of a redoubling of metaphysics, and all the problems connected with it, are mere pseudo-problems" ("On the Logical Positivists' Theory of Truth", p. 50). In part at least this dread charge resulted from the rather draconian application of the forced move from the material to the formal mode of speech, a device invented, or at any rate much exploited by Carnap for the eliminating of metaphysics (cf. Logical Syntax of Language, esp. Part V). Roughly, statements in the material mode are about extra-linguistic entities, those in the formal mode belong to syntax. For example "Every tone has a certain pitch" is material mode, "Every tone expression contains an expression of pitch" is its formal mode translation. If we constrain ourselves to express all philosophical theory in the formal mode we stand a much better chance of avoiding nonsense, and doing justice to the proper function of philosophy, which consists in the explication of the logic of science through attention to language, though it is allowed that "the material mode of speech is not itself erroneous; it only readily lends itself to wrong use." (The Logical Syntax of Language; cf. "Testability and Meaning" section 4, p. 312). The impossibility of translation into the material mode, seems however, to be taken as an evident indication that some putative philosophical thesis belongs to metaphysics. "Saying that empirical statements 'express facts' and consequently that truth consists in a certain correspondence between statements and the 'facts' expressed by them, is a typical form of the material mode of speech." (Hempel, op. cit., p. 54) But when the 'translation' into
the formal mode of this account of truth is effected, what results is the form of coherence theory advocated by Carnap and Neurath: "the concept of truth may be characterized in this formal mode of speech, namely, in a crude formulation, as a sufficient agreement between the system of acknowledged protocol-statements and the logical consequences which may be deduced from the statement and other statements which are already adopted." (Hempel, op. cit., p. 54.)

An argument of some interest can, I think, be extracted from this rather mechanical operation of some fairly dubious apparatus. But it is also to be seen as arrived at by independent epistemological considerations. Consider the issue how we come to know, or establish a statement S as true. By the correspondence theory we must show the existence of a fact to which S bears the appropriate relation. To do this we must first pick out the fact by describing it (facts do not come tagged with names) and then compare it to S. But how is a fact F to be described other than by a sentence S'? And if so, then the comparing of F with S must actually be a matter of determining that the appropriate relationship between S and S' obtains. In consequence, the truth of a sentence consists in its relationship to another, or other sentences. And so coherence.

Verificationism might be characterized as the doctrine that epistemology comes first. Thus in general to fault a theory as metaphysical for failure to allow for an appropriate epistemological account of its workings is indeed to use the verification principle as the sharp knife it was intended to be, by which metaphysical nonsense is to be excised from philosophy. This particular application, however, suffers from a mis-ordering of priorities
as it constitutes an attempt to apply a decision as to the nature of meaning to obtain a characterization of truth. By the framework developed from Chapter 1 as an account of realist/anti-realist interactions between truth and meaning, it should be reflections as to the character of truth which lead to an account of the nature of meaning. But in any case, whether from such grounds as these, or merely by failure to consider more accurately what goes on when we establish a sentence as true, it is evident that this coherence theory is quite untenable. As Ayer points out ("Truth", p. 180), while "there is no way of characterizing facts except by making true statements, ... this does not mean that the statements have to be identified with the states of affairs which they describe. The circle is broken by observation and by action." And indeed this circle the would-be coherence theorists had already found themselves forced to break (in attempting to avoid utter absurdity), by at least limited appeal to fact:

What characteristics are there according to Carnap's and Neurath's views, by which to distinguish the true protocol statements of our science from the false ones of a fairy tale? As Carnap and Neurath emphasize, there is indeed no formal, no logical difference between the two compared systems, but an empirical one. The system of protocol statements which we call true, and to which we refer in every day life and science, may only be characterized by the historical fact [my emphasis], that it is the system which is actually adopted by mankind, and especially by the scientists of our culture circle. (Hempel, op.cit., p. 57)

And of course in addition to appealing here crucially to at least one fact, there is the obvious demand for an account of why these particular protocol sentences are the ones generally accepted, the answering to which seems of necessity to require appeal to a good many others.
The evident enthusiasm with which Carnap greeted Tarski's analysis of truth may be surmised to have resulted in part at least from appreciating somehow how highly awkward and problematic a view as to truth it was, into which he had argued himself. The justification for switching over (or rather back) to an ostensibly realist conception of truth had presumably to be on grounds that Tarski's formal construction of semantics showed that expressing the truth conditions of a sentence (the fact to which it must correspond to be true) in the material rather than the formal mode could indeed be accomplished without lapse into metaphysical nonsense (a possibility which it will be remembered Carnap in general allows for). And as it was translation into the formal mode which resulted in coherence, that view is now avoided. But as our earlier considerations show, this move of Carnap's is not a matter of back-sliding, for Tarski's analysis of truth is quite free of any problematic talk of "comparing" sentences with reality; in so excising what was problematic in the Tractatus-inspired correspondence conception of truth, what was characteristic of it has, as we saw, been left out. The result is a framework which can do duty for either basic conception of truth.

§ 3. The roots of incoherence

We have considered thus far in this chapter the prima facie case for construing verificationism a form of anti-realist semantics, and we have also seen that the positivists did not view it as such, but rather uncritically (and it may be claimed, unsuccessfully) opted for a realist conception of truth. Given the arguments from Chapter 1 as to the essential interconnection between realist vs. anti-realist semantics and corresponding
conceptions of truth, this situation with respect to positivism calls for an explanation why and if successful how they combined these ostensibly incompatible conceptions. I shall first consider in this section the why of it, and then present in the following section an argument decisive, I think, against this hybrid combination of realist conception of truth with anti-realist semantics. Were such an argument not forthcoming the conceptual framework sketched in Chapter 1 would of course be called into serious question. This chapter will then conclude with the legacy of verificationism for anti-realism, chiefly in the form of problems which confronted verificationism and continue, perhaps in transposed form, to demand solution if some form of anti-realism is to prove tenable.

It is first to be noted in considering why verificationism fails to constitute anti-realist semantics that verificationism was conceived of by the positivists not so much as a contribution to the philosophy of language, the outgrowth of concern as to the basic character of language, but rather as a polemical weapon, a weapon by which various philosophical pronouncements they found uncongenial could be discredited through being branded nonsensical. The cutting edge of verificationism then, was its function as a criterion of meaningfulness. In some early formulations there seems to have been an unnoticed confusion between a theory which declares a sentence meaningless if possible experience can not be indicated which would tend to decide its truth value and a theory which actually identifies the meaning of a sentence with such possible experience, or better, the capacity to recognize such possible experience. Where the distinction was explicitly drawn, there were those positivists who professed the greater concern for the former, ostensibly weaker sort of theory. Indeed, sometimes in the face of
criticisms directed at verificationism it was claimed that retreat
from the stronger theory of meaning to the weaker criterion of meaningfulness both obviated the professed difficulties and preserved what was essential to the positivist enterprise.

There is something perverse and wrong-ended it seems to me in analysing the nature of meaning in language merely with the goal of casting aspersions of a particular sort on various odd bits of language. An account of the functioning of meaning in language is in itself such a difficult and delicate matter even with respect to the most straightforward and central uses of language, that it seems at the very least highly premature to expect any such account to solve for us problems about the nature of what must from the beginning be admitted are quite special, out of the ordinary uses of language. And the idea that one can have a criterion of meaningfulness without having already some account of how sentences are endowed with meaning seems to me entirely dubious - that is, unless the criterion is allowed to be a matter of free and seemingly arbitrary choice (a ploy adopted on some occasions by positivists). But then it is very difficult to imagine why anyone not already in agreement with that criterion could be convinced to submit to its dictates. Anti-realist semantics in general, as modelled on mathematical intuitionism in particular does not say of utterances by one who would profess a realist conception that anything he says is meaningless. It is rather that the intuitionist mathematician does not find the putative classical meaning of a particular statement comprehensible. But that form of words is not for him meaningless. For he has a general understanding of how syntactic combinations of basic language forms using such devices as sentential connectives and
quantifiers produce from the meanings of the basic components meaning for the whole complex. As the classical mathematician's sentence will have used those devices, the intuitionist's understanding of mathematical language will endow it with meaning, a meaning which will in general differ from that the realist thought it to have, and which difference can in the case of analysis lead even to divergence as to assessment of truth value; there are expressions of the form \((\forall x)(\emptyset(x) \lor \lnot \emptyset(x))\) which the intuitionist will take to be false, whereas of course the classical mathematician will maintain the truth of any statement of that form. This discussion points to the fact that verificationism as an account of language meaning lacks the structure to account for sentence meaning as a systematic contribution of its syntactic components. But as we have noted from early on Brouwer's development of anti-realist semantics has been fully in accord with it.

With their professed opponents the positivists wished to differ within a quite narrow range of discourse - practically all within the particular and peculiar confines of philosophy (or rather, they intended to do away with the peculiarly philosophical). The vast range of language use - scientific and ordinary - they wished to leave undisturbed. So in this approach they were entirely unlike Brouwer and mathematical intuitionism, and indeed they seem on the whole to have been mainly uncomprehending as to what Brouwer was up to (cf. Carnap's brief comments in Logical Syntax of Language and "Testability and Meaning"). The verificationists are more akin to Hilbert's form of constructivism than to Brouwer's intuitionism; certainly the nearly dogmatic faith in the principle of excluded middle is shared, as is also the desire to preserve the
classical edifice respectively of mathematics and science (taken broadly enough to include rather ordinary experiences) but on a sounder footing than the previous uncritical understanding of statements from those domains would have allowed.

Having noted that the positivists' interest in verificationism was primarily as a polemical tool to certify putative philosophy as nonsense, it is of interest to observe further that the doctrine which underlies their enterprise is not actually verificationism though there was on occasion confused failure to distinguish the two. That doctrine is a form of the principle of bivalence: every statement, unless it is nonsense and so fails to make a statement is either true, or it is false. And conversely, a statement incapable of being either true or false is thereby nonsense. Thus Carnap in his essay "The Elimination of Metaphysics": "Since 'Caesar is a prime number' ... expresses neither a true nor a false proposition, we call this word sequence a 'pseudo-statement'." (p. 68). And it is often emphasized in their objections to such doctrines as idealism and realism (as the claim that an 'external' world exists) that it is not that they are false, but incapable of even being shown to be false because nonsense, pseudo-propositions. Scheffler, in his careful (and generally sympathetic) reconstruction of the empiricist (positivist) criterion of significance takes as his first condition of adequacy:

"C.A. I: For every sentence S, S is true or false if, and only if, S is significant" (The Anatomy of Inquiry, p. 129). It no doubt goes without saying that this condition was understood classically (realistically) - not in the anti-realist sense of $\neg \neg (A \lor \neg A)$ discussed earlier in connection with Carnap's claims as to the principle of excluded middle in common
discourse. The following question evidently confronts us: how did the positivists come to embrace a conception of meaning possessed of such apparent anti-realist character starting from so thoroughly realist a principle as Scheffler's C.A. I?

The answer seems to be that verificationism was thought to constitute an explication or perhaps functional account of the application of C.A. I. Thus Ayer writes in the preface to the second edition of *Language, Truth and Logic*: (p. 15) "... it is only if it is literally meaningful, in this sense [i.e. satisfies the principle of verification], that a statement can properly be said to be either true or false." And it is this correlation which Ayer takes to provide grounds for adoption of the verification principle. This principle is construed as a definition of meaningfulness which we might or might not choose to adopt even if well advised to do so: "thus, while I wish the principle of verification itself to be regarded, not as an empirical hypothesis, but as a definition, it is not supposed to be entirely arbitrary."

Exactly parallel to the move from having a truth value to being verifiable in consideration of criterion for meaningfulness, there is a shift from accounting for the meaning of a statement as given by its truth conditions to identifying its meaning with the processes which would tend to establish or refute it. Arthur Pap presents a particularly inexplicit and unargued (but not thereby atypical) instance of the slide from one to the other: "Now, our principle is best interpreted as a guide to the discovery of the truth-condition ... of a statement [p]. How, indeed, do we go about verifying whether [p]?" (*An Introduction to the Philosophy of Science*, p. 7). For this same juxtaposition see Waismann "Logische
Analyse der Wahrscheinlichkeitsbegriffe", p. 229, and also in his "Thesen" p. 244 in Ludwig Wittgenstein und der Wiener Kreis.

But these quotations from Ayer, Pap and Waismann do not yet provide a complete answer to the question which is of concern here; for while they establish the terms in which the shift from accounting for meaning or meaningfulness in terms of truth to doing so with appeal to verification takes place, they as yet give no clue as to the justification for such shift. Or rather perhaps only a clue, namely that the two accounts were thought extensionally equivalent. At first sight there is little reason to expect that they will be, and indeed on the basis of the very distinctions drawn by Carnap in "Truth and Confirmation" prima facie grounds to expect that they will diverge. Following this section I will argue that those expectations are fulfilled. But for now the task is to consider the basis on which positivists were confident there would be no such divergence.

There is, I think, a two stage argument to this effect. We begin with a statement S possessed of determinate truth value. Stage one consists in appeal to the analytic/synthetic dichotomy; S has the truth value it does either solely in virtue of logic and meanings of the words occurring in it (analytic), or it must be in virtue of empirical facts, how it is with the physical, observable world (synthetic). In the first case verificationism need not be invoked, a constant caveat in their formulations which we have heretofore not found it needful to consider. In the second case we have it then that S must have "something" to do with (possible) experience. Evidently to have reached this conclusion requires not only the dichotomous classification into analytic and synthetic as to how sentences are possessed of truth value, it requires as well the doctrine, affirmed generally in
positivism, and not e.g. by Kant, that this dichotomy coincides with the classification into \textit{a priori} and \textit{a posteriori}. The argument actually has run: if S is not analytic then it must be \textit{a posteriori}. At stage two in the argument the notion "has something to do with experience" is cashed in terms of verification/confirmation. For this step I can see little in the way of justifying argument, and it seems thereby a serious lacuna in the verificationist position. The following passage from Schlick's "Meaning and Verification" illustrates the unargued move from having something to do with experience, here in the form of ostensive definition of \textit{words}, to the idea that this somehow dictates that \textit{sentence} meaning is to be given in terms of procedures which would allow possibility of verification (note of course that verification can apply only to sentences and not to words):

\begin{quote}
...In order to understand a verbal definition we must know the significance of the explaining words beforehand, and ... the only explanation which can work without any previous knowledge is the ostensive definition. We conclude that there is no way of understanding any \textit{[sentence?] meaning without ultimate reference to 'experience' or 'possibility of verification'. ... It is this situation and nothing else that we describe when we affirm that the meaning of a proposition can be given only by giving the rules of its verification in experience. \hspace{1em} (p. 143)
\end{quote}

The point at which the word "meaning" occurs with my query as to whether it is \textit{sentence} meaning seems the salient point at which the unargued change in perspective occurs through (unconscious) exploitation of ambiguity.

This lacuna, so well exemplified in this discussion by Schlick, is closely related to the lack of systematic account of sentence meaning in terms of sub-sentential components earlier expostulated upon. For a sentence can "have something to do with experience" by virtue of its predicates being
observational in character, and indeed this would seem to be the standard analysis of that notion. But this condition fails to guarantee sentence confirmability unless one has an account, as we have noted the mathematical intuitionists do and the positivists do not, of how the sentence forming devices relate directly the observations determinative of applicability for those component predicates to observations which will be determinative of assertability for the sentences in which they figure as components. The needed account for confirmability in empirical discourse might well be modelled on the systematic semantics of the mathematical intuitionists, but this would be precisely to reinterpret the logico-syntactic devices of languages in conformity with an essentially anti-realist understanding of language, in particular the truth functions could no longer be taken as determinatives of the sense of the sentential connectives, though of course individual calculations in conformity with them as to sentence assertability in terms of component assertability remain correct.

Hempel's "translatability into an empiricist language as a new criterion of cognitive meaning" (§3 of his article "The Empiricist Criterion of Meaning" - there credited to Carnap in "Testability and Meaning") can be construed as an attempt to remove the lacuna of an argument to justify the step from stage one to stage two simply by doing without this step, though he does not locate the difficulty which leads him to this proposal there; his motive is rather that he finds it "useless to continue the search for an adequate criterion of testability in terms of deductive relationships to observation sentences" (p. 116), the misadventures of which he has rehearsed in the preceding section. The doctrine then is that "a sentence has cognitive meaning if and only if it is translatable into an empiricist language" (p. 116),
where "a language L is empiricist if all its sentences are expressible, with the help of the usual [i.e. realist] logical locutions, in terms of observable characteristics of physical objects." (p. 117) Whether a suggestion of this sort can actually solve the difficulties it is intended to bypass may be doubted (a point realized by Hempel in his later postscript). In particular, if translation is construed in terms of likeness as to meaning, the appeal to translatability must be worrying since the proposal is tendered in consequence of failure to render an adequate direct account of meaning or meaningfulness. Still, it might be claimed that whether Hempel's proposal leads to a theory which can actually be worked with (which it seems not to) the dichotomous classification of statements into analytic and synthetic with the non-analytic ones being those to do with experience makes it "in principle" correct (the retreat to stage one of the above argument). And so at least the minimal claim that meaningfulness in the sense of "can be true or false" is to be cashed in terms of some connection with experience (even if not sentential testability) survives. But now we must scrutinize more closely the premises of this (weak) conclusion, and notably the identification of the synthetic with the a posteriori. And what is striking in this context about that identification is that the analytic/synthetic distinction has to do with a mode of truth, while that of a priori/a posteriori is in terms of the character of knowledge. I do not at this stage mean to argue against the claim of coextension, but merely to identify it as a source for the problematic scheme in which a realist conception of truth is conjoined with some manner of anti-realist conception of meaning. The coincidence of these different category dichotomies ("the view that all non-analytic knowledge is
based on experience") is termed by Hempel (in the opening of remarks of "The Empiricist Criterion of Meaning") "the fundamental tenet of modern empiricism". (p. 108). He there takes positivism to be advancing a further thesis when it is claimed that every cognitively meaningful assertion (which in an aside he effectively identifies with sentences which "can be said to be true or false") is if not analytic or contradictory, "capable, at least in principle, of experiential test." This formulation corresponds at least roughly to my two stage line of development to the principle of verification.

Before concluding this section on anti-realist semantics with realist antecedents with a discussion of what the positivists took from, or in any case thought they owed to Wittgenstein, I insert three brief comments on the analytic/synthetic distinction. The first is as follows: as we have seen the fundamental tenet of logical positivism is the identification of the a priori/a posteriori dichotomy with the analytic/synthetic one. On such a scheme mathematics must be counted purely analytic, an account of its status which puts logic prior to mathematical activity (a serious mistake, with disastrous consequence, on Brouwer's view) and leaves no room for the creative act of mathematical intuition. There is of course another feature of intuitionism which may explain positivism's blind eye toward this conception of mathematics: certain passages of Brouwer's have the sound of metaphysics which it was the positivists' mechanical reaction to brand 'pseudo-statements'. Thus in his "relatively late) paper (1948) "Consciousness, Philosophy, and Mathematics" Brouwer opens with the remark: "Consciousness in its deepest home seems to oscillate slowly, will-lessly and reversibly between stillness and sensation,"
in which he is seeking to describe how subjective experience gives rise to the notion of a reiteratable unit, out of which we construct arithmetic. But in fact Brouwer's papers in the 1930s have a considerably more sober, ordinary sound to them, with special uses of terms generally well defined.

My second comment is that as we have seen, the analytic/synthetic distinction plays a crucial role in the positivists' move to combine classical truth with anti-realist semantics, a hybrid which I shall argue in the next section proves, as we might expect, incoherent. This suggests that if the doctrine of the analytic/synthetic dichotomy is exorcised, as Quine and others following him would have it, we might then achieve what Schlick thought he already had in boldly appropriating to his program the sobriquet "consistent empiricism" ("Positivism and Realism", p. 106).

My third comment concerns the employment of the analytic/synthetic distinction as a device for developing a thorough-going realist semantics. This is indeed the use to which Carnap came to put this distinction in his later work in semantics, notably Meaning and Necessity. By taking the totality of analytic truths of $\mathcal{L}$ one isolates the interconnections between words which are characteristic of their meaning in $\mathcal{L}$; let them be denoted by $'\mathfrak{H}'$. They are "the meaning postulates for $\mathcal{L}$." A model structure of appropriate similarity type in which all the sentences of $\mathfrak{H}$ come out true constitutes a possible state of affairs describable in the language $\mathcal{L}$. (The subject of this comment is a matter of immense complexity, appropriately treated as a chapter in itself - to mention but one complication, the point of the preceding sentence goes through only if the analytic/synthetic dichotomy is taken as coincident with that of necessary/contingent.)
Given this apparatus, the realist notion of truth conditions may then be identified with possible states of affairs ('worlds') in the sense of model structures satisfying $\mathfrak{M}$. It is possible to construct a semantic system which may be claimed to embody the view espoused similarly by Frege, and Wittgenstein in the *Tractatus* that the sense of a proposition consists in the classification of possible situations into those in which it is true and those in which it is false (cf. Carnap, *Meaning and Necessity*, p. 9). From this impossibly fragmentary sketch one may perhaps extract a hint as to how it is that Carnap saw himself as engaged in the same enterprise from beginning to end, loyal throughout to the same basic tenets of empiricism, with merely a variation in how those tenets were to be realized in various attempts at the same goal. That variation ranges through strict verification, to logical syntax, to possible world semantics.

I mentioned the *Tractatus* in the previous comment as espousing a thoroughly realist conception of semantics. Certainly the notion of truth there employed is realist, governed as it is by the "classical" truth tables (a technique of Wittgenstein's devising). And it seems clear that Wittgenstein meant to exploit that conception in the account of meaning constructed in terms of it, and that his way of exploiting it fits what has been characterized in Chapter 1 as realist semantics. Thus at 4.024 he remarks: "to understand a proposition means to know what is the case if it is true."

It is unnerving then to encounter frequently in positivist literature remarks attributing the principle of verification to Wittgenstein and citing the *Tractatus* as source. Reichenbach speaks of "the idea expressed by Wittgenstein in his formula: the meaning of a proposition is the method of its verification," to which he then attaches the following footnote: "although this formula is
not verbally contained in Wittgenstein's *Tractatus Logico-Philosophicus* (London, 1922), it expresses his ideas very adequately and has been used, with this intention, within the 'Vienna Circle'.” (Experience and Prediction, p. 49). Compare also Carnap, "Testability and Meaning", p. 422: "The requirement of verifiability was first stated by Wittgenstein", with footnote to the *Tractatus*. The interpretation seems to have been considered so evidently apt as to obviate need for references to supporting passages from the *Tractatus*, and amongst such attributions this absence of support is not infrequent. There are passages which might be thought open to such misinterpretation, perhaps notably in the midst of 4.063: "... I must have determined in what circumstances I call 'p' true, and in so doing I determine the sense of the proposition"; it would be a matter, I suppose, of how one takes the word 'determined.' To the extent that there is this ambiguity of interpretation, what it is symptomatic of is that realism and anti-realism are semantic theories which share a common form; the source of their contrast is the input from which they begin, namely the character of truth.

There may be another explanation as to why positivists were inclined to credit Wittgenstein with the doctrine of verificationism and to cite the *Tractatus* for that attribution. It is now well established that between 1929 and 1932, the period during which he first returned to philosophical concerns (having left them with the completion of the *Tractatus*) and a time also when he was in contact with some members of the Vienna circle (notably Schlick and Waismann), Wittgenstein quite unequivocally formulated and espoused an extreme form of verificationism. For a concise, meticulously documented chronicle and illuminating analysis of this stage in Wittgenstein's thought
see the section of Peter Hacker's book *Insight and Illusion* entitled "The Positivist Interlude" (pp. 104-111). To take but one of the references cited there: "How a proposition is verified is what it says ... Verification is not a mark of truth, but the sense of propositions." (Philosophische Bemerkungen, §166). And also, "according to my principle two suppositions must have the same sense if every possible experience that confirms the one also confirms the other, if, that is, no decision between the two is conceivable on the basis of experience." (Philosophical Grammar, pp. 219-220). So it may well be that those positivists who acknowledged an indebtedness to Wittgenstein in the matter of verificationism had indeed, perhaps indirectly e.g. through report, been influenced by views of his which he indeed did hold after 1929. As for citing the *Tractatus* in their attributions, this work constituted Wittgenstein's only public statement of his philosophical position (barring the significant but almost immediately disowned paper 'Some Remarks on Logical Form') made by him during his own life time. This situation may well have encouraged the view that important doctrines he was known to espouse must somehow be contained in that elusive and aphoristic work, a misunderstanding which Wittgenstein seems not to have been in this period at pains to correct. So, for example, Waismann was allowed to struggle on in his collaboration with Wittgenstein, attempting to produce a readily comprehensible account encompassing both Wittgenstein's *Tractatus* views and the beginnings of what came to be seen as his 'later' philosophy.

Carnap, in "Testability and Meaning", remarks that "Wittgenstein ... requires that every sentence must be completely verifiable", and he cites as source the passage of Waismann's *Erkenntnis* Vol. I article I have
quoted previously. These remarks of Carnap occur in the context of a general discussion of what he terms "molecular languages," which are basically languages whose syntactic devices include sentential (truth functional) connectives, but not unbounded quantifiers. Sentences formed by such devices are termed molecular, an apt extension of the picture on which logic-free sentences are atomic. Carnap's Language I in *Logical Syntax of Language* is of this character; Skolem considered arithmetic restricted to what is expressible in such a language in his 1923 paper "The foundations of elementary arithmetic established by the recursive mode of thought, without the use of apparent variables ranging over infinite domains." The result in that case is primitive recursive arithmetic. And in general, to the extent that application of the primitive predicates is decidable, all statements expressible in such languages will be. Decidability is of course a property of considerable interest in attempts to formulate a principle of verification: "Such a language fulfils the requirements of confirmability and testability in its most radical form. Hence we understand the fact that certain epistemologists, especially positivists, propose or demand a molecular language as the language of science" ("Testability and Meaning," p. 17). So it is that when Carnap has it on authority from Waismann and Schlick that Wittgenstein is demanding complete verifiability he supposes that "we might expect him to acknowledge as legitimate only a molecular language". And indeed, as Carnap cites, proposition 5 of the *Tractatus* ("A proposition is a truth-function of elementary propositions") expresses just that. Neatly as this backward looking exegesis seems to account for Wittgenstein's then current position, there is in it, I think, a fatal flaw. As is notorious, nowhere in the
Tractatus does Wittgenstein indicate explicitly what for him will count as an elementary proposition. He does, however, give certain general conditions which they must fulfill, and notable among these is logical independence, one from another: "4.211 It is a sign of a proposition's being elementary that there can be no elementary proposition contradicting it." But this condition has the effect of ruling out as an interpretation of elementary propositions particular observation sentences (the positivists protocolsätze), for "this is now green" excludes "this is now red" (Wittgenstein, "Some Remarks on Logical Form"). I expect that it is such an interpretation which Carnap had in mind, and that he did not say so explicitly because it so evidently fitted with his whole picture of these matters. Ayer recollected in his recent Oxford lectures on Language, Truth and Logic that the Vienna circle at the time he participated in their discussions (1932) so construed the Tractatus. And further, their characterization of the status of physical laws as expressed in a molecular language, "not as a sentence, but as a rule of inference according to which one molecular sentence (e.g. a prediction about a future event) can be inferred from other ones (e.g. sentences about observed events)," (as Carnap describes Schlick's Wittgenstein influenced view) depends on taking elementary propositions as observational. And if they are not, then the argument for decidability is undone, for though the Tractatus language may be molecular, decidability cannot be established unless it can be established for the elementary propositions. Given the realist conception of truth and meaning embodied in that work, to suppose that they must be entails conjoining disparate, uncongenial bits of philosophy in the manner, and perhaps with the motives of the logical positivists, as I have described in this section.
§4. The incoherence

Verificationism then, combined an anti-realist theory of meaning with the classical, realist conception of truth. I have expounded and faulted the arguments, motivating considerations, and/or assumptions by which such a hybrid (from the point of view of this thesis) conception arose. Still, one may suppose that good arguments could be found. If so, then the whole scheme set out in Chapter 1 as to the interconnection between meaning and conception of truth would be called into question. That danger is now to be allayed in this section by considering examples which demonstrate directly the incoherence arising from the combining of constructivist semantics with a classical conception of truth.

We earlier observed the lack of any prima facie grounds for expecting that meaningful statements, construed as those which are true or false in the transcendent realist sense, are invariably such that the situation in virtue of which they have that truth value is accessible to human experience. Arguments meant to establish this correlation were seen to contain crucial gaps. In particular, there is the lacuna in a passage by Schlick of arguing from the condition that the meaning of a predicate is grounded in the ostensive conditions by which we may determine its application to the claim that sentence meaning consists in the conditions by which we determine the truth value of that sentence. What is missing is an account of how the sentence forming devices of language allow conditions of assertability for a sentence to be given effectively in terms of applicability of its component predicates. There are techniques for sentence formation, however, which in their classic (realist) operation do not admit such an account; unrestricted quantification over moments of time has this character. And
thereby it allows sentences which on a realist viewpoint have a determinate truth value that no possible experience could discover or render probable.

In coming at examples which illustrate and make good these claims I will proceed by way of discussing sentences put forward by Richard Swinburne in his recent article "Confirmability and Factual Meaningfulness" which he claims as decisive counter-examples to the "confirmationist principle". Swinburne expresses that principle as: "a statement $q$ is factually meaningful if and only if either $q$ is itself an observation-statement or there are observation-statements which, if true, would confirm or disconfirm $q$" and he understands by a statement's being 'factually meaningful' that "it describes a logically possible state of affairs, which, it makes sense to suppose, might or might not hold of the world." (p. 71) So he and I aim to hit the same target with an appropriately awkward sentence (though to be sure with rather different views as to the moral pointed in doing so). And in the end it is actually one of Swinburne's sentences that I use. But as I argue, the grounds for taking it as doing the intended job are quite different from those offered by Swinburne. He remarks of his examples: "all assert the existence of a discrepancy between the best available evidence which men will ever obtain about whether some state of affairs $S$ holds and whether or not $S$ holds"; more accurately, they assert the existence of an $S$ for which there is a discrepancy between the best evidence as to $S$ and $S$ obtaining, and where that $S$ is only indirectly accessible to human cognizance. His first example, upon which I shall concentrate, runs as follows:

$$p_1: \text{Among possible claims about the prehuman past which the best evidence ever to be obtained by man makes highly improbable some are nevertheless true.}$$
Given what is wanted of these counter-arguments, it might seem that Swinburne has hit upon the essential ingredients - the remoteness from human experience of S plays on the difficulty of verification, and the assertion of discrepancy between what it established and what is the case utilizes both the hypotheses that assertability is distinct from truth and the criterion of meaningfulness in terms of possessing a truth value. But in fact when these examples are sorted out it is apparent that each of these features is accidental and as utilized by Swinburne constitutes a red herring. What is crucial in the working of the counter-example is the quantification over all moments of time tacit in the phrase "ever to be obtained."

One might wonder then why I choose to consider this argument of Swinburne's in such detail. In answer I would say his errors are interesting ones, and sorting out what is essential to the argument helps clarify a complicated situation. And whatever the faults of his development, Swinburne has, in my view, at least managed to locate the most serious flaw in the verificationist conception.

Swinburne's argument ("Confirmability and Factual Meaningfulness", p. 75) to establish that no observation can count in support of the truth of $p_1$ depends essentially on interpreting the existential quantifier governing by strict constructivist tenets, i.e. it can only be established by demonstrating a particular instantiation. Given his conception of semantics there is no justification whatever for Swinburne insisting on this interpretation. But if the strict constructivist understanding of the logical constants is taken as governing this discourse, then his argument is perfectly valid, and constitutes indeed a demonstration of $\neg p_1$, on the strong constructivist
interpretation of negation as 'it can be established as absurd that we should ever establish that ...'. Swinburne cannot have it both ways.

The continuation of his argument, meant to establish the other half of his thesis (i.e. that no possible observation could count for the falsity of \( p_1 \)), seems exactly of a piece with my construal of the argument against the possibility of any possible observation confirming \( p_1 \) actually establishing \( \neg p_1 \). Since the previous argument has established \( \neg p_1 \) already, and done so by conceptual analysis, rather than any particular experiential observation, it is evident that no actual observation could increase our belief in the falsity of \( p_1 \), which is to say (or with an argument can be brought round to showing that) no actual observation can count for establishing the falsity of \( p_1 \). All this fits neatly with Swinburne's analogical subsumption of \( p_1 \) to the assessment of truth value for "Some bachelors are married." But now comes a twist in what Swinburne wants to conclude from these arguments, where he incoherently (inconsistently) tacks on the conclusion he actually wants to reach: "Yet, it makes sense to suppose that \( p_1 \), unlike 'All bachelors are unmarried', has either truth value." It would wreak complete havoc upon his point were it to be demonstrable that \( p_1 \) is analytically false. Now how can \( p_1 \) differ from "All bachelors are unmarried" on a point so fundamental to present considerations, and still be like it enough to admit of the argument showing that no observational experience could count against it? I think it can't. To make out the crucial difference requires a shift of viewpoint, consideration of \( p_1 \) from a realist viewpoint. But when that is done the previous arguments against observational experience counting for or against \( p_1 \) are no longer valid. Let us attempt to reconstruct the argument
on an entirely realist viewpoint.

From the perspective of realism, in which determinate truth or falsity of a fully specific sentence is a matter indifferent to our perception or establishment of it, we may consider the question (indeed it is an obvious issue on this scheme of things) of the extent of fit or misfit between truth and perception of truth. Now it might be the case either per accidens or by some feature characteristic of a particular domain of discourse that the fit indeed is perfect in that domain. We would describe such a situation by saying this constitutes a decidable domain. For such a domain we actually establish, by whatever means we used to show it decidable, the falsity of a claim of discrepancy between truth and assertability of truth. But what of such assertion for a domain in which we are unable to establish decidability? This presumably is the situation Swinburne means to offer us in his alleged examples of 'factually meaningful' statements which cannot be confirmed or infirmed by observational experience.

Rejecting the anti-realist viewpoint (as Swinburne tacitly makes evident he does) on the basis of which such statements would be (analytically) false, the viewpoint which is left, it seems to me, is one from which one will tend to suppose \( p \) true. The situation will be that encountered in a very great many empirical assertions, and particularly those as \( p \) involving general quantification over all moments of future time that absolute certainty on the basis of experience is to be recognized as unattainable. Indeed, as with assertions which it makes sense to suppose could go either way (Swinburne's crucial claim for \( p \)) we may well find ourselves confronted with evidence pointing either way, and even evidence which
contending arguments seem able to enlist in support of opposite assessments. But this situation is certainly congenial to "weak" verificationism, Swinburne's professed target; it is only problematic for one who would demand conclusive determinability from (possible) observational experience, and whether there have been such and whether if there were the position was held merely through inadvertance of formulation, these are historical questions which need not detain us. (That conclusive determinability from possible experience is a hopelessly over-restrictive criterion of meaningfulness is realized as soon as one reflects on scientific laws and generalizations and the character of their support from empirical induction.)

How might experience count in favor or against statements like \( p_i \)? I will use the notation '\( p_i \)' to denote arbitrary statements of the same general form as \( p_i \), including of course Swinburne's other examples. I have already mentioned that showing decidable the domain of discourse in terms of which \( p_i \) is framed would establish the falsity of \( p_i \). For example, suppose the domain of discourse chemical analysis; in particular we are concerned with statements of the form: "this bit of matter is composed out of the following elements." The complex fabric of observation and theorizing which enables us to assert confidently the atomic theory of matter, the valence theory of chemical bonding, the periodic table of the elements), red shift (for bits of matter far away and receding), the theory and technology of mass spectroscopy, gas and paper chromatography, etc., etc. enable us now to assert confidently the falsity of "Among possible claims about the chemical composition of particular bits of matter which the best evidence ever to be obtained by man makes highly improbable some are nevertheless true."
What now of undecidable domains of discourse, where presumably claims about the prehuman past lie? To begin with, a caution about the word "undecidable": applied to a class of statements, it means that there is no uniform procedure which applied to any particular statement in the class will unfailingly result in the determination of (or perhaps less stringently, merely render probable) that sentence's truth value. It does not mean that faced with a particular statement in the class, we cannot devise a means of obtaining evidence as to its truth value. Using the word in an importantly different sense from the one just delimited, we might want to say of a particular statement that it is presently undecidable, meaning that up till now we have not managed to devise tests for it. Sometimes it appears that various formulations which have been framed of verification as a criterion of meaningfulness would have it that properly there are no statements in this category, i.e. the only statements meaningful to us are those for which we at present possess some means of bringing experience to bear on assessment of their truth value. Such condition would I think make a mockery of everyday experience, where we are faced not infrequently by statements understood but about which we are quite perplexed as to how to establish or refute them, though we may well come to do so. This point will receive further development. Swinburne seeks to convince us, in the argument under present consideration, of the existence of 'factually meaningful' statements which are, as I shall call them 'absolutely' undecidable. That is, not only is no method presently at hand for rendering such a statement likely or not, but we can actually now demonstrate that none ever will be. That Swinburne takes himself to be establishing this of \( p \) is perhaps not quite explicit, but it is evident
I think that he wants to resist any suggestion that $p_1$ is analytically true or false. Coupled with his explicit goal of showing $p_1$ immune to conviction by experience, this comes to the claim that $p_1$ is absolutely undecidable. That there are no such on any strict anti-realist viewpoint is at once evident from the intuitionistic logical validity of $\neg\neg(A\vee\neg A)$. But the issue before us is whether there is such even on realist attributions of meaning. My enterprise here is not the attempt to develop some general argument against the very possibility, but to show simply that Swinburne’s proferred examples are not absolutely undecidable.

$p_1$ is probably true, and two lines of consideration from experience may be adduced in support of this assessment. The first proceeds by direct induction from instances of situations we did not know about, then did come to know, but evidently might not have. The fortuitousness of such a discovery carries with it the genuine possibility of its never being made – hence the likelihood of this happening for at least one possible claim about the prehuman past. It may be countered that the inductive base of experience appealed to in this argument actually supports the opposite conclusion; such indeed was Swinburne’s reply when I put this point in discussion of his Jowett Society paper this past October. Each instance cited by way of data will be a case where potential discrepancy between truth and assertability was averted. Hence our inductive conclusion from reflection on such cases ought to be that truth and assertability will (at least in the long run) always coincide. Now this argument recalls the unwitting anti-realism of Swinburne’s demonstration that $p_1$ does not admit of infirming comparison with experience, but with this difference:
the connection between truth and assertability so established is taken
to be fortuitous. The difference is crucial because it points to an
empirically grounded argument for showing $p_i$ to be false. As such
it is merely the skeleton of a possible argument, and without further
development has something of the plausibility (which I take it is not
much), if not the form of argument, which supposes a monkey at a
typewriter will, if given enough time, produce (by 'accident') the complete
works of Shakespeare. (The element of time in the proper considera-
tion of $p_i$ I will take up below.) As we have seen, establishing de-
cidability of the domain of discourse has as consequence the establish-
ment of $p_i$'s falsity, conclusively to the degree that decidability is.
Considerations such as those just mentioned suggest a means of in-
directly rendering plausible the falsity of $p_i$, not by confronting directly
the whole nature of our knowledge as embodied in a successful and en-
compassing theory, but, where such is lacking, via reflection on the
character of our ad hoc successes. In some cases such instances may
well exhibit a kind of regularity pointing to an underlying, encompassing
theory, the possession of which would indeed establish $\neg p_i$. In others
we may rather more confidently see the indicated domain as nothing so
much as comprised of random facts, indefinitely extensible as to detail,
without promise of general account. Such I think is indeed the case
with "the pre-human past." And there is a less directly inductive
argument (the second of the lines of consideration I mentioned one page
back) which might lend empirical support to conclusions of this kind:
we know something of the scale of the universe, the amount of detail that
can be entered into a description of a most insignificant little corner of it,
we project the likely increase in human knowledge (to the end of mankind — if that is at the end of time, as seems highly unlikely, then this line of argument won't get far) so we can work out that some parts will remain forever unscanned; certain levels of detail about certain situations will never come to human attention. And so \( p_1 \) is rendered to a degree probable.

The situation Swinburne considers has already figured for a time in philosophical discussion in guise of 'the preface paradox': "I am confident in assertion of each claim put forward in this book, but I acknowledge that there are errors in it." How can our modest (or rather perhaps merely prudent) author assert the second of these clauses while in the state described by the first? He manages it (even feels forced to it) by a kind of induction: observing that every book confidently delivered to the press is found in time, with the further expansion and refinement of human knowledge, to have contained error. Why should his work be any different?

The observant reader may at this juncture be bothered by my inattention to a point tacitly put and thus far only tangentially considered: I mentioned Swinburne's claim, given in reply to my argument, which is meant to show that inductive evidence does support the truth of \( p_1 \), that it supports its denial as well, and I rather welcomed the point, taking it that in some cases sentences like \( p_1 \) will indeed be false and arguments of this form will be used to support that determination. But surely this can't have been Swinburne's purpose in rendering that reply, for it would of course entirely undercut his fundamental point. I do not know how he meant his reply to advance his point, and did not press him at the time; for
polemical thoroughness, let me conjecture the argument: "What you say counts as much to the opposite conclusion." By propositional logic, classical or intuitionistic, the falsity of the premise may be established, and so the inutility of establishing \( p \rightarrow q \) (the valid doing of which is not thereby called into question) with the purpose of demonstrating \( q \). But this cannot be what is at issue here, as we are concerned not with deductive but with inductive 'inference.' As is notorious, conflicting hypotheses may be projected by induction from the same data base. Sorting out good from bad, or better from worse projections is a matter for ingenuity and good intuition on the part of the practising scientist, and less pressingly but in a degree the layman in his own sphere of experience too. Providing an account and even one hopes a criterion of this remains a central and crucial problem in philosophy of science. But all such considerations ought to suggest not, I think, that consideration in support of or against \( p_1 \) are not based on observational experience, but that they are and suffer the whole range of problems characteristic of being so based.

We may formalize the \( p_1 \) run as follows: \( \exists p \ (p \in P \land \forall t (\neg t \cdot p) \land p) \). What has been so far left out of account is consideration of that universal quantification ranging over all moments of time. And thereby my discussion heretofore does not take into account the full force of these counter-examples. The claim must be that no collection of experience over a finite range of time fixes, or even renders probable the outcome over unbounded time. This point is reminiscent of Dummett's consideration of "A city will never be built here" in "Truth." (In subsequent discussion this sentence will sometimes be denoted by 'b' and a statement instance
in which the reference of 'here' has been specified as \( r \) by 'b'. Dummett, in developing the anti-realist conception of truth wants to exhibit a statement which we are not entitled to say is either true or false (there is something in virtue of which it is true or it is false) because we lack the capacity to bring ourselves in finite time to a position where we are justified in asserting it or denying it. Swinburne's goal, with respect to the kind of sentence he considers, is the same, except that his thoroughly realist conception of truth does not lead him to conclude from its being undecidable that it is neither true nor false. The issue we face here is as follows: on a realist conception of truth, does the linguistic device of quantification over all moments of time introduce in its wake (at least sometimes) absolute undecidability? Swinburne claims so, and so too Dummett, I take it. Before attacking this issue directly, it is worth reiterating at this juncture that the question raised has the prospect of a positive answer only on some kind of realist viewpoint. For in espousing anti-realism anything which can be adduced to show absolute undecidability on a realist conception contributes to deciding the sentence in the negative. Oddly, Dummett's (admittedly brief) discussion in "Truth" of "A city will never be built here" leaves this point quite out of account, with the result that the conception he seeks to illuminate with it must be akin merely to constructivism within the context of classical mathematics, a different beast altogether from the anti-realism of Brouwer's mathematical intuitionism (which difference Dummett himself has expounded in lectures on intuitionist mathematics). Indeed, developing the example anti-realistically presents an instance within the domain of empirical discourse which refutes Markov's principle, a principle not valid for intuitionist
logic, but seemingly unexceptionable on a less radical account of constructivism. But of this last point, more must be said in other places.

As is apparent, there are a variety of situations in which $p_i$ and $b_r$ are decided. And evidently these cases are not exhaustive. What of situations where the domain of knowledge lacks a comprehensive theory with full scope for subsuming experiments in which case $p_i$ is falsified, as we have observed, or the spot is not so clearly uninhabitable, or in any case uninviting (e.g. $r = \text{the North Pole}$) that practicalities and psychology of preference unite to render $b_r$ true (or in any case probable - there is of course the problem in this example of long term changes in weather patterns). There is the possibility in such cases that while the point at issue itself in these examples can not be settled directly, nonetheless the end of human agency can be established, or shown highly probable, which would have the effect of removing the element of unsurveyable infinity from the examples. But it might be that no such end occurs, or in any case that circumstances are such that no prognostication of it can be supported by present observations and theory. The prospect then is that happenstance at instances of time unfold to plain view, without giving us in the process any hint as to what future cases will be like in the long run. Future happening stands entirely independent of present tendency. The realistically minded empiricist may find the $b_r$ examples slightly less worrying than the $p_i$ ones insofar as $b_r$ remains open always to conclusive refutation (the appearance of contractors with plans, promotional literature and earth moving equipment would do it). But $p_i$ might be such that whether it were true or whether it were false, no
evidence could ever turn up in time to indicate. This difference reflects the fact that \( b \) are purely universal statements, while the \( p \) contain a universal quantification (over time) within the scope of an existential quantifier (over statements). Only a god's eye view, outside time can survey the relevant facts. I tried earlier on to suggest ungodly considerations which might count - such as the presence of error in all conclusions of human research, subsequently refined out, but with the refinement then subject to its own law of error. But whether such considerations could tell us anything \textit{sub speciae aeternae} seems dubious - if we are betting better all the time, maybe in that long run we will finally get it all right - there is just no telling. We must admit that tortured though the surrounding conditions must be, there is no gainsaying the claim that there are instances of statements of a particular form such that no evidence we ever obtain will count to establishing their truth values. Whether we can actually exhibit and establish as such a particular fully specific absolutely undecidable statement seems to me problematic, but as we are operating in a realist frame of mind we accept the demonstration of an uninstantiated existential truth.

By way of conclusion then, what ought be the response of the realist minded logical positivist? Two possibilities immediately suggest themselves. The first is to embrace anti-realism. The second is to declare the example sentences meaningless, as is meant to be the fate of all absolutely undecidable sentences according to the principle of verification. But while the positivists were only too happy at so ruling in the case of e.g. "Imperceptible deities are all about us," (that was the whole point of the criterion) a discrepancy between truth and assertability (or in any
case their not being the same thing) is at the very base of a realist conception, a conception they uncritically embraced. "The difference between the two concepts 'true' and 'confirmed' ('verified', 'scientifically accepted') is important and yet frequently not sufficiently recognized"; so Carnap opens his relatively late (1936) essay "Truth and Confirmation."

So the positivists are not likely to have wanted to take this way with $p_1$. And less specialized considerations apply to like effect with respect to $b_r$: one who, on surveying a lovely and unspoilt track of wilderness remarks "I hope that a city will never be built here," must be counted as operating well within the canon of meaningful discourse. But if one can meaningfully hope that $q$ evidently $q$ must have meaning, and the agent whose propositional attitude toward $q$ is one of hoping must grasp that meaning. How then in the face of these examples might the logical positivist remain realist and yet allow them as meaningful, while maintaining the principle of verification (in some form or other) as a touchstone of meaningfulness?

I can think of a third response, which would attempt exploiting the observation that it is forms of words which are endowed with sense, or lack of it; the meaning of a sentence does not change from occasion to occasion of use. "A city will never be built here," means the same whatever location is ostended with 'here'; there will be variations as to truth value, but meaning and meaningfulness remain constant. So it is also for "Among sentences of this domain of discourse there is one such that the best evidence ever to be obtained will render it improbable, but nevertheless it is true", for various references picked out by 'this domain'. What this observation suggests is a scheme on which a
verification account of meaning applies only to certain statements. Statements for which no verificational content can be assigned are allowed to have meaning in virtue of being grammatically of the same form as ones admitting of such assignment. On first consideration, this is an ad hoc retreat in face of the Dummett and Swinburne examples, admitting them to be counter-examples, but attempting to limit the scope of their impact. Is this move to be compared with trying to continue work in a formal system demonstrated inconsistent by promising not to use contradictions for easy proofs? Or is it rather the beginnings of a reformulated and consistent system? Without further development it might, I think, be either. But either way p₁ and bᵣ demonstrate incoherence in the positivist program of semantics as actually formulated. I shall have more to say concerning non-uniform accounts of meaning in the next section, and the issue presents itself again in Chapter 4, where I consider Quine's conception of language.

§ 5. The legacy of verificationism for anti-realist semantics

We have seen the untenability of founding verificationist semantics on a realist (classical) conception of truth. So if a semantic theory of this character is to be maintained, we know that truth must be taken as an anti-realist concept. But of course so taking it merely removes an impediment to anti-realist semantics; achieving such a theory is evidently a program of considerable dimension. Many of the difficulties which beset verificationism remain, perhaps in transposed form, after the doctrine has been purged of the incoherence attributable to its realist element, and these then are problems for anti-realism -

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presumably of less fundamental character than the one we have lately considered but problems nonetheless and potentially fatal at that. This final section of the chapter on verificationism consists in a programmatic sketch of a few of these.

Verificationism constituted an attempt to provide a general account of what it is for sentences to have meaning, solely in terms of their epistemological status within the dichotomy "determinable from rules of language alone" and "requiring appeal to (non-linguistic) experience in its assessment"; notwithstanding, positivists (the better to understand their own theory, as well as responding on occasion to hostile challenge) attempted specific elaboration of their account with respect to particular ranges of empirical discourse. Often singled out in this way are psychological statements and (objective) statements about the (remote) past. *Prima facie* these two classes of statements are of unrelated character, and indeed subsequently I will consider, if only to cast doubts upon it, an argument to the effect that while the former is to be treated anti-realistically, the latter range of discourse is endowed with realist meaning. Different though they may seem, the two domains present an analogous and serious problem for any program of anti-realist semantics: the corresponding present and past tensed statement on the one hand and corresponding first and third person on the other seem respectively to make the same assertion, to mean the same thing. They must at least be seen to agree as to truth value, e.g. "He crossed the river" is true as asserted now just in case "He is now crossing the river" is true as asserted then, and comparably for "D. I. is happy" said by you and "I am happy" said by me. But if identity of sense is
tantamount to identity of truth conditions, then this truth-value link (to use Dummett's apt phrase from "The Reality of the Past") actually dictates preservation of meaning under transformation of tense or person. The difficulty for verificationism arises in that such shift in perspective moves epistemologically from what is directly accessible to what is only indirectly accessible and even, on occasion, inaccessible. As anti-realism is the attempt to ground meaning in epistemic conditions the problem of these two ranges of discourse may well be taken as a touchstone for the success of proposals in this program. I will not say anything more about this matter directly, though it will figure incidentally in subsequent discussion.

I want now to deal briefly with broad issues which arise as soon as one tries to get beyond the hopelessly general, vague and inaccurate slogan "the meaning of a statement is its method of verification." There are serious and substantive issues in such attempts sometimes difficult to sort out from terminological infelicity and confusion. To illustrate, take this formulation by Ayer from Language, Truth and Logic (p. 35): "A sentence is factually significant to any given person if, and only if, he knows how to verify the proposition which it purports to express - that is, if he knows what observations would lead him, under certain conditions, to accept the proposition as being true, or reject it as being false." The use of the word "verify" in this context is an instance of confusion, or at any rate misuse of terminology pervasive in positivist literature. "Verify" means something like "establish as true." But of course false statements are factually significant as well, a point no positivist ever wished to deny, indeed as we have seen, would assert.
In very many contexts this difficulty would be obviated by using the word "decide" in place of "verify". Evidently this is the case with Ayer's formulation just quoted, as shown by the paraphrasis introduced with the words "'that is'". This emendation brings us to what is either a confusion in formulation, or a serious mistake. That it would be a mistake is clear if we consider the status of a scientific hypothesis — for when entertained but as yet untested (let alone decided), indeed at the stage where the project at hand is to devise experiments and/or observation which will count in its establishment or refutation, it must be already understood (its meaning grasped). This might have been the order of things with e.g. the theory that diamonds result from the same geological process which produces coal (plausible hypothesis on noting they are both pure carbon, but an irrational hypothesis to the same effect would have been as well understood). The quoted passage from Language, Truth and Logic may stand convicted not on the maximum charge of error (the error of identifying the capacity to decide the truth or falsity of a sentence with understanding it) but just on the lesser of confusion in formulation. Support for this plea bargaining may be forthcoming in its continuation: "If on the other hand, the putative proposition is of such a character that the assumption of its truth, or falsehood, is consistent with any assumption whatsoever concerning the nature of his future experience, then, as far as he is concerned, it is, if not a tautology, a mere pseudo-proposition." For between being in a position actually to decide a statement and the situation described "on the other hand" there is the possibility of an account of sentential understanding not in terms of knowing what techniques would
support a decision on that sentence but rather in terms of the capacity
to recognize such techniques. Such an account could be modelled
on that offered by Brouwer in the domain of mathematics. Accordingly,
while at present we do not know what mathematical experiences would
result in establishing e.g. Fermat's last theorem as true, nor experi­
ences which would establish it as false, we manifest our understanding
of that statement in the capacity to recognize of any particular bit of
mathematical experience whether it serves to establish, to refute,
or leaves undecided that statement. We might ask then in what this
capacity consists. The general answer is that it is a matter of grasping
the conditions of application of the primitive terms of language (as in
the three place arithmetical relation $x+y = z$) and in knowing how in the
case of a complex sentence built up from these by such devices as
quantification and sentential connection, the assertability of the whole is
determined by assertability conditions for the components. It may be
instructive to consider the particular answer in case of e.g. Fermat's
last theorem (call it 'F'): $\forall n \forall x \forall y \forall z \; (n>2 \rightarrow x^n + y^n \neq z^n)$. A
rather specific account as to the kind of experience which would establish
F as false can be given: it will consist in having in hand four natural
numbers of which the first is seen to be greater than 2, raising each
of the others to that power, summing the results of the first two of
these calculations and finding on comparison that the result of this
calculation agrees with the third exponentiation. What about mathema­
tical experience which would establish F as true? Here we cannot
be anywhere near so specific; in place of a particular ordered quadruple
of natural numbers we must now have a composite and rather abstract
kind of thing: a procedure which, for any quadruple of natural numbers having its first element > 2 produces a demonstration that the appropriate inequality holds, and a demonstration (valid by the canons of intuitionist mathematics) that the procedure does this. We say much as to what this might be like by delimiting the elements of mathematical experience (which have intuitionistic content), but we cannot in advance specify much detail as to the form in which those elements might come together to produce the verifying demonstration. We manifest our understanding of F in being able to say of any combination of such elements whether it is such a verifying demonstration - we will recognize one if we have got it. From this account three levels of evidence might usefully be distinguished, or rather we must distinguish between evidence and two levels of description of evidence. At the first remove from evidence itself come descriptions fully specific save for parameters. The account of evidence which would establish F as false is of this character. At the second level comes description in terms of intended effect and allowed techniques. Evidence for the truth of F was set out in this degree of abstractness. Now it would be plainly absurd to identify the meaning of S with actual evidence which decides S, for of course plenty of testable statements may remain untested. But I do think that many accounts of verificationism while they do not intend this absurdity (contrary on occasion to literal reading), do suppose the adequacy of an account at the first level of description. I.e. they require that but for the specification of parameters tying the observation to the particular occasions relevant to a particular use of S, a complete account of the observational evidence which would test S can be set down and is tantamount to the meaning of S. So I construe Ayer
when he requires that one who would understand S "know what observa-
tions would lead him, under certain conditions, to accept the proposition
as being true, or reject it as being false." (loc. cit.) But any such account
will be far too restrictive and inflexible. Take the sentence "A tree once
grew at this spot." In a large class of cases the matter can be settled
by an appropriate instance of the observation "Here is a tree stump."
But in others it will be "This old map shows a dense forest over this
region" (you would never believe it to look now at this monster parking
lot); still others "This rock is petrified wood" and so on, to far more
indirect (and in general probabilistic) evidential relations: observations
which support theories which account for techniques (e.g. mass
spectroscopy) by which we make observations which support a theory which
has as consequence that it is highly likely that a tree once grew at this
spot. If my understanding of this sentence is to consist in my knowing
what observations would support it, that knowledge must be a capacity
to decide, of observations mediated as evidence by even as yet undreamed
of connections, that they are confirmatory or infirmatory, or irrelevant as
the case may be. And an account of this capacity will necessarily be
phrased in what has just been termed second level description of evi-
dence. Now earlier I suggested that such an analysis in terms of capacities
might be compatible with Ayer's account of factual significance, on the basis
of possibilities opened out in his continuation (quoted p. 109) of the passage
on which I initially focused attention. What is laid down there in effect
is the criterion that S is factually meaningful for someone just in case he
knows that were S true, it could make a difference to his future experience.
This condition is seemingly inequivalent to, and less restrictive
than the initial formulation. For his knowing that it could make a difference does not on the face of it require him to be able to say here and now what that difference would be. Possessed of the capacity to decide of impinging experience its relevance to establishing $S$, he knows that future experience, if $S$ were actually true, might reflect that fact by involving experiences which support $S$. I am reasonably certain that this is not the interpretation which Ayer intended to have put upon these lines, firstly of course, because it would render them inconsistent with the immediately preceding and secondly because those lines when interpreted consistently with the preceding ones express the intuition which it seems to have been Ayer's constant aim to express in various formulations and reformulations of the principle of confirmation. The intuition seems to be that we know a (non-analytic) statement to be confirmable and hence significant just in case we can show that it would if true make some difference to experience, and the way that is done is by exhibiting such a (potential) difference. The sorry history of attempts to express this idea precisely in some sort of formalization is well rehearsed by Scheffler in *The Anatomy of Inquiry* pp. 150-154. He there also gives quite general grounds for not expecting success through further amendment and specifically points out the vacuousness of the then most recent attempt, by Nidditch. All this rather suggests that while this might not be the intended interpretation of the lines discussed, there is need for a different one in any case.

A feature of the previous discussion may have impressed the reader as ironic, a bit of role reversal. It might appear that Ayer is here on record as demanding a strong, constructivist interpretation of an existential
quantification, and I cavalierly allowing assertion without instantiation. Understanding a sentence, on his view, requires being able to say in advance what experience would test it; I urge merely the capacity to assess such experience when, and if, it turns up. The point is a vague but suggestive one, and I have for it an answer which is vague and, I hope, suggestive. I think the position I adopt here is fully consonant with the tenets of anti-realism, and that it is dictated to anyone who would espouse verificationist type semantics by facts which must be agreed to by realist and anti-realist alike. The point is that the possible forms of experience are not only infinitely variable in a fairly literal sense, they are indenumerable, not in the sense that there are $\aleph_i$ for $i > 0$ of them, but they are in their variety and interconnections indefinitely extensible. We can not run through them not because we are barred from ever finishing an infinite list, but because we have no systematic procedure by which we can order them into a list such that anyone of them will be encountered by going sufficiently far along it. Swinburne must be on to this point when he claims (p. 73):

we hardly have before us a catalogue of types of observation-statements which we can run over quickly to see whether they have any confirmation relations to some given statement. So we may easily make a mistake in concluding that a statement p is not confirmable or disconfirmable by any observation-statement through not having thought of a certain observation-statement q, which does in fact confirm (or disconfirm) p.

But constructivist semantics demands no such a priori list (a point of course not contended by Swinburne); the intuition that the meaning of a sentence is grounded in the experiences relevant to establishing or refuting it finds ample realization in the capacity we do have for assessing those sentences in light of experience.
Not all positivist formulations of verificationism appear to commit the whole range of sins I have been here belaboring. Thus Carnap in his relatively early (1927) statement on these matters "Pseudoproblems in Philosophy" writes:

...there can be a difference of opinion about those statements which are neither testable nor have so far been supported ... the customary method of the empirical science, including physics, does not consider statements of this kind as meaningless, but admits them either as hypotheses, preliminary conjectures, or at least as statements that permit the formulation of certain problems. (p. 328)

Sensitive to this issue, Carnap allows that "we [meaning 'I'] consider statements meaningful even if they merely have factual content, but are neither supported nor testable." But then Carnap takes it that a statement has factual content when "we can think and describe the characteristics of an experience through which this statement would be supported." And here I think his formulation is over-restrictive in much the way that the second part of the Ayer passage proved to be. Carnap's account will fit e.g. the then current status of "There is a mountain of a height of 3000 meters on the other side of the moon" [an example popular in positivist literature - cf. Schlick "Positivism and Realism" p. 88] for while (at that time) no way of testing the assertion was at hand, we could describe the experience, merely technically unfeasible, of going to the moon and carrying out the appropriate measurements. But it will not work generally for the reasons already indicated in the case of Ayer's formulation, and I add here the further counter-example of "The center of the sun is at a temperature in excess of $8 \times 10^5 \text{ K}$", by form even an
observation sentence but which requires for its support a complex scientific theory which might well have been lacking at the time it was first advanced.

Finally, I observe that Carnap manifests the same "misuse of terminology", slip in formulation, or confusion the pointing out of which began my discussion of Ayer: "If it is impossible, not only for the moment, but in principle, to find an experience which will support a given statement then that statement does not have factual content." (p. 327) The point was that false statements are, though not verifiable or supportable, factually meaningful. But I also then remarked in introducing that point the difficulty in sorting out substantive issues from terminological infelicity. Now the following substantive point may support the aptness of formulation: the positivists took the doctrine of verificationism to apply to non-analytic statements - and these it is argued, or taken for granted, are precisely those which are contingent, i.e. could have turned out either way. So even if S is actually false, it makes sense to suppose (its meaning is such that) it could have been true. And to suppose it might have been true is to envisage ways in which we could establish it as true, i.e. verify or support it. Now this formulation bears substantial similarity to the intuitionist account of sentence sense in terms of how it is to be established (leaving aside the feature of the systematic functioning of language). It might be thought to count against this apparent consonance that this positivist account seems to turn essentially on the notion that S is contingent, it might have turned out true. Whereas of course mathematical statements, even for intuitionists, are such that if false, they could not have been true. But the distinction as to sentence status
which is of importance to this discussion is the epistemic, not the metaphysical one (as Kripke uses these terms). The problem for attempts to provide an account of meaning in terms of grounds for assertion is how properly to deal with undecided statements, statements which for all we now know might turn out either way. Statements of mathematics and statements of geology alike may share this character. What we, as anti-realists, want to say in such cases is (roughly) that understanding these statements is knowing how experience might lead to establishing such currently undecided statements as true. In such broad outline I think verificationism and intuitionism are in accord. But there remains the difference earlier noted between the kind of description (one in terms of parameterized observation sentences, the other in terms of capacity to assess experience) these two would offer as to possible future experience in rendering an account of sentence meaning. I earlier suggested that this difference might result from an unwarranted modelling of their semantics by verificationists on the account possible only in the case of decidable statements (statements perhaps not actually decided, but with respect to which we know of techniques such that when applied, as we might choose to, would result in evidence for or against). And as such then it manifests a kind of error. But it may rather, or also reflect the following serious and general problem for any anti-realist semantics with application to natural languages. It comes about through the existence of linguistic devices for deixis, and the attendant distinction between sentences and statements, in other words context dependence. For certain purposes the matter is easily handled by putting the context explicitly into the sentence, the resulting "eternal sentence" doing
adequate duty for statement (in the sense of what was stated). But here we cannot, I think, help ourselves to these devices. The difficulty is that meaning attaches to sentences, and it is statements which are true (or false). Thus immediately when we attempt to give an account of sentence meaning in terms of conditions for asserting as true there is prima facie reason to expect misfit. Thus on the basis of my command of the English language I already understand "A tree grew on this spot then", but this form of words cannot itself be established as true or false; rather we must wait for a particular occasion of use, and then looking at the situation, an assessment is in order. But if the meaning of the sentence is to be given in terms of how such an assessment would be made, then we must be able to give in advance of particular ones a uniform account of that process. The most natural idea to try might be to model this account of that given as to how this form of words (sentence) can be applied on particular occasions to make a statement. That is done by specification of values for the parameters 'this' and 'then'. Parametric variation is, to be sure, the simplest kind of uniform account possible. The attempt to apply it to an account of how evidence would support a particular statement leads to the kind of first level description of evidence which I earlier detailed and criticized. Sticking to our current example, in very many cases "Here is a tree stump" filled in as to parametric value comparably with that in "A tree grew on this spot then" provides the desired evidential account, or at any rate could. But as 'this spot' varies over odd corners, or 'then' gets realized as a very long way back, or just the evidence for the tree having been where this parking lot now offends must be indirect, the simple parametric account
proves entirely inadequate. The link between evidence and statement cannot have the simple uniformity of that between statement and sentence.

The difficulty singled out here is not one with which Brouwer's account of meaning in the domain of mathematical statements had to deal since in mathematics sentences are most naturally to be taken as the bearers of meaning. But I did suggest in my initial discussion of these matters a few pages back that the difficulties might be got at by appeal to the intuitionist account of meaning in terms of capacity to assess evidence, though without bringing out the fact that this would itself constitute application of the notion in a way rather different from that in the mathematical case. C.I. Lewis in his important critical assessment of verificationism "Experience and Meaning" seems to come rather close to formulating the point that empirical meaningfulness of S for an individual must consist in his capacity to recognize experiences which would tend to confirm S, but not necessarily (and indeed in general not) in knowing in advance what those experiences might actually consist in. Rather aphoristically he remarks that "in making any verification we expect something which we cannot anticipate." (p. 138)

And then in more detail:

If those who believe in the electron as a sort of ultra microscopic bullet cannot envisage this object of their belief in such wise that they would be able to recognize certain empirical eventualities as the verification of it, in case the conditions of such verification could be met, then they deceive themselves and are talking nonsense. But if they can thus envisage what they believe in, then the fact that such verifying experience is highly improbable, and even that the details of it must be left somewhat indefinite, is no bar to its meaningfulness. (p. 140)
But it may be that even the capacity for assessment of evidence is too restrictive an account of empirical meaning. How good do I have to be at sorting out putative confirmatory experiment and observation? Take \( S = "A \text{ grove of trees grew at this spot 2,000,000 years ago.}" \) I am presented with carbon-14 tests, geological surveys and the like. Does my understanding of \( S \) consist in realizing the significance of this data? Not very plausibly. I seem to understand \( S \), though lack of a science background leaves me quite at sea with this preferred evidence. But perhaps my understanding of \( S \) means that I must be able to be brought to see the relevance of this relevant data. Well, I might be hopeless at science, have no head for theoretical or quantitative matters - what carbon-14 has to do with anything always remains for me a mystery. Surely this is no bar to an understanding of \( S \). In what then does such understanding consist? A plausible account can be rendered in terms of analogy. I have seen groves of trees in various places and realized that it was in that case proper to say "there is a grove of trees", and then I have come back to that spot and found them gone (under a developer's bulldozer, say) and took these observations to justify the assertion \( S^1 \) "A grove of trees stood at this spot two years ago."

Then in the case of \( S \) my understanding consists in the realization that it asserts something exactly analogous to the situation described by \( S^1 \), and where \( S^1 \) is understood by knowing the kinds of experience I might-have which would show that situation to obtain. So we have it then that \( S \) is understood by virtue of knowing observations which would establish a situation of that kind as obtaining, but not in virtue of the of the observations which would show that situation to obtain. C.I. Lewis,
in the article just cited, notes this pressure toward grounding meaning in analogical verification, but there is, it will be noted, a difference between his sketch and the one I have just given. "Perhaps the chief requirements ought to be that we should be able to analyse the supposed connection between the projected verifying experience and what is actually given (the 'rule of operation') in such wise that this procedure of verification can be envisaged in analogy with operations which can actually be carried out." (p. 139 op. cit. Cf. also Hempel "The Empiricist Criterion of Meaning" p. 110 and esp. fn 5). What I have tried to do in my account is avoid the suggestion explicit in these remarks of Lewis that e.g. S could be verified by processes analogical with those which do the job in the case of S¹ and that my understanding of S is in terms of those analogical processes: if I had been there 2,000,000 years ago I would have observed a grove of trees. Such appeal to counterfactuals seems to me in certain ways alien to the guiding intuitions of constructivist semantics, and it is in any case problematic whether on even the most liberal standard we know quite how to attach definite sense to them. But I have to admit that if I want to avoid using counterfactuals in this sort of way, desirable though that may be, a lacuna in my account is thereby created, for it seems some account must be given as to how knowledge as to observation which would support S¹ endows S itself with meaning. In what way do S and S¹ describe relevantly similar situations (despite the evidently vast difference as to their epistemological character), or more to the point, in virtue of what are we allowed to take it that the meaning of S is parasitic on that of S¹? Here I want to appeal to a notion of 'the most immediate
and central uses of language; they are those uses by which our immediate experiences can be described. That in the process of language acquisition our linguistic competence is first manifest in this range of discourse seems nearly indisputable; the child's early uses of language are in describing and dealing with immediate experience. There are those indeed who do not feel themselves particularly pushed in later life to go much beyond that, and they are nonetheless in full possession of linguistic competence, even if parochial as to interests. Those, whose concerns do come to encompass a wider range than what presently goes on about them and what they remember, bring to bear in learning of and considering these remote matters a linguistic competence already fully developed.

An explanation seems to be called for as to how this linguistic competence is seen to be applicable to ranges of experience beyond those in terms of which it is grounded. Extrapolation of parameters may be called upon as the device to explain, in part, this extension of application. Experience teaches us how to test and support statements about last year and the year before that, and so from the understanding of sentences about the present we come to understand sentences about the immediate (remembered) past. But once sentences of that sort are understood, sense may be attached also to the assertion of a description as applicable to a situation a hundred years back: it is the same use of language in asserting of that situation a hundred years ago that it is such and such as when we assert such and such of a situation two years in the past. But how, it may be demanded, do we know that it is the same use of language? Here is where we help ourselves by appeal to analogy: saying this about 200 years ago is like saying it about 2 years back. But whether this
constitutes an explanation with which we can rest content I am not fully confident. This unease may reflect an entirely general feature as to explanation by analogy. But at least in the preceding account of verificationist meaning applied to e.g. statements of the remote past in terms of central cases of language use, I have managed, or in any case it has been my objective, to limit the application of analogy just to the last question and avoid applying it to the whole notion of evidential experience.

From the constructivist point of view appeal to analogy and "in principle" possibility in the explanation is, at least on particular occasion, a matter of dubiety, a source of sense of error and unrealized confusion. Thus the platonist minded mathematician answers the challenge as to how we mere mortals form a conception of the infinite totality of the natural number sequence, and beyond that of the set of all its subsets by remarks such as: "you know what it is like to count (finite) sets of arbitrary size. Well just imagine that you carry out that process, speeding up as you go, say each step twice as fast as the previous, and don’t stop when you get to any particular number; as you can plainly see if you took a minute getting from 0 to 1, at the end of the second minute you will have gone through the whole lot – a mere medical impossibility we can’t speed up that much (we can’t run the 2 minute mile either) – otherwise it is just like counting finite sets. And now that we have got the natural numbers, well the continuum is just a matter of taking all the subsets of it, and you know what that is like. For example, if we have a set M with five objects then there are $2^5 = 32$ ways of deciding independently of each element whether to put it in or leave it out of a set we form from the elements of M." This platonist patter is an attempt to
account for "in principle" possibility (e.g. counting the set of natural numbers, forming its power set by analogy (with e.g. counting and forming the power set of finite collections). And while the intuitionist mathematician will decry the cogency of what ostensibly has been explained, he cannot do it by faulting the very technique used in the explanation, for he has on occasion himself to resort through analogy to possibility in principle, this on pain of being pushed into extreme and implausible if not incoherent forms of constructivism of the strict finitist sort. For the intuitionist certainly wishes to maintain that the natural number property of being prime is in virtue of a simple algorithmic process decidable, i.e. $\forall n \left( \text{Prime}(n) \lor \neg \text{Prime}(n) \right)$. But simple and effective (in the sense of 'always' working) though this process is, there comes a point as $n$ gets very large where human beings, even with the aid of the fastest computers and all the paper supply on this planet, could not actually perform the calculation. Nonetheless it is in principle possible to do it; it is just like what we do with e.g. 23 only more complicated. We may wonder as to how stable a position intuitionism holds, attacking on one side classical mathematics for illicitly accounting meaning by appeal to analogically explained possibility in principle while on the other resisting rather similar sounding onslaught from strict finitism. This question I will not discuss here. But the issue which does concern me at this point is basically the same question transposed to empirical constructivist semantics i.e. verificationism. Does the sketch I have given allow enough use of analogy - but not too much?

Enough is, as we have seen, to allow knowledge of meaning without substantial training in the sciences, or even a good head for being
taught. Enough is also to avoid another, or perhaps rather it should be said further kind of over-restrictiveness in the account - this one akin to mathematical strict finitism in grounding the meaning of a statement in the specific techniques, humanly and actually performable by which it would be established. The former difficulty arose in the appeal to capacity for recognizing evidence which was invoked as a defense against being pushed to the claim that only decidable statements (those for which we now know of techniques which would in principle help to settle them) are understood. The strict finitist over-restrictiveness is actually a more extreme case of this problem of limitation to decidability. As the use of analogy was introduced in the preceding discussion with the purpose of being enough to overcome these problems of excessive narrowness in formulation, I will leave that flank and now consider whether as so introduced we have not too much analogy.

That assessment will be by way of considering what is to be said of ancient Greek or Lucretian formulations of atomism. Broadly two possible views may be taken: a) they did not know what they were talking about, as the theory was in no way grounded in any knowledge as to what experiences would tend to establish it, or b) they were talking about what, at least in some details, was later established - and they knew what they were saying, even if there was no way to show it. Of course a) is roughly the view of one imbued with anti-realist intuitions, b) the expression of a realist outlook. What I propose to do is set out first the role which analogy plays in the justification of b) and then show that the use of analogy previously introduced into my account of anti-realist semantics is not such as allows playing that role.
But before doing that, a comment on the claim that a) is the expression of anti-realist intuitions: I earlier remarked that positivists wasted the insight into language potentially afforded them by reflection on the character of anti-realist semantics by using the principle of verification merely as a polemical tool to denigrate philosophical views uncongenial to them. And I contrasted this situation with Brouwer's reconstruction of mathematics whereby the statements of classical mathematicians were not nonsensical, but merely endowed systematically with meanings different from those they had supposed they understood. But it looks in a) as if the prima facie anti-realist attitude there is polemical in the style of the positivists and not reconstructive in the manner of Brouwer. The answer is that the situation with ancient Greek atomism is to be compared with that of analysis in mathematics prior to the advent of Brouwer's theory of spreads, bar induction, the uniform continuity theorem, etc. At that time a destructive critique of classical mathematics was already frameable from an anti-realist perspective, even if what to put in its place was not then yet discovered and developed. Brouwer's later development of the intuitionistic continuum is comparable in terms of the present example to the modern discovery of atomic theory.

Let me now give the realist account by which Democritus may be said to have understood, as we do now, talk of atoms: by analogy with divisibility of perceptible objects (e.g. breaking stones) we may suppose this process carried on indefinitely till the objects being divided are so small as to be no longer perceptible (of course we can not do this, but it can be conceived of); either this process goes on without termination,
in which case matter is continuous, or it terminates. In the latter case there are indivisible imperceptibles out of which all else is composed. These are the atoms. By this account we know what is asserted in the claim "there are atoms". Now of course actually to establish that there are atoms requires adducing grounds for divisibility of the imperceptible and for the termination of the process. But we do not need to do so, or even to say what it would be like to do so to claim understanding of the atomic theory; that is fully accomplished by the account just given. "There are atoms" describes an objective situation which it makes sense to suppose does or does not obtain; and as it happens 2,000 years or so after the idea was first mooted evidence was actually adduced to show that it does.

Now is the anti-realist pushed to say these sorts of things, so unacceptable by the guiding lights of his position if he avails himself of the use of analogy I suggested? Recall that this limited use is intended to provide a constructivist account of meaning applicable to e.g. statements about the distant past couched neither in terms of the complex and indirect sorts of evidence by which such statements might be established, nor in terms of the capacity to recognize or assess such putative evidence or be taught to do so. First I want to argue that if we allow the use of analogy in the account of evidence of the sort envisaged in passages we have considered from Lewis, Carnap and Hempel the realist account of meaningfulness will ostensibly be backed by knowledge of assertability conditions. For one might appeal to direct observations which could be made if only we were very, very small. Of course we can not actually make such observations, but it is in principle possible that we should observe these tiny little
particles and discover by inspection that they cannot be split any smaller. Of course it is allowed that actually to establish this will require a complex interaction of theory and indirect observation. It is no good I think protesting against this account that it is not in principle possible to make these observations, say on grounds that any observing being and in particular its sensory apparatus would have to be made of lots of atoms and so would be too big to interact sensibly with them individually, not if one is going to account the meaning of "the temperature at the center of the sun is in excess of $8 \times 10^5$°K" in terms of the in principle possibility of direct observation by a suitably placed observer.

Now I come on to claim that the analogical extension of language from its semantically central applications would not of itself sanction the early Greeks in their talk of atoms. The point is simply that what is asserted of remote situations (in this case the imperceptibly small) is not such that with that form of words we make the same assertions when used with reference to accessible and sensible situations. Indeed, it is in the very character of the putative theory that what is claimed is something we could never claim or have experience of with respect to everyday experience of objects.

I fear that these brief remarks paper over substantial unclarified issues and confusions, and may be merely an attempt to do justice to intuitions which are more in the character of prejudice than insight. Even so, there seems to me something important in distinguishing between central and parasitic uses of language. It may be that in the preceding attempt to apply this distinction I tacitly claim that analogical extensions

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of language use beyond the range of accessible experience can happen only by translation of reference in space or in time. And possibly this would constitute too restrictive an account. Is it to be taken that primary uses of language as in "that object is too small to be seen at 100 meters", "that object is too small to be seen at 1 meter", etc. gives sense by analogical extension in a process of going to the limit to "that object is too small to be seen (at all)"? My inclination is, from an anti-realist perspective, to disallow such limiting processes as constituting grounds for attaching sense to sentential combinations of words. Rather it seems to me that such sentences do require a direct account of experiences which would support them, if they are to function properly in the interlocking scheme of meaningful discourse. The pressure not to require a direct account as in "a grove of trees once covered this spot" does not seem to me present in this case.

Early in the discussion of problems left for anti-realism from a purified verificationism, I mentioned the need to allow for entertaining a hypothesis in advance of knowledge as to means of testing or establishing it. It might be thought that the treatment just accorded ancient atomism is not consonant with that desideratum. But though ancient atomism is denied the status of hypothesis, there are other statements which on that account can be so taken. "The dinosaur whose fossilized skeleton we have before us" seems to me such hypothesis allowable on my account of analogical extensions of language, and so too the theory of continental drift. These statements can indeed be understood by one possessed of an ordinary linguistic competence. But science, I think it will be readily agreed, comes in grades of abstraction, and the
measure of how abstract is the remoteness of its language from that of ordinary experience. As it becomes more remote it is incumbent upon one who frames hypotheses in it to say what he means, to provide explicitly and by stipulation what is in the vast range of our common language tacit and pre-existent. When that is done then indeed hypotheses may be advanced. That something like this must be done is reflected in the commonly observed point that those who have not studied a particular high level science do not understand the sentences characteristic of it. And finally, the explicit process by which the theorizing scientist seeks to endow with meaning the language in which he frames his hypotheses exemplifies the anti-realist conception of semantics.

Two further crucial difficulties familiar from the positivist struggles to develop verificationism must be noted as pointing to the locus for substantial work in the establishment of anti-realist semantics. The first concerns reliance on counterfactual and subjunctive conditionals in the expression of conditions for verification. The second has to do with the character of the relation between a statement and its sense-determining evidential support; in particular, need it be one of entailment, or is the weaker relation of inductive support adequate to the account? These issues are vast, and merely to survey previous attempts on them would be no small matter. I shall limit myself here to the following few brief remarks.

The problem of reliance on counterfactuals arises when we attempt to specify what observations support a particular sentence, or rather would support it, if we choose or find ourselves in a position to make
them, or would have supported it though we did not actually carry out
the test or make the observations. An oft used example in these dis-
cussions is "That cube of sugar was soluble", where it is no longer
present for our study (let us suppose it has gone into some chemical
compound). Quine has attempted to account at least for such dispositional
terms as "soluble" by appeal to underlying structure or mechanisms
in virtue of which the property obtains (cf. §46 of Word and Object).
This approach seems to hold little promise of solving our problem
of an account of assertability conditions since "this solid has such and
such crystalline structure" (against a theory of the solubility of such
structures) leaves us still with need to ground that statement in con-
ditionals of the form e.g. "If this solid were X-rayed, the pattern
observed would be so and so." A promising departure in treatment of this
old problem in the context of anti-realist semantics might be to exploit
the anti-realist interpretation of the conditional, now transposed to
something like "we have it on general grounds that any experiences which
establish that a cube of sugar is placed in water will include experiences
of seeing it disappear" for "if a cube of sugar is placed in water then it
will dissolve".

The issue of entailment vs. inductive support suggests itself in an
attempt to understand the notion that the meaning of S consists in the
evidential experience which would establish it. This formulation
suggests that no gap is allowed between the evidential situation obtaining
and S being the case. But often, if not in general, for an empirical
statement S, experience cannot render S certain, but merely likely in
a certain degree. And the assessment of that degree of certainty belongs
to the canons of good inductive scientific practice. The old issue of 'strict' vs. 'weak' verifiability is a reflection of certain aspects of this problem. Wittgenstein's development of a distinction between criterion and symptom might be construed as an attempt to allow an account of meaning consisting in specified conditions of assertability even while allowing for defeasibility of S in the presence of satisfaction of those criteria. Gordon Baker's account of criterial semantics in his paper "Criteria: a New Foundation for Semantics" forthcoming in Ratio constitutes an important development of these matters.

As a final topic I want to consider the old positivist problem of the status of the principle of verification. From the perspective of this thesis the question is transposed to the issue whether realist vs. anti-realist semantics is an empirical matter, or whether analysis and reflections upon the concepts of truth and meaning reveal the correctness of one over the other.

It must be said that the positivists strenuously, and it seems to me also rather inexplicably and incoherently, resisted setting out the issue in terms of this sort of choice. They wanted neither to claim the principle analytic in virtue of the meaning of 'meaning', nor to allow it the status of empirical assertion as to the character of human discourse. Resistance to this latter view may have stemmed from their wanting to claim that some discourse (objectionably) proceeded in disregard of the principle. In any case by choosing to opt for neither of these accounts they created for themselves an especial embarrassment, as it was of course their claim that all meaningful assertion was either analytic or
verifiable. To extricate themselves they appealed rather weakly to such notions as "practical decision" (Carnap) and "persuasive definition" (Ayer): "I wish the principle of verification itself to be regarded, not as an empirical hypothesis, but as a definition" (*Language, Truth and Logic*, p. 16). But still, we must have some reason for wanting to accept and adopt it, and he adds "it is not supposed to be entirely arbitrary." His grounds for the claim are that "it is only if it is literally meaningful in this sense [i.e. satisfies the principles of verification], that a statement can properly be said to be either true or false." We have already considered the complex character of the claim for this coincidence (it is notable that no grounds were ever thought necessary for taking being either true or false as the mark of meaningfulness, as earlier remarked a sign of realist assumptions). Despite his disclaimers Ayer goes on in this same paragraph to a claim which sounds quite empirical: "unless it satisfied the principle of verification, it would not be capable to being understood in the sense in which either scientific hypothesis or commonsense statements are habitually understood." We find the same sentiment in Schlick's remark ("Meaning and Verification", p. 148) "Our view ... proposes to be nothing but a simple statement of the way in which meaning is actually [Schlick's emphasis] assigned to propositions, both in everyday life and in science." Incredibly enough this passage follows on the claim that verificationism "is no theory at all, for the term 'theory' is used for a set of hypotheses about a certain subject-matter, and there are no hypotheses involved in our view." This seems an altogether remarkable view as to the character of empirical observation in general and the scientific character of linguistics in particular. To
conclude this implausible story I will quote without discussion a para-
graph by Hempel, except to remark that it seems a heroic attempt to
fit together these disparate and apparently incompatible doctrines,
with at least explicit awareness as to what these elements are:

The empiricist criterion of meaning, like the result of any other
explication, represents a linguistic proposal which itself is neither
true nor false, but for which adequacy is claimed in two respects:
first in the sense that the explication provides a reasonably close
analysis of the commonly accepted meaning of the explicandum -
and this claim implies an empirical assertion; and secondly
in the sense that the explication achieves a "rational reconstruc-
tion" of the explicandum, i.e., that it provides, together perhaps
with other explications, a general conceptual framework which
permits a consistent and precise restatement and theoretical
systematization of the contexts in which the explicandum is used -
and this claim implies at least an assertion of a logical character.
("The Empiricist Criterion of Meaning", p. 125)

There is a flexibility in the account of anti-realist semantics which it
seems was not available to the positivists in their formulation of verifica-
tionism: it is explicitly allowed that the account of meaning might be
non-uniform. This comes about if it should be that the realist/anti-realist
character of truth varies between separable domains of discourse. It may
be that in the end one hopes not to need such flexibility through arguments
to show that the character of truth is e.g. uniformly anti-realist, and
that where it might seem to be realist is just in virtue of anti-realistically
acceptable decidability (as in the coincidence of intuitionist and classical
theories in the domain of Skolem arithmetic). But be that as it may, the
discussion of semantics is greatly facilitated by structural provision in
the account for explicit limitation of the problem to ranges of discourse
sharing a particular character.
One obvious caveat needs be entered to the claim that the positivists' principle of verification does not admit a non-uniform account: enshrined in their whole enterprise is the distinction between the character of meaning attaching to statements whose truth value is analytically determined and those which have to do essentially with experience. Quite apart from the issue of whether the distinction as so drawn is tenable, drawing it is not a flexibility as to what the outcome of investigation into the nature of meaning might reveal, but a rigid frame into which the whole theory is forced.

A good example of where the positivists might happily have availed themselves of the possibility of a non-uniform account is in the treatment of past tense statements. They came to see themselves forced to a choice between an account in terms of present evidence (with all the problems as to changes of sense over time, and the knowledge of investigative techniques then requisite to understanding) or an account in terms of past observation (which can only explain verification on the part of present language users if couched in terms of analogy and/or counterfactuals - highly problematic in themselves, and most uses so rough as to admit equally a fully realist account). Faced with such dichotomy the positivists would have been reasonable in opting for neither and allowing verificationism inapplicable to this range of discourse. Only on their scheme, any such exception had actually to constitute a counter-example. Hence they embraced some extremely unintuitive and awkward doctrines. As it happens, if my account of past tense statements is tenable (in which they are separated into those belonging to the central and primary uses of language (for which a direct verificationist account may be given) with the remainder parasitic for
sense upon them) then the domain of statements about the past may be saved for anti-realism. But even if that account should be seen to fail and with no prospect of another, anti-realism as a theory of meaning would in no wise be thereby demonstrated untenable; rather merely limitations as to its application would have been shown.

It may be doubted whether anti-realism will prove appropriate in all domains of discourse. Dummett has expressed misgivings as to the chances of a fully inclusive anti-realist semantics in remarking that "there are a number of reasons for doubting whether global anti-realism is coherent". But the gloss he then provides of a potential or prima facie incoherence in too thorough-going an espousal of anti-realism seems to me unworrying: "behaviorism is one species of anti-realism, namely a rejection of realism concerning mental states and processes; phenomenalism is another species, namely the rejection of realism concerning physical objects and processes; it immediately occurs to us to wonder whether it is possible consistently to maintain an anti-realist position simultaneously in both regards." (p. 250). This apparent difficulty seems to me the product of a fluctuation as to the conception of anti-realism. It is a genuine difficulty for global anti-realism where that is an ontological thesis: anti-realism with respect to x's is the doctrine that they are subject to ontological reduction in favor of y's. Global anti-realism is then the doctrine that we can 'reduce' everything, and this at best threatens circularity and in all likelihood incoherence, as is illustrated by the example, in which mental states and processes are reduced to objective states of a physical body, and at the same time physical objects are reduced to mental, subjective experiences. But none of this, as far as I can see,
casts doubt on global anti-realism as a conception of truth, and hence meaning.

My own view, tentative but hopeful is that global anti-realism is indeed a viable approach to semantics, and that reflection on the character both of how we use language and what can be achieved (and with what materials) as to a theory of sentence meaning will show it to be the appropriate one for language of creatures of finite capacities such as ourselves. This position will be developed and supported inter alia in the following two chapters. The first of those chapters will focus on Davidson's program of semantics for a natural language based on development of a Tarski-style truth definition for that language. The next and final one will consider semantics in light of Quine's strictures on theory of meaning and in context of his constructive considerations couched in terms of an account of radical translation. I shall argue that Davidson's semantics is anti-realist in character, and this provides the basis for my expectation that anti-realism constitutes a viable approach to semantics. And in particular it seems to me that in calling for empiricism without (the third) dogma, as he did in his exchange with Quine at the Philosophical Society in 1974, he espoused a view plausibly termed global anti-realism. The third dogma is the supposition that there is some base class of statements (e.g. observation reports, Quinean occasion sentences or whatever) in terms of which an account of meaning for the language to which they belong may be grounded. In eschewing that, and to the extent that his account is anti-realist, Davidson may be taken as urging global anti-realism. What is it though that shows Davidson's account to be anti-realist at all? Recall that in this chapter an argument was presented to the effect that the notion
of truth captured in theories satisfying convention T are for all that neutral as between realism and anti-realism. But Davidson finds that he must modify convention T to allow for the empirical character of his enterprise, in contrast to the formal deductive approach of Tarski's. And this modification, as we shall see, has the effect that the notion of truth is a constructivist one, something like "held to be true" or "has good grounds for asserting that ...." The effect then is that meaning is seen to be analyzed in terms of an anti-realist conception of truth. So after forty years, and by a route not quite what was envisaged (though Carnap certainly wouldn't have been surprised at the key role played by Tarski's indication of the form of truth theories), and in some ways from a conception anti-thetical in orientation to that of early would-be practitioners, verificationist semantics gives promise of proving viable.
CHAPTER 3

TRUTH THEORIES AS SEMANTICS FOR
NATURAL LANGUAGES

§1. Introduction

Donald Davidson's program for semantics constitutes the focus of consideration in this chapter. Davidson's fundamental contention is, roughly, that a theory of truth for a given language, of the kind Tarski showed how to devise, is a theory of meaning for that language. The program for semantics which the realization of this idea gives rise to evidently exemplifies the first of the two aspects of truth which it is the aim of this dissertation to explore. Connection with the second aspect is less evident. Indeed, as I have argued in Chapter 2, mere conformity of an account of truth to convention T, or even having the formal structure of Tarskian theories, is insufficient to determine the realist/anti-realist character of a putative truth theory. Is it then that an account of meaning may be rendered in terms of truth with that account neutral as between realism and anti-realism? Were Davidson's program grounds for an affirmative answer to this question, it would constitute a sort of counterexample to the schematism set out in Chapter 1. It would, in this regard, prove less destructive than would have the successful functioning of the logical positivists' verificationism. This latter program was to be considered an attempt to wed anti-realist semantics with a realist conception of truth. The neutrality of a truth-theory based semantics would not
actually contravene the account of functional interconnection between character of truth and conception of meaning. It would however, suggest that, from some vantage point, the account of meaning in terms of truth could bypass issues as to the nature of truth. Davidson, as we shall now consider, does not take this line.

§2. Satisfaction and correspondence

Davidson attempts allegiance to a form of correspondence, and so realism, with his paper "True to the Facts". The key argument there (p. 758) is that "the semantic concept of truth as developed by Tarski deserves to be called a correspondence theory because of the part played by the concept of satisfaction; for clearly what has been done is that the property of being true has been explained, and nontrivially, in terms of a relation between language and something else." This construal of correspondence hardly supports a substantial realist position, and this is deliberate, given Davidson's attempt to eschew any full-blooded notion of fact. As I argued in §2.2, it is doubtful whether the definition of truth in terms of satisfactions means even that "truth has been explained, and nontrivially, in terms of a relation between language and something else", let alone that some realist notion of correspondence is vindicated. The existentially quantified definition of truth in terms of satisfaction might suggest that a given sentence is rendered true by something to which it is related. But the suggestion is attenuated if instead we focus attention on the equivalent definition where the quantification over satisfying sequences of objects is universal.

What might be suggested in the formulation "Tr('p') if and only if ∀s(Sat 'p')"
is that truth is a relation between a sentence and the totality of objects. This principle does not in itself say very much. On fuller interpretation, it will either turn out wrong, or misleading. With amplification of its most literal reading it seems plainly false. The truth of a statement is not just a matter of there being objects, for a true and a false statement can be about exactly the same objects. This observation is the source of pressure to allow an ontology of facts, additional to the ontology of objects. Davidson faults earlier correspondence theories for succumbing to this pressure: "the failure of correspondence theories of truth based on the notion of fact traces back to a common source: the desire to include in the entity to which a true sentence corresponds not only the objects the sentence is 'about'... but also whatever it is the sentence says about them" (op.cit., p. 759). The misleading interpretation I have in mind perhaps is not to be distinguished entirely from the attempt to save the literal reading by appeal to facts. The idea is basically the one faulted in my discussion in Chapter 1 of the attempt to ground a realist conception of truth in ontological considerations. The subsistence of entities was thought to carry in its train determinate and objective truth and falsity. The argument was that objects exist with all their properties inhering in them. Considerations of the interconnection between truth and meaning showed the untenability of this approach.

It might appear that Davidson avoids any such ontological reflections, by favoring instead appeal to the systematic character of satisfaction: "All true sentences end up in the same place, but there are different stories about how they got there; a semantic theory of truth tells the story for a particular sentence by running through the steps of the recursive
account of satisfaction appropriate to the sentence" (op. cit., p. 759).

Because of insurmountable difficulties over indviduation of facts, Davidson supposes that an account of truth in terms of corresponding with fact will end up with each true sentence true in the same way, namely by corresponding with "the Great Fact". This account will not be individually illuminating in the way the recursion in terms of satisfaction may be: "the strategy of facts can provide no such instructive variety" (loc. cit.).

Certainly it is crucial to recast the notion of sentential truth in terms of systematic features of language, as Tarski did. But it would seem that an important reason why this step is so enlightening is precisely that it frees us from the picture that there is some thing in which the truth of a given sentence consists. Davidson seems gripped by the picture to such an extent as to want to resurrect it within the systematic satisfaction-based account of truth. Rather than freeing us from this picture of truth in terms of objects, Tarski's account for Davidson merely licenses it in paler shades: "theory of truth based on satisfaction is instructive partly because it is less ambitious about what it packs into the entities to which the sentences correspond" (p. 759 f.). What Davidson has in mind as the corresponding entities is the totality of sequences of objects referred to in discourse: "these entities are no more than arbitrary pairings of the objects over which the variables of the language range with those variables" (loc. cit.). But on this picture it is dubious to call correspondence a relation, and for the reason which bedevilled the old attempts: there is no relatum to which correspondence (satisfaction) can relate a statement. Clearly, it is not some particular sequence (no one is more appropriate than any other for a given statement). And equally clearly, it is not the totality of sequences,
for our truth theory will in general not contain enough set theory for there even to be such an object.

In the end it appears that taking truth as correspondence may for Davidson be nothing more than a suggestive manner of speaking, for it seems evident he would not mean to deny the preceding contention. One may then be left wondering what the point of this construal of satisfaction might be. Davidson's reply may be taken from his remark that "the payoff is clear: in explaining truth in terms of satisfaction, all the conceptual resources of the language in relation to its ontology are brought to bear" (op. cit., p. 760). Davidson does not amplify this observation, and the framework in which he considers these matters is not in terms of realism vs. anti-realism. But I do think this move is precisely that described and faulted in Chapter 1, by which the realist would attempt to establish the objectivity of truth through appeal to the existence of objects. The argument against this attempt can be sharply formulated in the present context of taking truth as just that concept characterized by a Tarskian theory. In these terms, the point might be viewed as an extension of the argument that to know that an account of truth takes the form set out by Tarski is not to know anything thereby as to the realist/anti-realist nature of the concept so characterized.

The general argument against the realist attempt to justify objective (bivalent) truth in terms of ontology is not to doubt the existence of the objects, but to query how it is we find language so endowed with meaning that we can use it to express our ontological doctrines. Implicitly, Davidson's attempt to extract a correspondence (realist) conception of truth from Tarski's analysis proceeds via Quine's criterion of ontology: "to be is to be the value
of a variable" (cf. "On what there is"). When we define the truth of \( p \) by the formula "\( \forall s(\text{Sat } 'p') \)" our employment of a quantifier ranging over all sequences of objects commits us to there being such. I do not aim to contravene this maxim of ontological commitment. If our ideology allows such generalized reference to sequences, our ontology must be equal to it, on pain that we fail to say what we think we are saying. But there is the rub. Do we quite know what we are saying? What is it to be the value of a variable? The problem which impresses the anti-realist is how we are to understand e.g. the universal quantifier '\( \forall s \)'. To suppose it understood in a certain way may be to pre-suppose the realist conception of meaning and truth, rather than to provide a justification of that conception. My claim is not that anything in Tarski's analysis of truth is uncongenial to a realist conception. Davidson could indeed espouse realism (though I did argue in Chapter 1 that this position is not as tenable as it may appear at first sight). The point is that such an espousal must be on the basis of some contribution additional to Tarski's formal theories.

Davidson manifests an inclination, as already noted (§1.2), to support his claims about truth and meaning by purely formal considerations of Tarski's analysis. By my argument of the previous paragraph and earlier points in §2.2, were Davidson to succeed with this limitation, his program for semantics would constitute a weak counter example to my Chapter 1 schematism. That is, it would show that meaning can be analyzed in terms of truth without need to enter into consideration as to the nature of truth. Recall the principal formal considerations by which Davidson would justify or render his program plausible. They are what I have termed the argument from analogy and the argument from structure. How well do they
succeed? I earlier argued that the analogical leap from "s means that p" to "s is T if and only if p" succeeds not at all. And the structural similarity between the form of Tarskian truth theories and the systematic working of semantic contribution to sentence meaning by sentential components seemed more in need of explanation, than itself to constitute one. Davidson does avail himself of an explanation, namely Quine's argument from radical translation; that argument is far deeper and more substantive than anything extractable from considering the form of Tarskian theories. As I shall argue, it also constitutes the foundation of an anti-realist conception of the interconnection between truth and meaning. Radical translation presents us with a situation in which meaning is analysed in terms of truth, and the conception of truth emerges through reflection on the nature of meaning so analysed. It thus fits the schematism set out in Chapter 1. Argument for this interpretation of Quine's views is presented in the following chapter. As far as possible, my consideration of Davidson's program will be independent of detailed discussion of Quine. But clearly the two discussions complement each other, and a certain degree of mutual dependence seems unavoidable.

§3. How is a Tarskian theory of truth a theory of meaning?

Granting a structural similarity between meaning and truth in their systematic application to sentences by operation with subsentential components, we may still wonder how the theory of truth turns out to be one and the same thing as a theory of meaning. Closely related though we realize the two notions to be, they seem nonetheless distinct. Part of our difficulty may stem from supposing meaning to consist in some sort of entities.
(somehow attached to statements) while truth is a property of statements. The common locution "what is the meaning of p?" encourages us in this conception. And even dropping the apparently denoting phrase in favor of a cognate verb, so "what does p mean?", leaves us still with the substantive "what". Meanings as entities may be eschewed on rather deep consideration, as from Quine's doctrine of the indeterminacy of translation. But simpler reflection may suggest that supposing there are such objects neither helps us in our ordinary operation with the notion of meaning (as when we are asked to explain what some complicated or foreign sentence means), nor in our attempt to frame a systematic, encompassing theory of such operations. Davidson is persuaded on these latter grounds: "my objection to meanings in the theory of meaning is not that they are abstract or that their identity conditions are obscure, but that they have no demonstrated use" ("Truth and Meaning", p. 307). One may find this point plausible on reflecting that should there be entities which are what sentences and words mean, the only device for access to those entities which we can manage is use of the linguistic terms which mean them.

These considerations point away from an entified notion of meaning and in the direction of taking the problem of meaning as systematic: "a satisfactory theory of meaning must give an account of how the meanings of sentences depend upon the meanings of words" (op. cit., p. 304). Construing the problem in this way is to take seriously the notion that language has meaning in virtue of the use to which we put it. The problem of meaning is to account for human capacity for using language to express whatever we fancy. Davidson is taking the human phenomenon of language as that for which we seek a theoretical account. He remarks, in "Theories of
Meaning and Learnable Languages" (p. 387): "When we can regard the meaning of each sentence as a function of a finite number of features of the sentence, we have an insight not only into what there is to be learned; we also understand how an infinite aptitude can be encompassed by finite accomplishments."

A response to the preceding discussion might be that in seeking an account of meaning, we face a choice as to what it is we seek. The goal might be to unravel the workings of a particular spoken language to the point where we can say how it is that its speakers manage to express themselves and be understood in it. But alternatively, our objective may be more fundamental; it might be to explain what it is in general for any (abstractly considered) linguistic system to express meanings. And it might be supposed that at this level, to characterize meaning as e.g. a correlation between linguistic entities and some other sort of objects might illuminatingly reveal general features of language and the nature of meaning.

It would be a serious misapprehension to suppose this; I have, in earlier remarks, imputed this supposition to the would-be realist. The only general account of meaning possible is one which generalizes on particular accounts of the sort applicable to a given language. And such a particular account must, as previously characterized, explain (in the sense of mimicking the effect of) manifested human linguistic capacity. The anti-realist insistence, that any account of meaning satisfy the constraint that meanings must be grasped by human speakers, is an expression of this principle. We see then, that Davidson's approach to theory of meaning is at least compatible with the anti-realist viewpoint.
I return now to the question which heads this section, and which the preceding discussion (though it bears upon the issue) has not answered. Davidson is sensitive to the issue, allowing that his "freewheeling use of the word 'meaning' ... is apt to shock old hands" since what he calls "a theory of meaning has after all turned out to make no use of meanings, whether of sentences or of words." ("Truth and Meaning", p. 310). In footnote 8 he defends his conflation of theories: "Since a truth definition determines the truth value of every sentence in the object language (relative to a sentence in the metalanguage), it determines the meaning of every word and sentence. This would seem to justify the title Theory of Meaning." But surely the truth definition has little to do with the particular truth values of sentences. We may fully grasp the definition, and yet find circumstances to be such that we are none the wiser as to whether some given sentence is actually true or false. In the paragraph to which this footnote attaches, Davidson supplies a more plausible (and standard) account of how truth underlies meaning. He speaks of "the obvious connection between a definition of truth of the kind Tarski has shown how to construct, and the concept of meaning." He explicates the connection as follows: "the definition works by giving necessary and sufficient conditions for the truth of every sentence, and to give truth conditions is a way of giving the meaning of a sentence. To know the semantic concept of truth for a language is to know what it is for a sentence - any sentence - to be true, and this amounts, in one good sense we can give to the phrase, to understanding the language." The Frege-Wittgenstein argument from understanding here utilized serves to justify identifying the sense of a statement with its truth conditions. But what is not quite evident is how a Tarskian theory of truth actually specifies the truth conditions for the sentences which it processes.
The notion of truth condition leads, on one natural line of explication, to the notion of possible world. Carnap, in *Meaning and Necessity*, and David Kaplan, in his thesis, *The Foundations of Intensional Logic*, develop this conception, tracing the fundamental ideas back to Frege and Wittgenstein (in the *Tractatus*). Dummett expresses something of this conception in "Truth" (p. 100):

What the truth of a statement consists in always plays the same role in determining the sense of that statement, and a theory of truth must be possible in the sense of an account of what that role is .... Such an account would justify the following. A statement, so long as it is not ambiguous or vague, divides all possible states of affairs into just two classes. For a given state of affairs, either the statement is used in such a way that a man who asserted it but envisaged that state of affairs as a possibility would be held to have spoken misleadingly, or the assertion of the statement would not be taken as expressing the speaker's exclusion of that possibility. If a state of affairs of the first kind obtains, the statement is false; if all actual states of affairs are of the second kind, it is true.

Possible world semantics is a matter of taking substantivally the notion of 'conditions under which a statement is true'. Possible worlds (or states of affairs) are individuated conditions. To understand a sentence is to know in which of them it would be true; in uttering a sentence assertively, we must envisage one of them as obtaining. An account of sentence meaning, on this picture, will associate to each sentence the possible worlds in which it is true, i.e. its truth conditions.

Such a theory of meaning, on the face of it, gives no promise of fulfilling the desideratum that it account for sentence meaning in terms of sub-sentential semantic contribution. We might try to rectify this shortcoming by attempting
to work into the theory the sort of functional structure Frege developed in his account of sense and reference. But in any case, possible world semantics would remain a theory in which meanings figure as entities. Evidently, this is not what Davidson has in mind when he justifies his appropriation of the encomium 'theory of meaning' to Tarskian theories of truth, on the ground that they "give truth conditions" for sentences. In what way, then, do they do this?

I want to consider two different, though related answers to this question. The first is to see the instances of convention T as expressing, on the right hand side of the biconditional, the truth conditions for the sentence mentioned on the left. The second is to extract an expression of truth conditions, not from the T-sentence itself, but from the derivation of the T-sentence. Both answers are unsatisfying, because they leave it a mystery how such a theory of meaning could be informative and non-trivial. I will now amplify this objection, and then give what I take to be a more illuminating account of how truth conditions emerge from the construction of a Tarski-style truth theory. This account will exploit essentially the empirical nature of the enterprise of constructing truth theories for natural languages. The anti-realist nature of truth, as a concept characterized by such theories, will be evident from this discussion.

The claim that instances of convention T yield truth conditions is unexceptionable. But if we then take sentence meanings as represented (or expressed) by these truth conditions, we obtain the again unexceptionable and unilluminating insight that a given sentence expresses its own meaning. In the case where the object-language is not a sub-part of the meta-language
the situation, admittedly, is not quite this trivial. The truth conditions of a given object-language statement are then expressed by its translation into the meta-language. But the reduction in triviality is due to use of the highly non-trivial notion of translation, and not to any enlightenment as to notion of truth condition, or to any significant use of that notion.

Davidson brands it an error "to think that all we can learn from a theory of truth about the meaning of a particular sentence is contained in the biconditional demanded by convention T" ("Semantics for Natural Languages", p. 184). He would have us seek enlightenment as to the meaning of an individual sentence, not in the right hand side of its convention T biconditional, but in the derivation of that biconditional. "What we can learn is brought out rather in the proof of such a biconditional, for the proof must demonstrate, step by step, how the truth-value of the sentence depends upon a recursively given structure" (loc. cit.). If what we learn is the meaning of p, and to grasp the meaning of p is to know its truth conditions, it must be that we can gain such knowledge through reflection on the proof of "T('p') if and only if p".

Clearly, just this is impossible. The proof by itself cannot tell us under what conditions the statement is true. For we may have two languages alike as to formal structure, but differing as to interpretation (e.g. the language of set theory and that of order on the reals; both have as sole non-logical constant a two place predicate). Yet two such languages may be served by the same set of formal proofs of T-sentences. But to raise this point is sheer obtuseness, it may be protested. Of course, we do not just look at the deduction of "T('p') if and only if p". We take into consideration the
meanings of syntactically basic items of the lexicon. In other words, we start from word meanings. What the T-sentence derivation allows us to do is parlay our knowledge of word meaning into knowledge of the conditions under which the sentence made up from them is true, and so into an understanding of it.

This conception of truth-conditions is clearly a much more subtle matter than that framed just in terms of the T-sentences. And given the tenor of some of my earlier remarks e.g. when comparing Brouwer's treatment of meaning with that of the positivists, it is clear there is much in this conception with which I must agree. Where I wish to disagree is in the extent to which it is supposed that meanings can be identified with formal derivations, rather than with the process by which we come to establish the correctness of the theory in which those derivations take place. The point is at least analogous to, if not actually part of my earlier claim that Tarski's analysis is neutral on issues having to do with the nature of truth. My unease at identifying sentence meaning with the T-sentence derivation, or even with what we can do with that derivation, is related to the point that merely noting the structural similarity between account of truth and the functioning of semantic contribution is insufficient to establish that a Tarskian theory is a theory of meaning. What establishes and explains the connection between truth and meaning is an argument such as that from radical translation. When the relationship between truth theory and theory of meaning is properly understood, we realize that the 'is' which relates them expresses some sort of predication rather than identity. (Frege's asymmetry test for predication vs. identity already suggests this point.)
A Tarskian theory of truth is not a theory of meaning since in no sense, either overt or tacit, is it about meaning. In part, this point merely reflects the situation of there not being any meanings for a theory to be about. But more important, and less obvious, is the claim that there is no simple way to reconstrue a truth theory so as to make it into a device which deals directly with sentence meanings. Nonetheless, in the course of establishing by test and observation the correctness of a given putative theory of truth for a given language, we find ourselves thereby possessed of a systematic capacity for interpretation. And what we will have done in establishing correctness of the truth theory is to determine the conditions under which given sentences are true. From the point of view of meaning, the testing of a putative truth theory is more important than what the theory actually asserts.

The first step in elaborating this conception is to say something concerning the empirical character of a truth theory for a given natural language. Davidson demands that, "if a semantic theory claims to apply, however schematically, to a natural language, then it must be empirical in character and open to test" ("In Defense of Convention T", p. 83). Following Tarski, Davidson has, from his earliest discussion of these matters, taken production of all instances of convention T as the touchstone of correctness and adequacy of any putative theory of truth. At first glance, however, it may strike one as implausible that this test of adequacy can have empirical content. For surely, once given a particular deductive theory, it is a mere formal calculation to determine whether or not the theory generates any desired instance of convention T. And by perfectly general considerations, which do not even depend on the particular
character of a given language, or of its putative truth theory, we know
that any such deductive consequence of the theory will be true. How
could it be otherwise? And if it could not, how can determining that
it is so give grounds for asserting the empirical correctness of the theory
of meaning so tested? In "Semantics for Natural Languages" (p. 185),
Davidson rather encourages this puzzlement with the remark: "... it is
no harder to test the empirical adequacy of a theory of truth than it is for
a competent speaker of English to decide whether sentences like
' "Snow is white" is true if and only if snow is white' are true." "In
Defense of Convention T" furnishes a concise answer to this conundrum
(p. 83 f.):

A theory of truth is not treated as empirical if its adequacy is
judged only in terms of the T-sentences it entails, and T-sen-
tences are verified only by their form; this happens if we
assume the object language is contained in the meta-language.
When this assumption is relaxed, the theory can go empirical.
This it does the moment it is claimed that the theory applies to
the speech of a particular person or group. The fact that the
object language is contained in the metalanguage does not keep
the theory from having empirical content: the fact that the
object language is contained in the metalanguage can qualify
as the fact to be verified.

What is the sort of test by which this fact can be verified or, as the case
might be, refuted? An answer is forthcoming in the need to modify
convention T, when it is to serve as a test of correctness for the truth
typeory of a natural language, to deal with the indexical apparatus, absent
from formal languages of the sort with which Tarski dealt, but crucially
and characteristically present in natural discourse. In "Semantics for
Natural Languages" (p. 180) Davidson expresses this matter as follows:
A theory of truth for a natural language must take account of the fact that many sentences vary in truth value depending on the time they are spoken, the speaker, and even, perhaps, the audience. We can accommodate this phenomenon either by declaring that it is particular utterances or speech acts, and not sentences, that have truth values, or by making truth a relation that holds between a sentence, a speaker, and a time.

It is this latter device for which Davidson opts. The following picture emerges of the enterprise of testing a putative truth theory.

We have constructed a theory for the language of some given speech community. The recursion and base clauses of the truth theory correspond roughly to a grammar and lexicon for that language. We are using as meta-language in this project our own native tongue, (the usual procedure in most any project). So of course we speak and understand the meta-language. The theory will generate modified T-sentences, relativized to speaker and time, of the following sort ("Truth and Meaning", p. 319):

"I am tired" is true as (potentially) spoken by p at t if and only if p is tired at t.

This T-sentence will be rendered probable or improbable, in application to a particular occasion of use, as there is match or mismatch between our assessment whether or not p is tired at t, with p’s readiness to assert the sentence "I am tired" on this occasion. His readiness may be manifest in outright assertion, or we may attempt to determine it by eliciting assent or dissent on our querying the sentence (in its application to him). This scheme for testing our truth theory ought to remind us of Quine’s program for radical translation.
The key notion which enables us to break into a completely alien conversation, where we have no idea what they mean by their utterances, is that, even so, we are sometimes able to determine what they believe. Namely, we can pick out utterances (as yet incomprehensible to us) which they hold true. If the situation is simple enough, we can decide what anyone in that situation would be asserting, and so translate their utterance by the one in our language we use to make that assertion. Beginning with observation sentences, the utterances most apt for this technique, we build to ones more complex and abstract. The constraint on correctness of the theory we so construct is that it effect correlations of true utterances in their language with true utterances in ours. What Davidson realized is that this process is precisely an attempt to construct a theory of truth for the alien language: "In suggesting that an acceptable theory of radical translation take the form of a recursive characterization of truth, I go beyond anything explicit in Quine" ("Truth and Meaning", fn. 10). With this realization, we secure an explanation of why, and how it is that meaning is accountable in terms of truth.

It is not quite correct to suppose that these considerations justify the view that a theory of meaning turns out to be a Tarskian theory of truth. Davidson seems to suppose this identification so justified (at least in part). He describes the process of checking correlation of truths in their language with ones in ours, and then remarks: "this sketch of how a theory of meaning for an alien tongue can be tested obviously owes its inspiration to Quine's account of radical translation in Chapter II of Word and Object" (loc.cit.). My point is that the operation described by Quine is not the testing of a theory of meaning, it is the theory of meaning. And what is
there described (and which is to be seen as an account of how we come to understand radically alien discourse) is the process by which we test a theory of truth for the alien language.

The distinction which I am here attempting to draw underlies the contrast between theories of meaning based on a so-called 'absolute' theory of truth and ones in terms of 'relativized' truth (possible world semantics). It is also the basis for an account of how sentential truth conditions figure in a Tarskian theory of (absolute) truth. Before discussing these matters, I want to insert two comments concerning the empirical enterprise of testing a truth theory. The first concerns the relativizing of convention T to speakers and times. As discussed above, this relativity was seen to arise from the feature of indexicality in natural language (the need to "take account of the fact that many sentences vary in truth value depending on the time they are spoken, the speaker", and etc.). My comment is that there is need to relativize an account of truth for a natural (spoken) language, whether it contains indexical apparatus or not. The point is established simply by noting that the argument from translation does not appeal to indexical variability of sentences. The point, however, is probably a mere counterfactual, as variation in pronoun and demonstrative reference certainly figures prominently in Quine's account of the first steps of radical translation. The whole notion of an occasion sentence depends on it. But perhaps no theoretically insurmountable difficulties would accrue were it that all such references had to be by uniquely referring terms. It may be the case that all such references had to be by uniquely referring terms. It may be the case that all natural languages do contain indexical devices, but this is not an issue. In any case, predicate-functor systems for first-order
logic and set theory may be taken to suggest that pronouns are dispensable.

My second comment concerns the interaction between constructing and testing a theory. The apparent picture in the above discussion was of two separate operations—first the construction, then the test. A more realistic picture, of course, will involve partial theory construction, testing of that, then revision and further construction. But while hypothesizing and testing constitute the constantly alternating activity of radical translation, the two processes retain their theoretically distinct character. Incidentally, what Quine labels "analytical hypotheses", in his description of the project of radical translation, are identifiable as a putative truth theory.

I will now discuss where truth conditions come from. As earlier remarked, it is not entirely unproblematic to say in what way a Tarskian theory of truth tells us the truth conditions of the sentences to which it applies. What I want to claim is that it does not, but that we determine them in the course of establishing the correctness of the truth theory. The testing of a truth theory is a theory of meaning; the test is in the truth of (relativized) T-sentences. Determining that a T-sentence is true is a matter of extrapolating from determinations of its correctness for particular \( p \) and \( t \). Each individual T-sentence is itself a schema. To know the correctness of its T-sentence is to know the meaning of \( S \). But because of the parameters in the T-sentence, determining its correctness is not a matter of carrying out some definitive observation, specifiable in advance.

Consider the sample relativized T-sentence set out above. The degree to which we are confident of its truth is the degree to which we are confident that, on any given occasion in which someone, in the community for whose
language we frame the theory, utters assertively and sincerely the sentence "I am tired", we will deem it correct to say of that person that he is tired. In other words, we know the conditions under which it is true to say, "I am tired." Those conditions are not read off from the T-sentence itself. Rather, in having established that the T-sentence is true, we know the meaning of the object language sentence S to which it applies. To the extent that anything is a Tarskian theory does duty for the meaning of S, it is the grounds on which we know the T-sentence for S to be true.

This claim is somewhat overstated, as will be apparent in subsequent discussion. It holds good without qualification in those ranges of discourse most immediate to direct observation, in particular for occasion sentences. For more 'remote' sentences, where truth and belief might plausibly be expected to diverge sometimes, translation must appeal to the evidence for the entire T-theory. This evidence will include establishing T-sentences as true in ways which are not directly illuminating as to the meaning of the sentence mentioned in them.

What are the grounds for asserting a particular relativized T-sentence? The answer points, I think, to something like the anti-realism of verificationist semantics, but with an important difference. That difference is the holistic approach to sentence meaning - in which we ask after the meaning of a sentence only in the context of a language, i.e. with respect to a generative theory of some language. It is this holistic element of the account which gives promise of rendering tractable the near fatal difficulties noted in the previous chapter's discussion of verificationism. This theme I will develop in the context of discussing, in the following chapter, Quine's approach to language. Here I want to elaborate only briefly the anti-realist nature of
truth and meaning characteristic of Davidson's account, as I am here construing it.

Radical translation begins with occasion sentences (those sentences with a demonstrative element). "I am tired" is one; on some occasions it is true to say it, not on others. This variability provides our access to meaning, through assertion and truth, as the variation is a direct reflection of truth conditions. We understand our own (object language) sentence (grasp its truth conditions). We check whether it correctly translates the alien occasion sentences by checking whether we and the native speaker agree on occasions when his and our sentence are true. In doing this, we establish the meaning of his sentence, and the truth of the relativized T-sentence for it. Davidson takes it, as does Quine, that meaning is determined only by elaboration of the basic technique of correlation here described. From this expression of empiricism (for that, I think, is what it is) follows the anti-realist character of truth and meaning. The concept of truth which underlies this notion of truth conditions is utilized only in terms of what can be established. Meaning is determined by correlation of his uttering e.g. "I am tired" with situations in which we have grounds to take it that he is tired. It is important to see that what has emerged as truth conditions in connection with the instances of convention T for these simple sentences is not just the sentence on the right-hand side. It is rather the capacity to determine when the sentence is correctly asserted.

What is it that happens in the case of non-occasional sentences, where the demonstrative elements which provided so neat a means of surveying truth conditions are not present? Take the following example:
'S' is true as (potentially) spoken by p at t if and only if all men are scoundrels.

The truth predicate remains relativized to a person and time, but these elements find no place in the right-hand side of the biconditional. The sentence "All men are scoundrels", uttered assertively by anyone at any time, is used, in each case, to make the same statement. It might be that we can assess that statement outright as to its truth or falsity. And it may happen also that we can determine directly that a particular person on some occasion does, or does not, as the case may be, hold S true. But even if these determinations match, so as to render an instance of the above T-sentence true, this in itself seems hardly grounds for supposing that 'S' means that all men are scoundrels. It is at this stage that we must utilize the systematic theory which generated the T-sentence, and not just the determination that the T-sentence is true.

Let us suppose, plausibly, that the theory which generates this T-sentence for S does so in terms of the following syntactic structure for the meta-language sentence: \( \forall x (m(x) \rightarrow s(x)) \). What the systematic T-theory will do for us is show how the truth of S depends complexly on the satisfaction of statement forms 'm(x)' ('__ is a man') and 's(x)' ('__ is a scoundrel'). For these latter simple kinds of statements, the direct (observation statement) account of truth conditions is fully adequate to determine their sense. In this way, we may be confident in identifying in 'S' elements with the interpretations '__ is a man', '__ is a scoundrel'. What our T-theory will also allow is identification in S of syntactic elements with the function '\( \forall x \)' and ' \( \rightarrow \) '. (That identification may be complex, e.g. involving transformations.) Knowing how these elements function in our meta-language, we are then in a
position to generate the truth conditions for 'S' from our grasp of the truth (or satisfaction) conditions for 'm(x)' and 's(x)'.

The correctness of these truth conditions for 'S' is a matter of the systematic correctness of our whole theory, and in particular of our identification of object language elements doing duty as devices for quantification and sentential combination. The support, or evidence, for the systematic theory is the truth of T-sentences. And, as we just noted, T-sentences for non-occasion (i.e. 'standing') sentences, where indexicality disappears from the right-hand side, may sometimes be established in ways which are not directly illuminating as to meaning.

Davidson allows that the apparent limitations on what can be learned from T-sentences may threaten "failure of nerve" in our resolution to take all that can be said about meaning as what we can extract from a theory satisfying convention T. In particular, he considers that a theory might result in the biconditional: "'Snow is white' is true if and only if grass is green," on the face of it some kind of potential counter-example, showing the constraint of true T-sentences too weak. His point is that such an apparently aberrant T-sentence must be acceptable if it followed "from a characterization of the predicate 'is true' that led to the invariable pairing of truths with truths and falsehoods with falsehoods - then there would not be anything essential to the idea of meaning that remained to be captured" ("Truth and Meaning", p. 312).

The contention of those who would adopt possible world semantics is that there is something essential to the idea of meaning left out of T-theories, namely meanings. The claim is that this is not only the case as a matter

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of ontology (counted by adherents of T-theory semantics as a virtue), but is also a matter of ideology. T-theories are too weak, it is claimed, to guarantee correct interpretation of meanings. The point comes down to the old distinction between extension and intension. Because two predicates may be alike in extension but differ as to intension (e.g. 'has a kidney', 'has a heart'), we must 'leave something out' if we limit ourselves to extensions. Similarly, just to know that a correlation between two languages consistently pairs sentences alike in truth value is insufficient ground for concluding that it constitutes a translation from one language into the other. And, indeed, the pairing of "grass is green" with "snow is white" is just the sort of insufficiency envisaged.

As earlier remarked, possible world semantics is the attempt to work directly and explicitly with the notion of truth conditions. Truth conditions are what the theory is about, and so in an explicit sense, it is a theory of meaning. Further, it is a theory in terms of truth, and as formulated by e.g. Carnap, Montague, D. Lewis, Tarski's analysis of truth, applied to a model structure, is the fundamental tool. And the truth concept employed must satisfy the constraint:

\[(T)_w: \text{"}S\text{"} \text{is true in } w \text{ if and only if } S_w\]

This is not convention T because of the 'w' which occurs on the right ('S_w' is 'S interpreted in world w'). But as Davidson has come to realize, possible world semantics is not, on this score, to be ruled improper: "The interesting difference between a theory of truth in a model and a theory of 'absolute' truth cannot be described, as I have been pretending, by saying that in the latter, but not the former, truth is characterized for each
sentence S without appeal to any resources not in S itself. The reason, of course, is that 'absolute' truth goes relative when applied to a natural language" (''In Defense of Convention T'', p. 85). The difference between the two theories does reside in the character of their respectively relativized versions of convention T. And the difference is in the matter of being open to empirical test: "The verification of instances of the T-sentence, or rather their surrogates in a theory relativized to speakers and times, remains respectably empirical. No doubt some pragmatic concept of demonstration will come into play. But such a concept is one we may hope to explain without appeal to notions like truth, meaning, synonymy or translation. The same cannot be said for truth in a model" (loc.cit.).

What this point comes to is the claim, earlier noted, that the first steps of radical translation, by which correlation of sentences alike in truth value is effected, constitutes the whole base from which an account of meaning must arise. To suppose this basis insufficient to guarantee getting meanings right is to suppose that there is a fact of the matter, where there is none. Any truth theory which fits this account of how they are constructed and constrained is thereby correct. Thus we face the possibility that "a number of significantly different theories of truth will fit the evidence equally well" ("Semantics for Natural Languages'', p. 186). This possibility is Quine's doctrine of the indeterminacy of translation. I will consider this matter in some detail in the following chapter. In particular I want to look at ways in which this possibility might be realized (Davidson's "grass in green" example has the form but not the content of such an instance). The connection between indeterminacy and an empiricist conception of language theory will be considered. And I will elaborate the point that this conception
constitutes an expression of anti-realism.

I conclude this section with a discussion of Davidson's attempt to avoid taking the totality of instances of convention T as his theory of truth. While any theory which yields the instances of convention T for a given language is in some sense a theory of meaning for that language, it is clear that certain such theories will not be illuminating, in particular the theory consisting simply of the instances of convention T (from a formal point of view this is a perfectly good theory; given recursive syntax for a language, the instances of convention T may be recursively generated). Davidson's response has been to add to the condition that a theory of truth yield convention T that it be finitely axiomatized. To be sure, this immediately blocks the non-informative truth theory consisting in the instances of convention T, but I find this move utterly ad hoc and un-motivated (at any rate its only motivation being that it blocks one particular uninformative theory).

Unlike the finiteness condition, there are intrinsic reasons for demanding of a theory that its fundamental principles be effectively presented (recursive). These reasons have to do with our ability to understand and grasp the workings of a theory whose function is to help us handle some notion under analysis. None of these reasons can lead to a condition of finiteness. Indeed, it might well be in a particular case we would choose, on grounds of comprehensibility, a theory infinitely axiomatized by a single, short (easily surveyed) schema in preference to one axiomatized finitely, but with very many axioms. Consider the contrast in perspicuity between Gödel's characterization of the constructible universe in terms of the ramified
schema of definability vs. the explicit characterization in terms of the
eight 'F' operations. Davidson's discussion of the trivial truth theory
and the means of blocking it does indicate an appreciation that the finiteness condition is ad hoc, in the sense that it is adopted simply to block a
particular aberration, and he entertains the possibility that "other ways
exist of ensuring that a theory of truth has the properties we want"
("Semantics for Natural Languages", p. 178).

It is puzzling that Davidson does not adopt as the further condition on
theories of truth the very desiderata on the basis of which he is confident
in rejecting the instances-of-convention-T theory (and on the basis of which
he then looks around for a condition on the basis of which to reject that
theory): "Such a theory would yield no insight into the structure of the
language and would thus provide no hint of an answer to the question how the
meaning of the sentence depends on its composition" (loc. cit.). Davidson
firmly insists that the problem of meaning is a systematic one, a matter of
understanding how the meanings of sentences are built up from the meanings
of their components, rather than an absolute one of determining directly
the meaning of any given sentence. And it seems then natural that this in-
sight be added as a condition on any solution to the problem of meaning. It
is indeed the condition which directs Davidson's theory to the form it takes,
rather than any requirement of finiteness.

There is an exact, simple, and from certain viewpoints plausible restric-
tion on acceptable theories of truth which would rule out, amongst others, the
instances-of-convention-T theory. Such a viewpoint is adopted by Tarski
in much of his treatment of formal semantics. The restriction is to require
that truth, and any other overtly semantic notion, not appear as a primitive
in the theory, in other words, that the theory provide an explicit definition of the concept of truth. It is instructive to consider for a moment why this viewpoint is not adopted by Davidson. His motivation is, I believe, something as follows: our objective in constructing a theory of truth as a means of analyzing the notion of meaning for a given language is to reveal how the meanings of complex expressions of the language are generated from a finite stock of basic elements. This is precisely what is explicitly revealed to us in the recursion clauses which determine satisfaction (and so truth) for the language, and it is these clauses which play the role of axioms in a theory of truth where satisfaction is characterized recursively rather than explicitly. Hence our truth theory best achieves its function with respect to the problem of meaning when the recursion clauses are taken directly as axioms.

There is a further important difference between explicit and recursive truth theories which may be relevant to present purposes. The meta-theory in which an explicit truth definition is constructed must, in general, be in some way strongly set-theoretic in character. This is not true in the case of a recursive theory, which need contain, in the way of conceptual apparatus additional to the object language, only the two place predicate to be characterized as satisfaction and the power to form finite sequences of individuals of the object language. There seems a prima facie attraction to solving the problem of meaning for a given language sticking as closely as possible to the conceptual apparatus already present within that language. Just that is impossible, as Tarski's theorem tells us, but we may take it that postulating the formal properties of satisfaction is less a step away from the object language than adding assumptions about the
existence of sets. There are applications of Tarski's theory of truth where such assumptions must be made, in particular where one wishes to obtain relative consistency proofs by use of truth definition (the articles by Wang, Mostowski, Kreisel and Wang, Kreisel and Levy cited in the bibliography). But in all such applications the orientation is fundamentally mathematical, and set-theoretic assumptions are quite within the spirit of the enterprise.

§4. Achieving the program

Thus far I have discussed general grounds for holding that the problem of meaning for a given natural language can be solved through constructing a definition of truth for that language, and I have also considered what conditions might be put upon such a theory of truth in advance of its construction to ensure its informativeness as a theory of meaning. These considerations aim to establish the conditional: if a theory of truth (satisfying the indicated conditions) can be constructed for a given natural language, then the problem of meaning for that language is (thereby) solved. Thus, on the basis of philosophical analysis, we may be encouraged to embark upon a program whose goal is to provide a semantics for a given natural language and whose method is to construct a definition of truth for that language. Grounds for expecting success of the program (i.e. that we can establish the antecedent of the above conditional) may be adduced on the basis of philosophical argument, and similarly we might argue as to the most promising road to success, but the achievement of the program itself belongs in large part to empirical linguistics. Though considerations to this effect are found in Davidson's writings, the nature and magnitude of the work in
It is clear from the outset that in the execution of Davidson's program we have to do with an essential and non-trivial extension of Tarski's techniques. As we have noted in Chapter 2, Tarski himself specifically expressed doubt as to the possibility of an extension of the sort envisaged. While this certainly hints at the substantive work required beyond what Tarski has shown us how to do, it does not of course count as an argument against the possibility of this work being done. In particular, the situation at the time Tarski wrote about the matter of truth definitions in natural languages is importantly different from that at present as regards the state of linguistics. In the intervening forty years, and most especially since 1957, we have seen the rise of theories of syntax for English and other natural languages which promise, if they do not yet achieve, both comprehensiveness and mathematical exactness. At the time when Tarski considered the matter, no such tools were at hand ("We are not able to specify structurally those expressions of the language which we call sentences, still less can we distinguish among them the true ones" ("The Concept of Truth in Formalized Languages", p. 164). This difference is of course crucial, for if one reflects on the general characteristic of constructing a Tarski-type truth definition, one is reminded that an effectively presented syntax for the language in question is an absolute prerequisite. In the case of formalized languages the syntax takes a particularly simple (recursive) form. Indeed, this is precisely what characterizes formalized languages. Let us now consider how one might utilize the more complex syntax embodied in some type of generative grammar to
extend Tarski's technique for the construction of a truth definition to
the case of natural languages.

That this approach to constructing a truth definition for a natural language involves an essential mathematical extension of the technique developed by Tarski is clear when one notes that the syntax for a formalized language can be expressed by a simple phrase structure grammar. It is Chomsky's essential insight that to handle natural languages, a phrase structure grammar is inadequate and must be supplemented by a system of transformations (Syntactic Structures, Chapter 5).

If the scope of the truth definition envisaged as the core of a semantic theory for a given natural language is applied to a syntax of a sort such as Tarski has already taught us to deal with, we are then covering a proper sub-part of that natural language, and that part we are covering is clearly not a natural language. We are thus left with the problem of showing how providing a semantics for this segment of e.g. English extends to a semantics for the whole (or some natural sub-part) of English. Obviously, that it does so is not a consequence of the general argument that constructing a Tarski-type definition for a language provides that language with a semantics.

Davidson has written on occasion as though extending Tarski's work to natural languages is merely a matter of being more thorough-going and systematic in the enterprise of formalizing language, a project on which philosophers have been engaged since Frege's fundamental achievements. In "Truth and Meaning" (pp. 314-315) he describes the situation as follows:

Tarski has shown the way to giving a theory for interpreted formal languages of various kinds; pick one as much like English as possible. Since this new language has been explained in English and
contains much English, we not only may, but I think must, view it as part of English for those who understand it. For this fragment of English we have, ex hypothesi, a theory of the required sort. . . . Much of what is called for is just to mechanize as far as possible what we now do by art when we put ordinary English into one or another canonical notation.

This passage appeals to some extraordinarily problematic notions. What is it for a formal language to be "as much like English as possible"? What are we invited to do in the call to "mechanize as far as possible what we now do by art"? Is one formal language more like English than another if it has the greater amount of vagueness? On the standard view of formal languages they do not have any. The elimination of vagueness for purposes of philosophical and foundational purposes was certainly one of Frege's main motives in devising formal languages (cf. Grundgesetze der Arithmetik. Vol. ii, §56). Is a formal language more like English than another in virtue of grammatical complexity? As earlier noted, they all seem on a par, and separated by the gulf of transformations from natural languages, in being generated simply from a phrase-structure grammar. As to mechanizing the process of regimentation, clearly what is required is a systematic theory of English grammar in terms of Tarskian logical forms. Hitherto, philosophers hardly knew to look for one, let alone made progress in the construction of such a theory.

The most promising approach to providing a theory of the connection between formalized (regimented) and natural language seems to be the identification of logical form with the linguistic theoretic notion of deep structure. In "Semantics for Natural Languages" (p. 187) it is clear this is what Davidson has in mind: "In one respect, logical structure (as we may call the
structure developed by a theory of truth) and deep structure could be the same, for both are intended to be the foundation of semantics. Deep structure must also serve, however, as the basis for the transformations that produce surface structure, and it is an open question whether logical structure can do this job, or do it well." Davidson does not say how logical form might carry this linguistic function, but he notes that "no arguments to show logical structure won't serve seem conclusive", and he adds a brief expression of intuition: "It would be strange if the structure essential to an account of truth were not effectively tied to the patterns of sound we use to convey truth." I am in sympathy with the intuition expressed, but as a potential argument the remark feels circular. The force of it lies in the notion of "that structure essential to an account of truth", where this phrase is meant to pick out a Tarski type definition applied to the subpart of English whose syntax consists of elements from canonical notation. That a Tarski-type definition over a logical syntax is essential, or even adequate for the notion of truth in a natural language is precisely what is now at issue.

In any case, it should be clear that no amount of a priori argument can be decisive in settling the question of whether deep structure can be identified with logical form, and even if it could, the interest in such an argument would be far less than an affirmative answer based on actually exhibiting a working transformational grammar whose phrase structure component generates precisely the well formed formulas of quantificational predicate languages. There are a number of linguists and a few philosophers actively engaged in this project, including McCawley, Lakoff, Ross, and Harman. Harman's paper "Deep Structure as Logical Form" presents
some interesting results in this project, and at the same time provides a useful synopsis of the whole enterprise of basing the syntax of natural language on logical form. Also of interest in this connection is his more recent "Logical Form", which concerns itself particularly with adverbial modification. These papers seem to me the model of work needed to justify an affirmative answer to the question at hand and provide a basis for a theory of meaning.

It needs to be remembered that finding a transformational grammar which will generate the surface patterns of English from logical forms does not of itself assure smooth sailing to a truth-definition based semantics such as we envisage. There remains the problem of checking that transformations preserve semantic content, or if not, alter it in a way which can be "calculated" from the input and the transformation. In practice, this hoped for result will likely influence our choice of grammar. There is a good example of this interchange to be found in Harman's "Deep Structure as Logical Form" (p. 281), where it is demonstrated that purported active/passive pairs with different meanings (as measured by possibly differing truth values) actually come from distinct deep structures, when deep structure reflects logical form. Ultimately, we should require a completely general demonstration of meaning preservation or functionality for all those transformations we utilize to generate surface structure of English from logical forms.

A crucial empirical component of the entire enterprise appears to me the construction and testing of the linguistic theory needed, particularly the determination that the grammar does generate the intended natural language, where of course the very delineation of a natural language is a matter of empirical research. In general it seems to me that throughout
his discussion, Davidson gives insufficient emphasis to work in linguistics which must be done. Thus when he presents a partial list ("Semantics for Natural Languages", p. 186) of what will emerge as the deep problems of the program, mentioning such items as propositional attitudes, mass terms, adverbial modification and so on "through a long list familiar, for the most part, to philosophers," it seems to me he envisages that work as proceeding in the old philosophical program of finding logical form, whereas it must both do this and at the same time treat the complementary linguistic problem of finding a grammar which will bridge the gap revealed by philosophical analysis. Davidson has contributed to his program a number of insightful papers, including "Logical Form of Action Sentences" and "On Saying That", which get at logical form, but without also providing a theory connecting these forms with the surface structures they are claimed to underlie. If such exercises in philosophical analysis are to contribute to the systematic account of meaning for natural languages which is envisaged, they must be explicitly integrated with a working generative grammar.

Before concluding, I would like to insert a brief parenthetical remark regarding the character of the formalized language to be used as deep structure in the proposed program. It appears to me that at this stage of research and theory development, the question of what kind of underlying logic should be adopted remains an open one. The work which Harman reports suggests that for a smoothly functioning theory it will be advisable to add to the standard stock of universal and existential quantifiers: "quantifiers in deep structure differ from the familiar quantifiers of modern logic in their variety of type and in the restrictions they carry with them" ("Deep Structure as Logical Form", p. 281). Still, Harman and
Davidson and certainly Quine favor, indeed insist upon, first order quantification. One of the main arguments frequently brought forth to justify the unique position of first order languages is that they admit of a deductive theory complete with respect to logical validity. It seems to me, however, that such considerations are in this situation irrelevant as we are not so much concerned here with deductive properties (beyond the trivial ones needed to obtain instances of convention T); rather, what is at issue is expressive power (power to express the conditions for some statement's being true), and second order languages are on this score more richly endowed than are first order ones, e.g. it is within second order languages that we are in general able to characterize structures uniquely. More importantly, perhaps, the completeness Quine and others have in mind is of classical (realist) logic, and as I argue, the proper logic for this conception of truth and meaning is something akin to that valid on the intuitionist (anti-realist) conception.

Davidson's program sets out the framework in which a program for semantics based on the interconnection between meaning and truth may be developed. His inclination to a realist notion of correspondence was seen to be something of an aberration; the semantics which result from the empirical treatment of truth theory is seen to be a form of anti-realism. Davidson does discuss the substantive issues which underlie his program, but we have noted a tendency to utilize purely formal considerations perhaps more than what it is that needs establishing may demand. Further consideration of the issues which underlie Davidson's program will find development in the following chapter, where Quine's view of language is the focus of attention.
CHAPTER 4

QUINE ON TRUTH, MEANING, AND REALITY

§1. Introduction

This final chapter aims, in discussing those features of Quine's philosophy particularly bearing upon the issues so far of concern, to draw together lines of development from the preceding three. Let me first review briefly the enterprise of these three chapters, and then say how this one provides a completing link between them.

Chapter 1 sets out a schematism which is tested in the development of subsequent chapters as well as (or perhaps rather, in the process of) serving as framework for that development. Two aspects of truth are focused upon: first, the character of the concept in a dichotomy (or possibly along a spectrum) from being transcendent (realist), i.e. a matter of how the world is irrespective of human cognizability, to being immanent (anti-realist), i.e. understandable only in terms of human capacity to establish its applicability; second, the interconnection between truth and meaning. These two aspects themselves interact to provide a functional correlation between the realist/anti-realist character of truth, and theory of meaning. The enterprise has been to explore particularly realization of the anti-realist side of this scheme. Chapter 2 surveyed verificationism as a theory of meaning and found it exemplary of features characteristic of anti-realist semantics, but apparently to have been arrived at by positivists espousing the interconnection
between truth and meaning and also an ostensibly classical (realist) conception of truth. The force of this prima facie counter-example to the scheme set out in the first chapter was deflected by exhibiting directly incoherence between realist truth and verificationist meaning in the form of sentences evidently possessed of a "realistically conceived" truth value inaccessible to human determination. Purged of incompatible elements, there remains from verificationism a corpus of problems and attempts on them instructive for, if not actually a contribution to the realization of, a program for anti-realist semantics.

Chapter 3, focusing on Davidson's identification of semantics for natural languages with the establishment of Tarski style theories of truth, constitutes on the face of it a development from Chapter 1 orthogonal to that detailed in the study of verificationism. The point of departure for Chapter 2 is the apparent instantiation of anti-realist semantics by verificationism. For Chapter 3 it is, of the elements set out in the general framework, the second aspect of truth (the interconnection between truth and meaning) which at first glance is exemplified in Davidson's program. Superficially, that program makes only minimal claims as to the realist/anti-realist character of the concept of truth invoked. ("True to the Facts" constitutes an attempt 'in retrospect' to give allegiance to a realist conception of truth; that its failure, detailed in §2.2, leaves the overall program intact supports this assessment.) Indeed, quite generally, mere conformity to the constraints on a theory of truth set by Tarski, or even following the specific forms adopted in his analysis, proves insufficient to guarantee the realist or anti-realist character of truth. And finally, the semantics obtained in Davidson's
Tarski-type theories apparently bears no resemblance to the account of sentence meaning in terms of conditions for assertability. Nevertheless, grounds are adduced in the course of Chapter 3 for seeing in Davidson's program a road to realization of the scheme for anti-realist semantics. The anti-realist element enters with Davidson's taking the construction of truth theories for natural languages as an empirically constrained branch of scientific linguistics, with observation of human verbal behavior and the circumstances which elicit it as data. This project is markedly in contrast with Tarski's, where the establishing of adequacy for a particular theory by its capacity to yield each instance of (T) is a purely logico-deductive one. As we saw, the process of theory building and observation by which instances of convention T come to be established as tests of a putative truth theory are such that the concept T so captured must be understood along the lines of "justifiably asserted". To the extent then that Davidson's program provides in terms of this notion an account of sentence meaning, it is a form of anti-realist semantics.

So it is that Chapters 2 and 3 find, in two apparently disparate philosophical programs, lines of development congenial to the realization of anti-realism as a philosophy of language. That verificationism of the logical positions and Davidson's truth definitional semantics alike prove pliant in being co-opted to the anti-realist side of the scheme interconnecting truth and meaning is not accidental. As I hope will be established inter alia in the consideration of Quinean doctrines germane to that scheme, Quine provides a link, both genetic and conceptual, between these two programs.
Quine is rightly seen as one of logical positivism’s most telling critics, at least of those sympathetic enough to have understood what it was attempting to do. Indeed, much of the force of Quine’s critique stems, I think, from his sharing with positivism quite fundamental goals, in particular commitment to as thorough going an empiricism as should prove tenable. His negative doctrines, in particular the impossibility of drawing an analytic/synthetic dichotomy and the indeterminacy of translation, go to establish that much of what the positivists attempted in the name of empiricism was indeed untenable. But his own positive contributions seem to me clearly to aim at furthering those general empiricist goals, and especially as they relate to language theory. Thus I would take the account of radical translation, set out most completely in Chapter 2 of *Word and Object*, as aiming in effect at the accomplishment of as much of the original positivist goals with respect to an account of meaning as his appreciation of limitations on any such account would allow. Loosely, perhaps somewhat misleadingly, but not I think, entirely inaccurately, the program of radical translation might be described as "meaning = method of verification" gone holistic.

Chapters 2 and 3 of this thesis are then brought together with the further observation that in broad outline Davidson’s program for semantics can be viewed as putting into a particularly systematic form Quine’s program for radical translation. As has already been noted, Davidson himself has drawn attention to this view of his position: his "sketch of how a theory of meaning for an alien tongue can be tested obviously owes its inspiration to Quine’s account of radical translation in Chapter II of *Word and Object*." (*Truth and Meaning*).
The following pages will further reinforce this viewpoint. Davidson's espousal of Tarskian theories of truth as solving the problem of semantics may engender, for those familiar with Carnap's philosophical development, a certain sensation of déjà vu. Quine's anti-realism is apparent already in his taking observation statements as the foundation of semantics. And as I shall argue, holism, in the form of theory dependence, which he espouses, leads to an anti-realist conception of truth for non-observational statements as well. This ought not to occasion much surprise, in view of the following ad hominem points: Word and Object is dedicated to Rudolf Carnap, and the dedication celebrates him as teacher (as well as friend). And, as an epigraph to the volume, Quine has taken lines from Neurath's "Protokollsätze", Erkenntnis 3 (1932), p. 206.

§2. "Two Dogmas": the attack on verificationism

The idea that Quine's philosophy of language constitutes a line of development from the verificationism espoused by logical positivists might impress for its initial implausibility. Indeed, as is famous, Quine has stigmatized verificationist reductionism as mere dogma. This condemnation is bound up with his disavowal of a distinction between a statement's having the truth value it does analytically (in virtue of meaning) or synthetically (in virtue of experience). Maintaining that such a distinction is there to be elucidated constitutes the first of the "Two Dogmas of Empiricism", which are the focus of attack in what is probably Quine's most widely known paper. As a background to
discussing directly what Quine says in that paper, I want to sketch an account, drawing on points developed already in Chapter 2, of the interconnection between verificationism and analyticity.

As detailed previously, the principle of verification arose not directly as a contribution to philosophy of language or semantics, but as the outcome and in furtherance of general and mainly negative reflections on the character of philosophy. The enterprise was to purge philosophy of nonsense. A statement is nonsensical if it fails to say anything, not as in being 'empty' or vacuously true, e.g. "He is not both here and not here", but in the sense of failing to be either true or false. The problem then for logical positivists was to provide a substantive analysis of how it is that statements are endowed with a truth value, and so manage to say something, have meaning. The point from which they began was allegiance to empiricism, as set out in the following terms: "The fundamental tenet of modern empiricism is the view that all non-analytic knowledge is based on experience. Let us call this thesis the principle of empiricism," (the opening lines of Hempel's "The Empiricist Criterion of Meaning" subsequently to be referred to as (i) ). It is this principle which Quine labels the "First Dogma". Properly it is itself an amalgam of two separably distinct doctrines: first, that an account of analyticity can be made out as an analysis of what it is for a statement to be true or false in virtue of meaning alone, and secondly, that if a statement is not analytically endowed with truth value, it is so in virtue of a situation open to determination by human experience.

Not that much turns on epithets, but it seems to me the second of
these doctrines but not the first was, as asserted by the logical positivists, dogmatic. By this I mean that it was a doctrine such that all who would be countenanced as co-workers in the empiricist program must subscribe to it, and little if any argument was actually expended in its support. While the positivists were quite aware that no less an anti-metaphysician than Kant did not accept the doctrine, in virtue of his doctrine of synthetic a priori truths, they tended to wave his position aside, rather than argue with it. Cf. the following rather patronizing remarks by Ayer:

I think that we can preserve the logical import of Kant's distinction between analytic and synthetic propositions, while avoiding the confusions which mar his actual account of it, if we say that a proposition is analytic when its validity depends solely on the definitions of the symbols it contains and synthetic when its validity is determined by the facts of experience.

(Language, Truth and Logic, p. 78)

In contrast, as Quine's own arguments against successive attempts in the first four sections of "Two Dogmas" bears witness, a considerable effort was expended in the attempt to provide a working analysis of analyticity. While there was faith that the notion was there to be elucidated, that was not taken as sufficient to render the notion usable.

Hempel continues the lines just quoted as follows: "Contemporary logical empiricism has added to it [the principle of empiricism] the maxim that a sentence makes a cognitively meaningful assertion, and thus can be said to be either true or false, only if it is either (1) analytic or self-contradictory or (2) capable, at least in principle, of experiential test." (This further thesis will subsequently be referred to as (2).) Here
then is Quine's Second Dogma, what may be called the principle of sentential verificationism. It was this principle which early positivists took it solved their problem of detailing a substantive empiricist criterion of meaning. Its formulation constitutes a non-trivial extension from the principle of empiricism, and as I indicated in discussing Sohlick's (1938) formulation of verificationism, a largely unjustified (and so it may be said dogmatic) extension. What would provide justification is a systematic account of how the assertability of a sentence is an effective function of assertability conditions on its sub-sentential components. A sentence which constitutes non-analytic knowledge may conform with the principle of empiricism by being 'based on experience', in the sense that obtaining such knowledge requires the experiences by which applicability of the predicate terms occurring in it are determinable; and quite likely other experiences belonging to the inductive support of theories, inference from theories and the like by which science and knowledge in general is advanced. But lacking the account of sentential assertability in terms of sub-sentential assertability, a sentence being so based on experience does not guarantee that it is itself capable of individual test, as the testability criterion of meaningfulness would require. Hempel is explicit that he sees the move from (1) to (2) as a step requiring justification. In a footnote attached to the word 'added' in (2) he agrees with Stace that the Testability criterion of meaning is not logically entailed by the principle of empiricism: "according to the latter, a sentence expresses knowledge only if it is either analytic or corroborated by empirical evidence; the former goes further and identifies the domain of cognitively significant discourse with that of
potential knowledge." Hempel then goes on to consider attempts to justify this latter claim starting from the former. These attempts aim at an account of verifiability in terms of deductive relationships to observation sentences, not by analysis of systematic contribution of subsentential assertability conditions. Hempel concludes "it is useless to continue the search for an adequate criterion of testability in terms of deductive relationships to observation sentences." (p.16) Instead, he proposes that "a sentence has cognitive meaning if and only if it is translatable into an empiricist language." Hempel takes this view to originate with Carnap in "Testability and Meaning." In that paper Carnap's principle of tolerance is exercised to the full, and much of his effort is expended in presenting careful formulations of the most stringent criteria of meaningfulness and allowing that "there are no theoretical objections against these requirements, that is to say, objections condemning them as false or incorrect or meaningless or the like; but it seems to me that there are practical objections against them as being inconvenient for the purpose of science." (p. 35) But he is ready to assert the following general and minimal condition: "As empiricists, we require the language of science to be restricted in a certain way; we require that descriptive predicates and hence synthetic sentences are not to be admitted unless they have some connection with possible observations." (p.33) Carnap was throughout his philosophical endeavor an adherent of analyticity and its application in the principle of empiricism. Early on he went further to maintain sentential verificationism: thus §7 of "Pseudoproblems in Philosophy" (1928) is an
espousal of "Factual content as a criterion for the Meaningfulness of Statements", where a statement has factual content when "we can think and describe the characteristics of an experience through which this statement would be supported" (p. 327, Rolf George, trans.). By 1936, as we have seen, Carnap was not anxious to maintain verificationism (even if not actually claiming it was impossible to do so), and as we shall shortly consider in some detail, he had already abandoned the doctrine by 1933, in *Logical Syntax of Language*.

Given the account here rendered as to the two doctrines which Quine takes for Dogmas, his focussing attention on them is a natural structure for a general assessment of the state of contemporary empiricism. Compatibly with that account, Quine's paper deals initially with analyticity in its role, one might have supposed, as the more fundamental of the doctrines. The first four sections of the paper takes up successively various attempts to explicate the notion, and find fault with each, notably circularity. This kind of argument cannot by itself show the impossibility of some other account succeeding, but it at least leaves the doctrine unsupported. And in light of previous remarks, sentential verification is thereby left unsupported too, but for all that potentially tenable. So it is natural to seek a general argument showing that no other line of support can be forthcoming. Quine finds in holism such an argument: "our statements about the external world face the tribunal of sense experience not individually but only as a corporate body" (p. 41) "If this view is right, it is misleading to speak of the empirical content of an individual statement." (p. 43) The holistic view of science is attributed
to Duhem, and the application of these doctrines to show the impossibility of sentential verificationism is attributed to Carnap. Quine cites (p. 41) "Carnap's doctrine of the physical world in the \Aufbau\"; I do not find in that work any very explicit passages as expression of this viewpoint, but §82 of The Logical Syntax of Language is just that. There, in particular, occurs the following italicized sentence: "Thus the test applies, at bottom, not to a single hypothesis but to the whole system of physics as a system of hypotheses (Duhem, Poincaré)." (p. 318)

However, the structure of "Two Dogmas" is not quite as this sketch would have it. Consideration of sentential verificationism arises in §5 as coordinate with various attempts earlier considered and faulted in §§ 1–4 to explicate analyticity. Thus "if the verification theory can be accepted as an adequate account of statement synonymy, the notion of analyticity is saved after all." (p. 38) Verificationism would, if itself acceptable, provide such an account, for it could then be set down "that statements are synonymous if and only if they are alike in point of method of empirical confirmation or infirmation." (p. 37) So Quine’s consideration of the interconnection between the two dogmas is not exactly in terms of the conceptual and historical account I earlier sketched. Thus when holism is invoked to demonstrate the impossibility of sentential verificationism, it is as one more nail in the coffin for analyticity but nothing that guarantees the coffin stays shut. However, having considered matters this way round, Quine does go at it the other way, in which an analytic/synthetic (\textit{a posteriori}) dualism is cited in
support of sentential verificationism. Such dualism, I take it, comes down to a claim that "the truth of a statement is somehow analysable into a linguistic component and a factual component." Quine's argument from this (in his view, of course, mistaken) claim is that "the factual component must, if we are empiricists, boil down to a range of confirmatory experiences." (p. 41) Quine's conclusion (expressed in opening the paragraph from which these lines are taken, and just following one which rehearses the definition of analyticity in terms of vacuous confirmation) is that "the two dogmas are, indeed, at root identical." I'm sure I do not know what the criteria of individuation for dogmas happens to be. One might reasonably require as a necessary condition on two forms of words expressing the same dogma that they be dogmatically asserted by the same people (rather than one asserted and the other denied by a given person). If so, we've already seen conclusive grounds that Quine's two are not one; and further, when Grice and Strawson came to the defense of "a dogma" it was not the root of Quine's two but rather the first (analyticity) they cherished, as against the second (verificationism) which, I am sure, they happily left to perish. But more to the point, previous considerations as to interconnection between the two doctrines provide grounds for doubt that they are so much as equivalent, in either the strong sense that espousing one requires on pain of inconsistency or the like espousal of the other, or in even a weak sense that espousing one provides the substance of a theory on which to espouse the other. Quine's derivation of analyticity from sentential verification cannot be faulted, but that has not been the direction of development in which philosophical efforts have been expended.
As to the opposite direction, even those inclined to embrace both doctrines found it difficult to justify the account of sentence meaning in terms of confirming experience on the basis of the general empiricist doctrine that the truth of a sentence consists only in the components of meaning and empirical fact. Quine's one-line argument quoted above ("The factual component must, if we are empiricists, boil down to a range of confirmatory experience") indeed expressed the intuition on which they pinned hope, but the need to provide an actual account of how the factual component consisted in confirmatory experience was generally appreciated. And we have seen that there were those such as Carnap and Hempel, sympathetic to the program, who came to maintain the untenability of verificationism, Hempel on the inductive evidence of failure to achieve a working account, Carnap on general considerations from Duhemian holism. Both saw no reason to doubt the usefulness of analyticity as a tool in language theory. And there are many philosophers, indeed firmly counting themselves empiricists, who would maintain that an empirical study of language may reveal what that language contributes to the truth of statements expressed in it, even while allowing that commitment to sentential verificationism would constitute a reductio of their position. Such ramifications as well as reasons to doubt the point, justify and even dictate fuller treatment than accorded in "Two Dogmas".

A more immediate consideration might have prompted amplification. With the step from analyticity to sentential verificationism, a complete argument is obtained against the analytic/synthetic distinction of the following structure:
So holism is seen to provide general grounds for failures in the accounting of analyticity detailed in §§1–4, and of course more crucially, for the failure of any attempt. Quine acknowledges Carnap as his source for this key notion. At the same time, Carnap is, in the earlier sections, taken as chief purveyor of attempts to render an account of analyticity. There is, of course, no contradiction in the use Quine makes of Carnap's doctrines — on the tacit assumption that Carnap contradicts himself. It would illuminate the discussion to know just where Carnap does so.

The final section, "Empiricism without the Dogmas", constitutes a somewhat metaphorical account of holism, in the course of which one is meant to see directly, I think, the untenability of identifying particular sentences as true in virtue of meaning. It will be instructive to compare these remarks with Carnap's explicit consideration of the analytic/synthetic distinction in the context of holism, as set out in §82 of *The Logical Syntax of Language*.

The holistic picture of human knowledge is that while we claim to know individual statements, we can do so only in virtue of their belonging to an inclusive theory which is itself supported by experience. In its most basic form, that experience is direct observation. As the practice of scientists makes evident, observation does not uniquely dictate theory.
Incompatible theories may be hypothesized to account for a single collection of data. The two theories will agree in having as deductive consequence the common set of observations they set out to systematize, but they may be incompatible in attributing opposite truth values to particular statements not in their common observational basis. Now if these statements over which they disagree are of a simple form, e.g. as yet un-conducted observations, then it may at least be possible to support one theory as against the other by conducting that observation. Even here the situation can be complicated. While the one theory may be 'decisively' supported over the other, the latter may not be entirely refuted thereby. Consider in this connection the observation of the precession of Mercury, in which the result was that predicted by general relativity, as against that of Newtonian mechanics. Further, the observation may itself be 'indirect' and require for its interpretation a background theory whose inter-relation to the two theories being tested would need to be taken into account. There are devices, subtle and not so subtle ("They were drunk when they made the measurement") for setting aside the judgement of 'decisive' observation. But it may be that the two theories disagree on a quite 'high-level', i.e. theoretical assertion, remote from immediate observations, conducted and potential. Determining the truth value of that assertion is thus bound up in the whole complex enterprise of choosing between the two theories, and that may turn on wide-ranging considerations of the remote consequences of the theories, pragmatic factors as to which theory is most comprehensive, comprehensible; perhaps also considerations as to familiarity and elegance will be weighted. What seems to emerge indubitably from such an account is that the meaning of an assertion

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about which the two theories dispute cannot be rendered simply in terms of the observations which would support it. And so more generally, all statements are mediated by theory, in the broadest sense, in their connection with observational experience. It is not implausible to suppose that the unending failure in attempts to provide a general theoretical account of what it is for a particular statement to be confirmed by another is directly a reflection of this complex and holistic state of affairs. The account of a particular determinate relation between two statements (one usually taken as observational) could not encompass the general inductive procedures by which whole theories are affirmed, as well as the pragmatic and wide ranging considerations which enter into the assessment of theories, and so of their parts. It is thus that Duhem's holistic picture of science counts against the attempt to render the meaning of a sentence in terms of conditions for its confirmation (the second dogma). Let me amplify this picture.

Total science is like a field of force whose boundary conditions are experience ... No particular experiences are linked with any particular statements in the interior of the field, except indirectly through considerations of equilibrium affecting the field as a whole. If this view is right, it is misleading to speak of the empirical content of an individual statement - especially if it is a statement at all remote from the experiential periphery of the field. (pp. 42, 43)

To this explication of holism by way of metaphor I should like here to contribute an analogy drawn from formal logic - in particular model theory. I have in mind here the compactness theorem, which tells us that if some infinite collection of sentences T (typically the theorems of some
axiomatic first order theory) is such that every individual sentence in it (or equivalently, every finite subset) has an interpretation under which it is true (a model), then there is an interpretation under which the entire set $T$ is true. What provides the point of analogy with Quine's claim that "in taking the statement as unit we have drawn our grid too finely. The unit of empirical significance is the whole of science " (p.42), is that the encompassing interpretation appropriate to $T$ as a whole cannot be extrapolated from, and in general is not determined by, the interpretations which we manage to assign to the individual sentences of $T$. Thus take for example Peano arithmetic supplemented by the additional non-logical constant (singular term) 'a' governed by the axiom schema $a \neq 0 ' \cdots '$. Every finite set of these axioms can be interpreted in the standard model of arithmetic, with 'a' taken to denote any natural number $k$ whose corresponding axiom $a = 0 ' \cdots ' \text{ does not occur in that finite collection. This guarantees an encompassing interpretation, but this interpretation can't be the standard model since the infinity of axioms } a \neq 0 ' \cdots ' \text{ force 'a' to denote some non-standard integer. We might describe this situation by saying that though we can interpret statements of $T$ individually, it is only when we look at theory as a whole that we get a proper understanding of what it's about. More examples might render this picture clearer, and particularly examples are needed which would make it plausible to suppose that it is generally applicable in the whole range of epistemology, and not merely with respect to scientific knowledge. A notion of 'theory' such that any bit of knowledge, even outside theories of science as ordinarily delimited, belongs to a theory requires
explication. But setting aside these cavils, I want now to consider how Quine, in contrast to Carnap, deploys this account to discredit any attempt at drawing a distinction between analytic and synthetic.

Quine takes it that once this holistic viewpoint is adopted,

it becomes folly to seek a boundary between synthetic statements, which hold contingently on experience, and analytic statements, which hold come what may. Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system. Even a statement very close to the periphery can be held true in the face of recalcitrant experience by pleading hallucination or by amending certain statements of the kind called logical laws. Conversely, by the same token, no statement is immune to revision. Revision even of the logical law of the excluded middle has been proposed .... (p.43)

Carnap and Quine seem completely in agreement as to their holistic picture of science. For Carnap, "it is, in general, impossible to test even a single hypothetical sentence. In the case of a single sentence of this kind, there are in general no suitable L-consequences of the form of protocol-sentences; hence for the deduction of sentences having the form of protocol-sentences the remaining hypotheses must be used." (p.318). And as to the underdetermination of change in the face of recalcitrant experience, Carnap has it that

if a sentence which is an L-consequence of certain P-primitive sentences contradicts a sentence which has been stated as a protocol-sentence, then some change must be made in the system. For instance, the P-rules can be altered in such a way that those particular primitive sentences are no longer valid; or the protocol-sentence can be taken as being non-valid; or again the L-rules which have been used in the deduction can also be changed. There are no established rules for the kind of change which must be made. (p. 317)
And more fully, and strikingly, bearing in mind the previous quotation from Quine:

No rule of the physical language is definitive; all rules are laid down with the reservation that they may be altered as soon as it seems expedient to do so. This applies not only to the P-rules but also to the L-rules, including those of mathematics. In this respect, there are only differences in degree; certain rules are more difficult to renounce than others. If, however, we assume that every new protocol-sentence which appears within a language is synthetic, there is this difference between an L-valid, and therefore analytic, sentence $S_1$ and a P-valid sentence $S_2$, namely, that such a new protocol-sentence - independently of whether it is acknowledged as valid or not - can be, at most, incompatible with $S_2$ but never with $S_1$. In spite of this, it may come about that, under the inducement of new protocol-sentences, we alter the language to such an extent that $S_1$ is no longer analytic. (pp. 318-319)

So Carnap has it that the interconnection between observation (protocol sentences) and theory (P-rules) mediated by logic and language (L-rules) is such that by suitable readjustment elsewhere in this scheme, any statement may be maintained in the face of recalcitrant experience. No statement is immune to revision. Quine supposes these considerations to demonstrate that no statement is analytic, while Carnap concludes, from the same considerations, that attributions of analyticity are not fixed absolutely, but are relative to particular choice of P and L-rules. Specification of P and L-rules is a choice, guided though not fixed determinately by the overall constraints and pragmatic considerations operative in the holistic enterprise of theory construction. Apparently Quine is arguing against a conception of analyticity which Carnap did not feel committed to defending. But perhaps Quine has an
argument to show that Carnap's conception of analyticity actually does commit him to the one Quine has established as incompatible with holism; or alternatively this might be a direct argument against the conception espoused by Carnap, but presumably not based on considerations from holism. Or if so, we have the third alternative that Quine argues that Carnap has failed to appreciate fully the lesson to be drawn from the holism he espouses. Before following out these points, it may be of interest to note that Grice and Strawson have precisely, even if unwittingly, allied themselves with Carnap as against Quine on this dispute (if that is what this confrontation arranged by juxtaposition of quotations can be called). Thus their concluding point in "In Defense of a Dogma" runs: "Now for the doctrine that there is no statement which is in principle immune from revision, no statement which might not be given up in the face of experience. Acceptance of this doctrine is quite consistent with adherence to the distinction between analytic and synthetic statements." (p. 157) Their argument is that the kind of revisability on which Quine's claim rests must consist in shift or revision of our conceptual scheme. Relative to such a scheme (corresponding to Carnap's notion of P and L-rules) some statements will indeed by analytic.

To facilitate the comparison and assessment of Quine's and Carnap's positions in this matter something needs be said in further exposition of Carnap's construal of (relative) analyticity. For Carnap part of specifying a (formalized) language \( \mathcal{L} \) consists, in addition to specifying a set of primitive symbols and a syntax of formation rules, of setting
out L-rules, or what in other stages of his development were sometimes called 'meaning postulates' or 'semantical rules.' A statement in $L$ is analytic just in case it can be established on the basis of the meaning postulates alone. So what are meaning postulates? What principles guide us in the selection of them? Intuitively they are supposed to encompass the principles of logic which we want to have govern discourse in $L$ and the interconnections between the primitive symbols of $L$ which are to hold in virtue of the meaning with which we intend to use them.

To illustrate,

suppose that the author of a system wishes the predicates 'B' and 'M' to designate the properties Bachelor and Married, respectively. How does he know that these properties are incompatible and that therefore he has to lay down the postulate: $(\forall x)(Bx \rightarrow \neg Mx)$? This is not a matter of knowledge but of decision. His knowledge or belief that the English words 'bachelor' and 'married' are always or usually understood in such a way that they are incompatible may influence his decision if he has the intention to reflect in his system some of the meaning relations of English words. ("Meaning Postulates", pp. 224-225).

Characteristic of Carnap's viewpoint in this passage is the attitude that how such postulates are laid down is a matter of decision; the decision will be taken in furtherance of some particular enterprise of rational reconstruction, whereby unclear discourse, which fails to forbid confused expression because the rules of the language are only tacit, is replaced by one in which the rules are fully explicit and set out in virtue of precise decisions as to what notions are to be expressible. If the ambition is rational reconstruction of an entire natural language, e.g. English, or a substantial part of it, I think the meaning postulates might be visualized as resulting
from taking a dictionary, replacing each word by a primitive symbol of appropriate syntactic category, e.g. 'loves' by the two place predicate (1)L(2), and then taking as postulates sentential forms of each dictionary entry. Of course Carnap never claims that just this could be done - a great deal of tidying-up would be called for along the way.

Quine does not attempt to argue that this putative conception of analyticity entails the view that analytic statements are precisely those which we can, or must "hold, come what may". I do not think such an argument could be made out, and the impossibility is demonstrated in Carnap's validly drawing as a consequence from holism the relativity of analyticity (without begging the question of whether his notion of analyticity is tenable).

The second of the three possible argument structures set out above is realized. § 4 of "Two Dogmas", on "Semantical Rules" constitutes a direct argument against the conception of analyticity in terms of language postulates which Carnap takes to be consonant with holistic vagaries, and that argument is not itself couched in terms of holism. Now of course Quine does not object that Carnap's device in terms of laying down rules is impossible, or unintelligible in its effect (certain sentences being assertable on the basis of them), but Quine faults this exercise for its failure to tell us what property sentences have in being so assertable, other than being so assertable: "We understand what expressions the rules attribute analyticity to, but we do not understand what the rules attribute to those expressions." (p. 33) Two counter-arguments are forthcoming, both expressed by R.M. Martin
in "On 'analytic' " Phil.Studies 3 (1952), pp. 42-47. The first is by
vidious comparison with the explications of the concept of truth in
particular languages on the general pattern indicated by Tarski. As
is pointed out correctly, each such definition is a different theory
particularly constructed for the language to which it is to apply. Tarski
has given general and broad hints so that we have some idea how to
proceed in each case, but in no sense has he given a definition of 'true-
in- £' for variable £ (on pain of contradiction, since it would be framed
in a particular language £* and would then constitute in £* a definition
of 'true in £*'). So too, it is claimed for analytic in £: Carnap
has given the general form of such accounts in terms of laying down
meaning postulates. The actual construction needs, of course, to be
worked out in terms of the language and purposes at hand for each
desired application. To this argument there is a decisive reply enuncia-
ted by Quine in the concluding paragraph to "Notes on the Theory of
Reference". The point forms the general basis for Quine's confidence
in 'theory of reference' as against 'theory of meaning'. It is simply
that for the enterprise of constructing particular truth definitions, our
efforts are constrained by the requirement of conformity with conven-
tion (T); we know when we have obtained a formally correct definition
or not. And we have general assurances that any such theory is a
theory of truth (even if, as I have argued, not as to the exact character
of that truth concept). For analyticity we have no such general constraint
or condition for success. No general argument tells us that any theory
of such and such form is a theory of analyticity for the language to which
it applies.
The second counter argument appeals to what can in general be achieved by utilizing the method of postulation. While we cannot say in advance for an arbitrary case what is to count as managing to characterize by a set of postulates some particular concept we wish to formalize, we have a good idea as we set out whether we have succeeded well or ill; in any case we are familiar with considerations which count in such an assessment. Quine might seem to concede something to this viewpoint in a paragraph added to "Two Dogmas" (after its original Journal of Philosophy publication) in response to the Martin article earlier cited. He remarks: "Now the notion of semantical rule is as sensible and meaningful as that of postulate, if conceived in a similarly relative spirit." (p. 35) But this apparent concession turns into the general objection already voiced against semantical rules (that in effect we lack guiding principles in picking them out), for he continues: "but from this point of view no one signalization of a subclass of the truths of L is intrinsically more a semantical rule than another." But his final restatement of the point following immediately on the remark just quoted seems in fact to collapse the entire matter back into terms of revisability, a point, as we have noted, which Carnap is explicit in avowing: "if 'analytic' means 'truly semantical rules', no one truth of L is analytic to the exclusion of another." On Carnap's construal of the holistic interplay of language, theory and experience choices will need to be made in the assessment of various considerations which on one decision a particular statement will be counted analytic, on another equally plausible, not. What embodies those contrary conceptions of analyticity are contrasting postulations of semantical rules.
Quine's objection must come down to something like the point that we do not know well enough what conception it is we are trying to capture to suppose that it guides us in the choice of semantical rules. Indeed, the enterprise is circular if the set of semantical rules is taken as the only way to suggest the notion they are ostensibly meant to capture. Rather than being an instance of the same process as choosing an axiom set for e.g. arithmetic, laying down semantic rules contrasts crucially with it. There is no notion of analytic-in-English about which we have anything like the highly developed and informed intuitions which guide us in the selection of first principles for arithmetic. Carnap might rejoin by instancing axiomatization of set theory (as Quine conceives it; a conception open to criticism - cf. D.A. Martin's review of Set Theory and its Logic - but one obviously fair to cite in this context). In view of the paradoxes, and perhaps also from epistemic and ontic consideration, Quine seems loath to take it that we are guided or constrained in our choice of axioms for set theory by a conception or intuition as to what is true in this abstract domain. So a parcel of disparate and inconclusive desiderata and viewpoints get taken into account - as elegance for deductions, perspicuity for metatheoretical investigation (these two generally conflict), ease in allowing interpretability in it of the whole, or some particular part of mathematics, evident consistency (=weakness), strength, ready comparability to other systems, etc., etc. To be prima facie an axiom system for set theory in Quine's view seems to require nothing more than apparent consistency and that it be about a two place relation '∈' which we can visualize as membership. Thus, as for NF, it is not even required that we be able to say
what the structure characterized by particular choice of axioms looks like. If Quine can countenance such a degree of indefensibility in the search after axiom systems for set theory, so the argument might run, why not also for semantics? As a concluding fillip to the line of these considerations, which aim to show Carnap’s use of stipulated postulates or the like in determining a notion of analyticity consonant with Quine’s own practice, it may be claimed that Quine’s doctrine that semantics depends on setting down undetermined "analytical hypotheses" is exactly Carnap's disputed account, or very like it. Discussion of this point requires first exposition of this key notion in Quine’s approach to semantics.

§ 3. Radical translation and the empiricist conception of language

Quine has set the problem of meaning in terms of accounting for radical translation, "i.e. translation of the language of a hitherto untouched people." (Word and Object, p. 28) The crucial point in the theoretical austerity of this construal is that no interpreter, or more usual in actual field work, chain of interpreter-native informants may contribute to the enterprise. The linguist then finds himself in a situation where "all the objective data he has to go on are the forces that he sees impinging on the native's surfaces and the observable behavior, vocal and otherwise, of the native." (loc. cit.) Clearly whatever can be so determined as to native meanings in such a setting is thereby entirely consonant with the strictest canons of empiricism; the very situation excludes appeal to any dubious mentalist items considered by some accessible through the medium of introspection. And
a conclusive argument that this delimitation of the problem of meaning does it full justice, even if other construals seem to make for any easier account, is that this enterprise can, and does actually, succeed. Quine cites work of Raymond Firth in this connection. (This point, that the construal is adequate, is weaker and so less controvertible than the tendentious empiricist claim that it must be adequate.) The jungle linguist comes finally to converse with the natives, to understand and be understood.

What allows radical translation to happen is situations in which the native and the linguist turn their attention to the same matter. The linguist elicits the native’s assessment of the situation - it will of course consist of a sentence in his own tongue which the native takes to be true in the circumstances; it expresses his belief. The linguist will (tentatively) translate it by the sentence of his language by which he describes the passing scene. So it is that from information on what the native believes an account is constructed as to what he means. Clearly an immediate requisite to any such interchange is recognition of what for the native counts as endorsement or denial of statements in his language. In § 7, of Chapter 2, Word and Object, "First Steps of Radical Translation", one sees that even this immediate beginning may be problematic: "how is he to recognize native assent and dissent when he sees or hears them? ... What he must do is guess from observation and then see how well his guesses work. ... If extraordinary difficulties attend all his subsequent steps, the linguist may decide to discard that hypothesis and guess again." (pp. 29-30) Having established likely candidates for assent and dissent, the field linguist may seek out
situations for correlating descriptions as previously described. At these early stages the variety of sentences which can be so entertained for translation will be rigidly restricted. They must be such as to be conspicuously about the very scene immediately present to the native and linguist as they stand together in field or forest. Should the native be moved to rhapsodize about the happy times of his youth associated with this spot, the linguist has no candidate sentence of his home language to correlate with the native utterance; similarly if the native remarks of a man who walks into view that he's a good hunter. At this stage the native must be constrained to offer just his **observation** of what's about, just to describe the situations which the linguist is himself at this stage in a position to describe. Such observations must be clearly attached to particular features of the surrounding scene. Much pointing, gesturing, handling of objects will be in order at this stage. What is wanted is **occasion sentences**, as against **standing sentences**, ... which **command** assent or dissent only if queried after an appropriate **prompting stimulation.**" (pp. 35-36) But further, we want occasion sentences "whose stimulus meanings vary under the influence of collateral information" (p. 42) - this to rule out "he's a good hunter". Occasion sentences satisfying this condition are for Quine **observation sentences**, "and their stimulus meanings may without fear of contradiction be said to do full justice to their meanings." (loc. cit.) There are subtleties to Quine's account which need not detain us, as e.g. the relativity of these categories to modulus of stimulation, "what bound we set to the length of stimulations counted as current." (p. 28) Also to be set aside at this juncture are vagaries of reference, basically
the old problem of how well ostension can succeed, and leading to Quine's doctrine of ontological relativity. These matters do not obtrude at this stage because e.g. "Gavagai" taken as a sentence has a translation "unique to within stimulus synonymy; for the occasion sentences 'Rabbit', 'Rabbit stage', and 'Undetached rabbit part' are stimulus-synonymous and holophrastically interchangeable" (Quine, J. Phil. 67 (1970), p. 132). Observation sentences, then, prove the entering wedge by which radical translation begins, and they are themselves completely translatable in the initial scheme of matching linguistic responses to prompting stimulations. There are three further elements in these early stages of the enterprise where on Quine's view the data is fully adequate to determine the outcome. Two of these are notions of stimulus analyticity and (intrasubjective) stimulus synonymy. A sentence is called stimulus-analytic "for a subject if he would assent to it, or nothing, after every stimulation (within the modulus)." (p. 55) Relativity to subject and modulus of stimulation render this empirically unproblematic notion, "a strictly vegetarian imitation" of the grander and more absolute notion Quine supposes those who would have analyticity are after. Quine cites in this regard divergences of stimulus analyticity for novice and expert numismatist in the identity of "Indian nickels" and "Buffalo nickels." Presumably one might also cite here the stimulus analyticity for G.E. Moore of sentences such as "There exists at present a human body which is my body." I will not have very much more to say about these notions. The third further behaviorally suitable element of radical translation, in Quine's view, is (classical) sentential logic; a point I shall dispute in some detail, most of which consideration will be deferred until the final section of this chapter.
These elements constitute the basis from which the project of radical translation is to be elaborated. Having thus broken into the unknown idiom at all constitutes a considerable accomplishment, but this breakthrough leaves a very long way to go. In particular it must be settled how the language allows by grammatical devices the formation of sentences more complex than the simple reports of direct observation and sentential combinations of them so far dealt with. Presumably the language will contain much vocabulary beyond the range of observation predicates heretofore encountered. In short, most of the work on a grammar and a dictionary for the still largely unknown tongue remains before us. But at least now there is a substantial constraint on this further work: it must proceed in conformity with the four elements already fixed. Any such putative grammar and dictionary so constrained are what Quine terms analytical hypotheses. They are described in general terms as follows:

There is no need to insist that the native word be equated outright to any one English word or phrase. Certain contexts may be specified in which the word is to be translated in another way. The equational form may be overlaid with supplementary semantical instructions ad libitum. Since there is no general positional correspondence between the words and phrases of one language and their translations in another, some analytical hypotheses will be needed also to explain syntactical constructions. These are usually described with help of auxiliary terms for various classes of native words and phrases. Taken together, the analytical hypotheses and auxiliary definitions constitute the linguist's jungle-to-English dictionary and grammar. (p. 69).

Two sorts of questions about analytical hypotheses immediately suggest themselves. The first will concern the detail of what they will be like in
actual application to a particular language. The second will seek elucidation of how the behaviorally scrutable elements of radical translation, in particular the fixing of stimulus meanings for observation sentences, serve in practice to constrain putative correlations of native words with the linguist's, to guide him in his choice of what indeed to count as a word, as a plural ending, as a tense operator, as notion of identity, and etc., etc. As to the first of these, one could not expect to find much within the relatively brief penultimate section of Chapter 2, which introduces the notion of analytical hypotheses. That almost the whole of the account of "Translation and Meaning" (eight sections) has been devoted to what in the end is but the bare beginnings of the project, with but a single section to the remaining realization of it may impress one as gross imbalance. The point must be that while the framework of radical translation largely ceases to be explicit beyond Chapter 2, the remainder of Word and Object might be taken as an account of it in the higher reaches of language, where grammatical structure becomes complex (and unobvious - as seen from Chapter 5 on "Regimentation") and subjects of discourse may be remote from immediate experience. Answer to the second question (how the base of stimulus meanings, analyticity and synonymy plus sentential logic constrains the projection of analytical hypotheses) constitutes Quine's doctrine of the indeterminacy of translation. But before going on to consider that matter, let me stop to put the point on behalf of Carnap, the suggestion of which has occasioned this exploration of Quine's notion of analytical hypotheses.

Applying Carnap's general picture of semantics as rational reconstruction to the particular objectives of radical translation, the following points
of consonance appear between Carnap's account as presented e.g. in § 82 of Logical Syntax and Quine's in Chapter 2 of Word and Object. Carnap's notion of protocol sentences seems comparable to Quine's observation sentences, first, as to what they are, second, in terms of our access to them (though nowhere of course does Carnap draw the matter with the sharpness and careful detail Quine brings to his treatment of observation sentences), and thirdly, as to the manner in which they provide the basis and constraint for construction of semantic theory in the less directly experiential reaches of language. Further, these semantic theories appear consonant as envisaged by both Carnap and Quine. Carnap's L-rules and Quine's analytical hypotheses seem to be called upon to do much the same thing, namely provide what in traditional terms is done separately by a grammar and a dictionary. Both Quine and Carnap do not attempt to present grammar and lexicon as separable components. Furthermore, and most crucially, the manner in which analytical hypotheses/L-rules interact with observation statements/Protokollsätze is represented for both Quine and Carnap in Duhem's holistic model of theories. And in accord with that model, matters of 'fact' are bound up with and influence the very theory (L-rules / analytical hypotheses) selected and even what basic statements (protocol sentences / observation statements) to accept as constraining that selection. Where then is there room for Quine's apparent disagreement with Carnap as to the notion of analyticity? Holism was seen to provide Quine's argument against the conception of analyticity which would equate it with 'held true, come what may.' This, as we have noted, is not Carnap's conception. Rather it is: that which can be established from the L-rules alone. We
have already seen an indecisive argument advanced by Quine against this conception in § 4 of "Two Dogmas". Is there an argument from holism which will count against this conception of analyticity?

The impact of holism on the Quinean scheme of radical translation is indeterminacy. And indeterminacy provides, in Quine's view, an argument against any conception of analyticity couched in terms of language, akin as that would be to the slightly cruder formulation: true in virtue of meaning. So it would seem that here at last is a holistic argument embodying what must be Quine's objections to Carnap's L-rule conception of analyticity. But Carnap explicitly shapes his account to the holistic employment of language in theory, resulting in a relativized notion of analyticity - relative to a selection of L-rules. We are led to consider the question: is relativized analyticity indeterminacy of translation?

Consider how the first steps of radical translation (in which a base of observational data establishes a notion of stimulus meaning) constrains the projection of analytical hypotheses by which the job is completed. The extent to which stimulus meaning underdetermines the choice of analytical hypotheses, to that extent there is indeterminacy of radical translation, for it is to be realized that any putative translation which does full justice to the empirical constraints of stimulus meaning is a correct translation. If two systems of translation diverge (in that they translate one and the same native sentence by two sentences of our language which are unequivalent in that we take one as true, the other as false) but both are in agreement on the translation of observation sentences (to
within stimulus synonymy), then they are both correct. Conformity to all possible evidence from observation of verbal and other behavior of the native speakers is ipso facto adequacy of translation. To suppose one correct as against the other is to suppose that there is a fact of the matter which outruns all possible evidence. Quine's famous slogan, "There is no fact of the matter" is an explicit, even if elliptic denial of this possibility. An unverifiable difference is no difference at all. Such is the basic anti-realist tenet of verificationism to which Quine subscribes. I will discuss later the ramifications for general themes central to this dissertation of Quine's adherence to this principle. This treatment of language exhibits striking affinity to Wittgenstein's mature conception, a large topic which I will not attempt to treat. Quine notes the consonance in a footnote: "Perhaps the doctrine of indeterminacy of translation will have little air of paradox for readers familiar with Wittgenstein's latter-day remarks on meaning." (p. 77)

Having established this framework which tells what it would be like were there indeterminacy of translation, we now must consider grounds for taking it that this situation is realized, or in any case realizable. The argument is from holism. Recall the general account of how radical translation is effected. Linguist and native in common turn their attention to some matter; the linguist translates the native's assessment expressed, of course, in the alien language of that situation, the sentence of the home language which the linguist takes as true in this matter. This model represents an idealization, or embodies a principal of charity which it might be supposed seriously distorts its plausibility: the native might
just be wrong in the matter. But imputing error is a luxury that can come only late in this game — and if we impute too much, it becomes increasingly plausible that we misunderstand him (a clue which ought, I think, to be utilized more in philosophical disputation). In any case, not only do we have no choice but to suppose early in the project that the native speaks only the truth, early it is entirely plausible that he does so; immediate observation leaves little scope for error. The methodology is ideally suited to these earliest steps in radical translation, and with good scientific method the correct account is achieved. The next step must be shrewd guesses about grammar and lexicon, a choice as to one set of analytical hypotheses — and then the attempt to determine whether the translations effected in accordance with this choice fit what has been so far established and result generally in matching truth for truth as the linguist, with his increasingly flexible command of native idiom, compares notes with the native on matters abstruse, or at any rate remote from immediate ken. The claim is that incompatible sets of analytical hypotheses may fulfill these demands. Consider the Duhemian underdetermination of theory by observation. As described earlier, two theories $T_1$ and $T_2$, may fit all the facts (observations) and yet be incompatible in assigning opposite truth values to a high level theoretical statement remote from experience. We may specify, speaking our own language (and the one in which $T_1$ and $T_2$ are both couched) which we opt for (on grounds of beauty, convenience, etc.). But there is no saying of the native whose speech we translate which he opts for. For consider statements of the native's language linked by theory to the observations which alike support $T_1$ and $T_2$. Translating such statements by incompatible theses of $T_1$
and $T_2$ may be fully in accord with the constraint: preserve stimulus meanings as given in observation sentences. As Quine has expressed it, "where physical theories A and B are both compatible with all possible data, we might adopt A for ourselves and still remain free to translate the foreigner either as believing A or as believing B" ("On the Reasons for Indeterminacy of Translation", p. 180). Now it might be supposed that once the linguist has proceeded far enough in his enterprise he can simply ask the native which theory he opts for. But the short answer to this ploy is that "no basis can be gained by interrogation in a theoretical vein, since the interrogation would take place in the foreigner's language and so could itself be interpreted according to either plan." (loc. cit.)

This argument from holism to establish indeterminacy of translation might, by excessive abstractness, fail to convince. As remarked in earlier pages, the very claim of holism may seem shadowy in the absence of concrete and detailed examples. Quine remarks boldly and abstractly that "physical theories can be at odds with each other and yet compatible with all possible data even in the broadest sense. In a word, they can be logically incompatible and empirically equivalent. This is a point on which I expect wide agreement." (op. cit., p. 179). Now on the face of it entertaining the prospect of theories empirically equivalent but logically incompatible seems to flout empiricist common sense. If their difference is beyond any possible experience, can this be a genuine difference? Now I suppose Quine confidently expects wide agreement as to this possibility on account of the general picture of scientific theorizing, in which masses of data by no means dictate
a particular explanation, and competing explanatory theories may, and frequently are put forward. An example, presumably, of this kind of conflict between theories which account equally for the observational data is Ptolemaic epi-cycled geocentric orbits vs. Copernican circular heliocentric orbits. And I take it that by sufficiently complicating the epi-cycles of the Ptolemaic theory it can compete even with Kepler's theory of the elliptic orbits. Now whether it can fit the data as well, not only with respect to the observations available to Kepler, but to any possible observations of planetary motion seems to me somewhat doubtful. But it is this stronger claim which Quine's holism would demand. Or rather, I am not certain what the answer to this question is with observations of planetary motions restricted to the earth as vantage point. But if not so restricted, as with a telescope equipped helio-satellite launched perpendicular to the plane of planetary orbits, then it certainly will be that particular observation sentences will decide in favor of Kepler and against Ptolomy. What will be readily agreed is that there are cases where conflicting theories were on a par as to compatibility with then available data. General relativity seems for a short time to have been in this situation. But Quine's claim is "that physical theory is underdetermined even by all possible observations", where this latter notion is glossed by the directions: "apply dates and positions to [occasion sentences that are suited for use in reporting observable events in the external world] in all combinations, without regard to whether observers were at the place and time. Some of these place-timed sentences will be true and the others false, by virtue simply of the observable though unobserved past and future events in
the world." (loc. cit.) I find it difficult to imagine two theories so strongly in agreement and not being thereby equivalent theories. But perhaps Quine's argument does not actually require this unrestricted appeal to counterfactuals and subjunctive. The supposition that it does must come from the notion that were such unactualized but possible observation capable of turning up, then whether we actually hit upon it or not, it constitutes grounds for a fact of the matter as to correct translation. But perhaps all one need suppose is that no observation accessible to the native whose speech is being translated be such as to decide between conflicting theories. Then an example of indeterminacy in pre-Twentieth century field linguistics would be non-observational statements of astronomy. By suitably juggling the whole scheme of analytical hypotheses one could, in conformity with translation of all possible (for that community) observation sentences, translate a particular sentence as 'the earth goes around the sun', or equally as 'the sun goes around the earth'. It may be that Quine would not accept this emendation, on grounds that the linguist could query the native as to the expected truth value of an observation conducted from a vantage point, say at 100 million miles above the plane of planetary orbits. But this, it seems to me, hardly could be managed without substantial utilization of analytical hypothesis. In particular, identifying a native language device of subjunctive conditionalization must belong to non-observationally fixed ranges of the project.

Further doubts might attend Quine's argument from holistic under-determination of theory to indeterminacy of translation. There seems a linguistic-centrism or assymetry in the account akin to the supposition
in philosophy of mind that I have access to my private mental states directly, and so can distinguish between situations which are behaviorally inscrutable (and so non-existent?) for me in others. I have in mind the supposition that we can actually adopt one as against the other of two conflicting but empirically compatible theories, whereas in translating the foreigner there is (for us?) no fact of the matter which he has adopted. But such is the situation Quine appears to envisage in the (previously quoted) remark: "we might adopt [physical theory] A for ourselves and still remain free to translate the foreigner either as believing A or as believing B." Resolution of this puzzle proceeds from the point that Quine's program of radical translation begins at home, as he himself insists (e.g. "Ontological Relativity" p. 46). Interpreting the speech of my English speaking compatriots, I generally 'translate' by the most obvious and reasonable selection of analytical hypotheses: complete homophony. But reasonable though that selection proves on the whole, it is no more the correct one than is the first one we hit upon which works (fits all the data) for a remote jungle language. And there are times when what seems like massive disagreement and espousal of absurd positions suggests my ostensible co-linguist isn't using language as I do, and some non-homophonic analytical hypotheses will render his position more plausible and thereby intelligible. It is a common enough experience for a hotly disputed point to be resolved not by one party winning the other over to a new assessment of what is true but in the realization "Oh well, if that is what you mean, then of course (but I would have put it this way)." But it is crucial in understanding indeterminacy to realize that the facts do not force me to such a resolution; it's just that if it is
relatively easy to do, then pragmatic social desiderata such as "maximize agreement" recommend doing so. On the other hand, if our dispute over what is true appears to be my Keplerian to his Ptolemeic astronomy, the amount and complexity of non-homophonic re-translation to produce accord may be so vast and difficult as to disincline me in the effort. It is simpler to take him as believing Ptolemy's wildly epicycled theory than to suppose that for genetic reasons which are not apparent to me, he uses English in a massively and complexly, even if systematically, different way from the way I do.

These considerations may illuminate the previous puzzle as to how a good empiricist could even envisage the possibility of two theories logically incompatible and empirically equivalent. He does so on the supposition that the two theories are couched in language inter-translatable by homophonic analytical hypotheses. But the facts do not force this viewpoint, and it may be supposed instead that one has two uses of language interconnected by non-trivial, complex analytical hypotheses in which the same beliefs about the world get expressed. As above, pragmatic considerations from e.g. principles of minimal effort may militate against the latter construal, and lead us to think of incompatible theories. But it is, I take it, worth our while to frame the non-homophonic analytical hypotheses which effect translation between the ranges of language in which relativistic and classical mechanics are expressed, so as to be able to make sense of the claim that, with respect to bodies not too small and moving not too fast, the two theories say the same.
The problem of how 'we' can make choices between theories which we cannot impute 'them' making has been resolved so as to ensure uniformity in account of meaning both as regards fellow members of my speech community and speakers of a tongue entirely alien to me. But what about 'me'? Is not the account still committed to an ego-centric asymmetry? For I can translate between the language of the competing theories homophonically and then opt for Kepler's elliptic orbits, or I can translate complexly and say the two theories come to the same thing. The apparent ego-centrism manifests itself in that it is a fact of the matter, and one known to me, which I do. The answer here is simply that this is not the fact of the matter whose existence is disputed in the thesis of indeterminacy. What is claimed is that there is no getting it right, in terms of fitting all relevant observations, as to which of these options I explicitly and knowingly adopt. And indeed I may have good reasons for which I do adopt, only those reasons will not be fidelity to observations.

Before concluding the present account and discussion of Quine's doctrine of indeterminacy of translation with consideration as to problems of reference, let me add a few remarks concerning the example of incompatible astronomical theories. One might suppose that responses to the querying of particular observation sentences can settle it for the radical translating linguist whether the native is a helio-, as against geo-centrist, or conversely. This point is additional to the difficulty over 'bird's eye' direct observation of the solar system, for which I already adduced mollifying, if not conclusive considerations. For is not the two-place relation 'a goes around b' a perfectly good
observation predicate whose stimulus meaning is determinately pinned down (observing a dog chase around a tree) in the first (pre-analytical hypotheses) phase of radical translation? Also occurring in that phase is determination of native names for sun, earth and the various planets visible to him. May we not then find the native uttering assertively a sentence $\psi$, as talk turns to planetary matters, which the linguist now has grounds to translate as "Venus goes around the earth." The reply must be that with native and linguist restricted to earth based observations, this utterance is not itself an observation statement. That it has something to do with the motion of the planet Venus is for the linguist a reasonable supposition, but the observation sentences involving Venus are all reports of its position as sighted at various times. And so to interpret $\psi$ we must go beyond the domain in which stimulus meanings are adequate. Though its superficial appearance is as an observation sentence, it is actually remote from direct observation and thereby theoretical. With the linguist and the native in complete agreement as to observations of Venus, and the linguist firmly convinced that Kepler's account is best to codify those observations, and impressed with the general acumen of his native informant, he may want to suppose him likewise inclined to the helio-centric picture. Then $\psi$ must be interpreted according to analytical hypotheses which give a highly complex treatment to the native word whose stimulus meaning is 'goes around'. Alternately, the linguist may impute to the native what in his view is bad science, but makes for a simpler manual of translation. The fact that Kepler's and Ptolomy's theories are not decidable by particular (earth bound) observations means likewise that no observation can decide between these two translations. What does decide
between them is considerations of simplicity of translation manual on the
one hand, and, in this case, oppositely, confidence (on the part of the
linguist, and by his standards) that the native practices good science.
These considerations suggest points relevant to, though not it seems con­
sonant with some of my remarks in § 5 of Chapter 2 on the analogical
extension of sense to the use of observational language with reference to
remote situations.

The grounds for indeterminacy of translation come then at least initially
from reflections as to the character of scientific theories, in particular
their underdetermination by observation. But there are those who would
find it quite wrong-ended to argue from the remote reaches of science,
with its own peculiarities, to the general character of all language. And
Quine does allow that "if you were one of those who saw physics as empirically
underdetermined only in its highest theoretical reaches, then by the argument
at hand I can claim your concurrence in the indeterminacy of translation only
of highly theoretical physics" ("Reasons", p. 181). Whether considerations
such as this ought to influence our view of the nature of meaning in everyday
discourse seems closely akin to the issue whether relativity theory need
be taken into account, let alone be determinative, in our attempt to analyse
the nature of human perception in space and time. Strawson has, I take it,
argued to the negative in respect of this latter case, and would, it seems
likely, do so also in the matter of language theory. But Quine does attempt
to provide grounds for taking it that the phenomenon of indeterminacy manifests
itself in language near to home and everyday experience. Such is the point
of his "Gavagai" example, but mediately so. "Gavagai" is not itself a
sentence whose translation is indeterminate. It is an observation sentence
which has a translation "unique to within stimulus synonymy, for the occasion sentences 'Rabbit', 'Rabbit stage', and 'Undetached rabbit part' are stimulus synonymous and holophrastically interchangeable" ("Reasons", p. 182). What this situation does show is behavioral inscrutability of reference; ostension can only go so far in determining what is ostended. Such is the doctrine of ontological relativity. The relativity is to analytical hypotheses. Once we isolate certain morphemes of the native language and hypothetically identify them with our devices of plural ending, counting numerals, pronominal reference, identity, (the words 'some' and 'other'), 'physical object', 'thing' and the like, then we can supplement ostension by interrogation and settle the divided reference of the native's general observation terms. 'Is this the same as that?' 'Are there two gavagai here or one?' At least this is what we suppose we are asking. But precisely because reference is behaviorally inscrutable at the level of observation sentences, there is no getting it right on the analytical hypotheses in terms of which we think these are the questions we are asking. Take establishing the counting numerals. We try counting off collections on our fingers. The native volunteers words at each stage. He might be volunteering not the early ordinals but the name of each finger. It may be that fingers are always considered composite, a collocation of the three joints, so we're learning how the natives count in threes, or maybe it's two items, the nail and the skin, so he is counting by twos. And so also with attempts to individuate by counting off "gavagai". These examples are disposed of in practical terms because the linguist operates with maxims the like of "an enduring and relatively homogeneous object, moving as a whole against a contrasting background, is a likely reference for a short expression"
"Ontological Relativity", p. 34). What is crucial to realize is that choice between analytical hypotheses which establish the apparatus of reference and individuation are on the basis of such maxims, and not dictated at the level of stimulus meaning for observation sentences. At that level 'Gavagai' indistinguishably picks a rabbit of which there is one before us, or rabbit parts of which perhaps the native ontology would have it there are five.

So far in this account of ontological relativity no mention of translation and its indeterminacy, but the application now is fairly immediate. What the inscrutability of reference may be taken to show is that certain analytical hypotheses are underdetermined by that part of translation which can be established to within stimulus synonymy. Consequently we might expect that non-observational statements involving the vocabulary governed by those hypotheses could be translated incompatibly according as one or another empirically acceptable set of analytical hypotheses is adopted. Quine is unforthcoming with an example, but the following seems a candidate: there might be a single native sentence θ which on one choice of analytical hypotheses is translated as "a rabbit consists of undetached rabbit parts," while on choice of other analytical hypotheses, equally justified by observations, it comes out as "an undetached rabbit part includes a rabbit phase." The former seems to us true, the latter false (or perhaps tending a little toward nonsense). The considerations between the two translations cannot turn on further observation and interrogation of the native, since what would need to be decided between the two translations is a matter of establishing one extension as against a stimulus equivalent one for observation terms. Rather the choice will be by considerations similar
to those in the case of discourse on astronomy. Here the non-observational considerations seem, by and large, to point to the first, as maximizing agreement between native and linguist and also allowing the simplest manual of translation. Poorly developed though this example may be, it at least illustrates the notion that low-level, near to home statements may also exhibit the indeterminacy of translation. It should be realized that while the sort of considerations may seem to differ by which these high and low level examples of indeterminacy are arrived at, they are both examples which exploit the same phenomenon: incompatible statements interconnected by indirect links to the same observational reports. We might not think of "a rabbit consists of undetached rabbit parts" as a theoretical statement, but it is in the sense that while anchored in its connections with observational statements it is not itself one. It is to that extent remote from immediate experience. It is considerations such as these, aiming to show the theoretical character of greater and more ordinary ranges of discourse than commonly supported which Quine has come to call "pressing the doctrine of indeterminacy from below" ("Reasons", p. 183). The argument 'from above' in terms of e.g. physics and astronomy may be expected to command assent to the very idea of indeterminacy. The arguments from below aim to show it is a phenomenon we encounter in every day discourse, long before we reach the rarified atmosphere of the theoretical sciences. Considerations of ontological relativity are important, then, not as the basis for believing there is such a phenomenon as indeterminacy, but for taking it to be a phenomenon operative in even the most homely ranges of language. Ontological relativity maximizes the scope of indeterminacy in another
dimension as well, be it noted, for now we see that it "cuts across
extension and intension alike" (Ontological Relativity", p. 35). Not
only is there no fact of the matter as to meanings, there is none either
when it comes to reference; these two points are seen to be intimately
bound together.

Indeterminacy of translation entails the untenability of significantly
identifying some sentences as analytic: i.e. "true purely by meaning and
independently of collateral information" (Word and Object, p. 65). Since
there is no fact of the matter in rendering a correct manual of translation,
there is no fact of the matter as to what an individual sentence means, and
hence no determinate class of sentences of the native language which are
true because of what they mean. It may seem there is a lacuna in this
argument. Might it not be that while observation fails to pin down a
uniquely correct manual of translation, nonetheless for each of the ones
which are allowed, the same class of sentences are established as true
just in reference to it? The reply is that were this the case, the variant
manuals would, in a sense stronger than 'merely fitting all observation',
be equivalent, and meaning would thereby be fixed, translation determinate.
The variant manuals of translation would differ just in the way equi-
valent but non-identical axiom systems (e.g. for the theory of groups) may
differ while delivering precisely the same theorems. The incompatibil-
ities in translation which previous argument has aimed to show possible
rule out equivalence in this sense between all manuals of translation.

I now return to comparison of Quine's position on these matters with
that of Carnap. As noted, it is tempting to identify Carnap's Protocolsätze
with Quine's observation statements, and the L-rules with a set of
analytical hypotheses. Both men assess in this context the impact of holism. For Quine it leads to indeterminacy of translation and untenability of any notion of the analytic. Carnap draws the lesson that decision as to what L-rules we set down cannot be fixed once for all, but is subject to revision in the light of experience, both by observation and from general considerations as to the overall theory. This conception carries with it for Carnap a notion of analyticity (true in virtue of L-rules), but as the L-rules cannot be taken as fixed, it is then a notion relative to particular selection of them. Do Quine's strictures against analyticity count against Carnap's conception of it, or only against an absolute notion? Conversely does Carnap's relativized notion of analyticity constitute the doctrine of indeterminacy of translation?

Let us consider ostensible arguments from a Quinean perspective which would fault Carnap's conception. But before launching into that let me add the observation that Quine's program of regimentation of language would appear closely comparable with Carnap's rational reconstruction of scientific discourse. Both enterprises are immanent (in Quine's use of the term in Philosophy of Logic, p. 19 and passim) to particular languages - and to particular purposes. I will consider three arguments.

The first one, envisaged earlier (p. 196) as a possible response to Carnap's apparent pre-empting of the characteristically Quinean view of language, is that Carnap has failed to appreciate fully the lesson to be drawn from the holism he espouses. This, it will be claimed, is manifest in his supposing that there is, in constructing a language system, some non-arbitrary distinction to be marked by labeling certain rules of the
system 'L' and the others 'P'. For this is, of course, the attempt to separate out the component which meaning (logic, in a broader sense than Quine countenances for the term) on the one hand, and facts on the other, contribute to truth. For a given statement S established within the system, facts have contributed precisely in the physical principles appealed to in its demonstration, and the meaning factor is identifiable with the logical principles occurring in that demonstration. We need not accuse Carnap of quite such a crude, purely nomic account of factual component in truth, since the P-rules are merely the codification of currently accepted science, and the full play of inductive vagaries will enter in the account of how it is we come to accept the P-rules we do. But still, it will be claimed, to specify a principle in our language (system) as a P-rule represents an assessment that the facts make it reasonable to assert it. And to call a principle an L-rule comes to the view that it embodies a principle of meaning to be reflected in the language system under construction. But that there is no such distinction to be drawn is precisely the lesson from holism via indeterminacy of translation. Of course, the immediate reply for Carnap is that the lesson has been learned, as manifested in the idea that "no rule of the physical language is definitive; all rules are laid down with the reservation that they may be altered as soon as it seems expedient to do so. This applies not only to the P-rules but also to the L-rules" (The Logical Syntax of Language, p. 318). But then the objection may be put from the Quinean side, if the distinction is admitted not to be determinate, then it must be an empty and arbitrary ritual to mark accepted principles with the labels 'P' and 'L'. One has simply a total system of basic principles taken as sufficient to encompass
the broad range of current knowledge. This charge will be rejected by citing Quine's own appeal to analytical hypotheses in achieving for a particular language an account of the meaning of its sentences. Though the facts do not dictate what those hypotheses are, that we choose one of the permissible sets of them is dictated by the need to get on with the job of translation. Making such a choice means selecting certain principles as constitutive of meaning. And surely setting out the L-rules, or meaning postulates, of a constructed language system in Carnap's conception of semantics is doing just this. Though it may be a matter of choice which route we follow in getting from Seattle to San Francisco, if we mean to get to San Francisco it has to be by some road, even if it's not the road. And once we are on it we can do the sightseeing that goes with it. That there is no such thing as the sights one sees in getting from Seattle to San Francisco, does not mean we can't enjoy the ones which our path does make accessible to us.

These remarks might seem to belabor a point which Quine would not be inclined to dispute. But note that Quine's most generous admission as to analyticity is his "strictly vegetarian imitation" by stimulus analyticity, and he clearly does not expect this to satisfy carnivorous adherents of an analyticity grounded in rules of language.

Quine's resistance to any construal of analyticity in terms of rules of language relates to the close identification of that notion with linguistic convention. There are two sorts of objections to a concept of conventions governing linguistic behavior, one of which subsumes the other. The first relates to the notion of analyticity and indeterminacy of translation.
The second point turns on features peculiar to convention and general reflections as to what constitutes language. This point betokens substantial and deep difference in conception between Quine and Carnap.

As to the first of these considerations, it is immediately evident that if language does operate in virtue of actually established conventions, then analyticity and translation are alike determinate, and the correct account of each is a matter of applying the inductive techniques of science to establish explicitly what those conventions are. Of course no one believes for a moment that founding fathers of language convened in linguistic congress to agree on conventions, which it is the modern linguist's task now to make explicit—any more than political theorists would seek after the text of an historical social contract. But the notion in both political and linguistic theory is that the relevant phenomenon are ordered as if there were such antecedent explicit agreements. Underlying linguistic convention is sometimes thought to be rendered more probable, or in any case clear, by pointing to actual cases where language is altered or expanded by an explicit act of convention, typically the introduction of scientific nomenclature, but also more literary or everyday pursuits, e.g. Horace Walpole's "serendipity". Clearly, the fact that convention has an element of arbitrariness (might have been otherwise) would in no way whatever constitute an explanation of Quinean indeterminacy of translation. This indeterminacy infects language just as it stands. It is not just that it might have been different; there is no uniquely right answer to saying how it is actually.

Quine's reservations about linguistic convention extend beyond the point that there aren't any. In the case of accounting for legal/moral principles
from the consequences of there having been an occasion for entering into a social contract, it at least makes *prima facie* sense to suppose the contracting actually happened. Such is not the case for a linguistic contract; for in supposing deliberation and agreement, we cannot answer the question 'what language was used in these deliberations and to signal agreement?' In any case no answer is forthcoming without regress, and a vicious one at that (cf. Quine's Introduction to *Convention* by David Lewis). This argument may convey a certain air of casuistry. After all, one may feel, if we can find the notion intelligible and useful in the full knowledge that it concerns a fiction, why need we worry over the issue how that fiction might have been realized, how the conventions of language came to be formulated and accepted? Or rather, knowing the linguistic contract to be a myth, why not an equally mythical account of how it came to be promulgated. We might suppose that God drew up the whole scheme (a long list of L-rules), and then implanted them in Adam and Eve on creating them along with the capacity to utilize them for communication. What makes this myth useful, so it will be claimed, is that the present situation of linguistic behavior is 'explained' by it, in the sense that what is now to be observed can be accounted for by coming up with what would have been on that list of L-rules. This story is roughly Carnap's picture of language. It is a picture decisively rejected by Quine as deeply incoherent; that rejection is crucial to his account of what empiricist language theory consists in.

For rhetorical effect it might be put that what separates Quine and Carnap in their conceptions of language is encapsulated in a four letter suffix: *formal vs. formalized* languages. For Quine language is *always*
a natural language. It is embodied in the complex dispositions to verbal and other behavior manifest in a community of human beings. Formalized language results when we regiment the language or ordinary discourse for one particular purpose or another. And even that process is merely a matter of self-consciously careful performance of linguistic practices already part of everyday discourse: "opportunistic departure from ordinary language in a narrow sense is part of ordinary linguistic behavior" (Word and Object, pp. 157-158). "To paraphrase a sentence of ordinary language into logical symbols is virtually to paraphrase it into a special part still of ordinary or semi-ordinary language; for the shapes of the individual characters are unimportant" (op. cit. p. 159). Synonymy is not to be demanded between original and paraphrase, partly of course because "we seek not a synonymous sentence but one that is more informative by dint of resisting some alternative interpretations." (loc. cit.) But this stems more deeply from the general limits imposed on any notion of synonymy by the phenomenon of indeterminacy. The principles which are set down to effect regimentation are analytical hypotheses for translation between two idioms; they are not merely formal rules governing the formal language. They constitute the principles by which our everyday idiom is, for whatever particular purposes, formalized.

In contrast, Carnap's meaning postulates for a particular formal language $\mathcal{L}$ are not conceived as tying $\mathcal{L}$ in its inception to the particular language we speak. They create $\mathcal{L}$ as a formal language; we then face the pragmatic choice of whether to adopt $\mathcal{L}$ in our rational reconstruction of e.g. discourse in physics or geology. But notice that this conception of what one is doing in creating $\mathcal{L}$ by specifying its $L$-rules actualizes the myth of a linguistic
contract, with Carnap (or whoever chooses the L-rules) as God. And while we might forbear to demand what language the divine mind used in its reflections as it set about constructing this marvelous device (a language), we feel no such reticence when it comes to Carnap. The impossibility of satisfying such a demand gives substance to Quine's claim that labeling an ink covered sheet of paper with the mark "Semantical rules for L" cannot be illuminating as to the concept of analyticity or meaning in L (§4, "Two Dogmas"). Identifying Quine's notion of analytical hypotheses with Carnap's semantical rules, meaning postulates, etc., must be rejected as false consonance. For Quine, the analytic hypotheses, and for Carnap, the meaning postulates endow the new formal idiom with meaning. But there is serious dissonance in how this is seen as coming about. For Quine any such meaning is carried over from the language we actually speak. Carnap's picture is of semantical rules which are constitutive of a language in that they endow with meaning a collection of strings of symbols formed by certain other (syntactic) rules. Supposing for the moment that meaning postulates could do that, so that a (formal) language is thereby created, its proving efficacious to us, on Quine's conception, will be dependent upon establishing a translation between it and the language we speak. So analytical hypotheses would be called upon in addition to the meaning postulates by which the formal language L is established. But now we face the question how is translation between English and L to be established? Ex hypothesi is a language radically alien to English. Or at least, on Carnap's conception, it may be; for Carnap takes it as entirely a matter of choice whether the meanings with which L is endowed by laying down semantical rules reflect many, or even any meanings from the language we who construct the system.
happen to speak: "knowledge or belief that the English words 'bachelor' and 'married' are always or usually understood in such a way that they are incompatible may influence his decision if he has the intention to reflect in his system some of the meaning relations of English words" ("Meaning Postulates", as reprinted in Meaning and Necessity, p. 225). So let us consider the case where a constructed formal language $L$ is radically alien to English, in the sense that we find ourselves with no prior hints as to how translation between the two might be effected. But now we come up immediately against the impossibility of applying to this problem Quine's schematic account of radical translation. There is no stimulus meaning to be established through observation of linguistic and other behavior, and so no analytical hypotheses to project from that basis, for there is no linguistic behavior. As Quine remarks in Philosophy of Logic (p. 101): "Carnap ... has represented language as analogous to a formal deductive system. ... We do better to abandon this analogy and think in terms rather of how a child actually acquires his language." A similar thought is expressed by Wittgenstein: "language is not something that is first given a structure and then fitted onto reality" (Philosophical Grammar, p. 89). Compare this with Carnap's remark in the section of Logical Syntax of Language entitled "The interpretation of language" (p. 227):

There are two ways in which anyone may learn to use a language as a language of communication: the purely practical method which is employed in the case of quite small children and at the Berlitz school of languages, and the method of theoretical statements or assertions, such as is used, for instance, in a text-book without illustrations. In the present [and indeed all subsequent] work, by the interpretation of a language we shall always mean the second procedure, that is, the method of explicit statements" Those explicit statements are, of course, the L-rules.

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But if the meaning of sentences of $\mathcal{L}$ is not manifest in dispositions to verbal and other behavior on the part of those for whom it is a language, in what does it consist? The answer would seem to be extraordinarily platonistic, as that term is applied in the philosophy of mathematics. It seems we must suppose them abstract entities picked out from a realm of all such (possible?) entities by satisfying the meaning postulates set down for $\mathcal{L}$. The platonist conception of mathematics embodies a serious epistemological problem as to how human beings interact with and obtain knowledge concerning abstract mathematical entities. But even supposing these problems settled, a serious puzzle will remain as to how meanings in such an abstract conception are grasped and expressed. From Quine's point of view the whole problem and picture which has engendered it is thoroughly incoherent. Meanings just are nothing beyond what happens in meaningful discourse. As Quine remarks in "Carnap and Logical Truth" (p. 390): "Any acceptable evidence of usage or meaning of words must reside surely either in the observable circumstances under which the words are uttered (in the case of concrete terms referring to observable individuals) or in the affirmation and denial of sentences in which the words occur." On an alternative picture meanings are out there, selected by L-rules, and grasped by human minds in accepting the L-rules. Quine's thoroughly empiricist view of language and meaning rejects this entire picture. There is no fact of the matter beyond the use we who speak and understand the language make of it. And to accept this consequence fully is to accept indeterminacy of translation.

Faith in the thoroughly scientific character of formal system building may have seduced Carnap away from the tenets of empiricist language theory,
for anything scientific must be empirical. The result is to suppose that the technique of axiomatic characterization of mathematical structures extends to the postulational determination of meanings. Even if platonism proves a tenable and indeed from certain vantage points attractive philosophy of mathematics, hypostatizing meanings is quite another matter which must be counted a dogma against the background of thoroughgoing empiricism, an empiricism which embraces not only physics and psychology but the study of language itself. Carnap's conception of a realm of meanings accessible through postulation of semantic systems raises a serious question as to how human beings come to grasp these meanings, understand the sentences to which they attach. In my initial picture (caricature) of conventional specification of language by God (free of any need to deliberate over how the system is being set up - pure act of intuition ??) this problem was dealt with by a notion of 'implanting'. Carnap of course cannot appeal to such a notion because his conventionally structured languages do not in general get taken up by any given speech community; they don't become natural languages. It might be said that Chomsky has availed himself of such an account. Quine's disagreement with Chomsky is as to what, from the standpoint of an empiricist scientific methodology, may be taken as the reality which linguistic science seeks to discover. But to pursue this matter would divert us too far from current concerns.

To conclude this discussion of language and convention, let me remark very briefly on work in these areas by David Lewis. Lewis appears engaged in a program to rehabilitate and then utilize the various language concepts which Carnap thought available through appeal to formal languages. His first book, *Convention*, provides an account of that notion which Quine concedes
in his forward, leads one "to appreciate convention ... as a key concept in the philosophy of language." The notion of convention there developed (from the basic condition of indifference - red light could have meant 'go' for all we care - and utilizing T.C. Schelling's theory of games of coordination) belongs to a behaviorally scrutable theory of communication consonant with conception of language. It has little to do with Carnap's austere and abstract notion of unspoken languages. What can be done with this notion of convention proves limited: "A verbal signaling language $\mathcal{L}$ has been called a language, and rightly so. But it is a rudimentary language, for at least the following reasons " (Convention, p. 160). Then follows a long list, of which perhaps the most important is "there is only a closed, finite set of sentences of $\mathcal{L}$. Truth conditions can be given sentence by sentence. There is no way to create a new sentence, with its truth conditions, out of old parts " (loc. cit.). To rectify these shortcomings Lewis goes on to an account of far richer possible languages with semantics given by Carnapian meaning postulates whose interpretation is given in terms of possible worlds. He considers this the first step in specifying a possible language, and takes a language to be so specified "in abstraction from any users it might happen to have, and in abstraction even from the question how it would be used if it had users. Later we shall consider what it is for a population to use a language - in other words, what makes a possible language someone's actual language " (op. cit., pp. 161-162). But from the perspective of Quinean objections to Carnap's conception of language, Lewis' answer to this question is disappointing. It comes to the point that "the convention of language whereby the members of a population P use a given possible language $\mathcal{L}$ may best be described as a convention of truthfulness in $\mathcal{L}$" (p. 177). This point,
as far as it goes, seems largely unexceptionable (just subject to the usual caveats about non-indicative moods). But what needs an account, and seems not to get it in those concluding pages of Lewis' book, is how a population might come to know what it is to say something true utilizing the abstract structure $\mathcal{L}$ - in other words, how might they come to understand a 'language' created in complete isolation from language users? Lewis has devoted a substantial article (55 pages typescript) ostensibly to this very issue: "What is the connection between what I have called languages, functions from strings of sounds or of marks to sets of possible worlds, semantic systems discussed in complete abstraction from human affairs, and what I have called language, a form of rational, convention governed human social activity?" ("Languages and Language", p. 7) Yet the crucial issue which is here of concern is barely addressed within the compass of that article, despite the promise of that opening formulation. Lewis, with Carnap, seems in a curious way oblivious to the essentially empiricist conception of language with animates Quine's study of human linguistic behavior. It is a conception which finds resonance in the concluding line of Strawson's inaugural lecture "Meaning and Truth": "as theorists, we know nothing of human language unless we understand human speech" (Logico-Linguistic Papers, p. 182).

§4. Quine as anti-realist

It is time now to engage again more directly and explicitly in considerations of the general themes to which this thesis attempts to address itself. This is not, of course, to allow that the preceding excursus on Quine's stricures as to analyticity and related notions is not relevant to those themes - only that
the connection may not have appeared immediate or explicit. Recall that this discussion began with Quine’s rejection of sentential verificationism, a doctrine, as we have seen in Chapter 2, to a certain degree exemplary of anti-realism. That rejection is founded in a conception of language, seemingly shared by Carnap, but for Quine revealing also the untenability of maintaining a genuine difference of kind marked by the traditional terms 'analytic' and 'synthetic'. Analyticity remained for Carnap the key device of language theory. It has been my endeavor in the previous pages to sort out in this situation agreement and disagreement, apparent and substantive. In the end it has seemed illuminating to understand the divergence between Quine and Carnap as over what constitutes language. The issue of analyticity proves but the rallying cry, the popular (and unthinking) slogan which betokens a deeper conceptual contrast. Whether language (to put it with misleading crudeness) is anything other than the verbal and concomitant behavior observable in human beings and dispositions to that behavior seems indeed a fundamental matter, insight as to which illuminates a wide range of issues. So it is that the preceding discussion of Quine's view of analyticity contributes to the general development, first in connecting directly with his rejection of sentential verificationism, and secondly as providing a general appreciation of his conception of language and nature of language theory. I want now to apply that conception of language directly to the issue of truth, in the context of opposition between realism and anti-realism.

As we have seen, crucial to Quine's theory of language is the holistic structure of human knowledge. The image of sentences embedded within encompassing theory and theory underdetermined for boundary conditions of observation provides key elements to Quine's rejection of any determinate
notion of analyticity. So too, this image defeats sentential verificationism, for to be embedded in underdetermined theory leaves no basis on which to apportion the empirical content of a single sentence. Verificationism tried after a fashion (if crudely and even incoherently) to be an anti-realist semantics. Nonetheless, I want to claim that this holistic account which defeats verificationism constitutes in itself a decisive and powerful expression of what is essential to an anti-realist conception of truth. The point emerges if we review the argument from holism by which sentential verificationism is defeated. The basic feature is that "any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system... Conversely, by the same token, no statement is immune to revision" ("Two Dogmas", p. 43). This systematic flexibility reflects the situation that sentences must be assessed as to truth and falsity only as elements of an encompassing theory: "our statements about the external world face the tribunal of sense experience not individually but only as a corporate body" (op.cit., p. 41). A claim to this effect meets with scepticism, for it seems evident that we do establish or refute all manner of particular statements. This may be to overstate the counter claim, but it must be allowed and indeed is forthrightly maintained on the holistic conception that statements come in what may be termed "grades of theoreticity" (cf. Quine's article of the same name). Those at the lowest grades, in general observation statements, are most susceptible to individual assessment. But even here the lesson for holism is to be drawn in the impact those assessments have upon other statements, and it must be allowed, vice versa. Thus observation statements tend to confirm or disconfirm general theories; they allow or disallow other observation statements. And in the opposite direction, an inclination to favor som
certain overall theory will predispose us to discount observations prima
facie incompatible with it - perhaps by such crude devices as hallucination,
or more subtly in bringing to bear a theory of extenuating circumstances.
At higher levels of theoreticity (e.g. theoretical physics, but also, Quine
would have us realize, matters very much nearer to home) we may entertain
the issue of a particular statement's truth or falsity - but the considerations
which need to be brought to bear concern whole theories and their indirect
support in ranges of individual observation. We appeal to background theories
and their supporting evidence in establishing such statements. If it should
be, as this picture suggests, that we cannot usefully, or even sensibly talk
of a statement being true simplicity, but must construe the notion relative to an
encompassing theory, we seem committed to an account of truth couched in
terms of grounds for assertion, i.e. anti-realism.

Nor ought this feature of Quine's position occasion surprise. On the
conceptual framework which the aim of this thesis it is to explore, anti-
realist semantics has its grounding in an anti-realist conception of truth.
And as I have already sought to render plausible, verificationism constitutes
anti-realist semantics. Quine rejects decisively sentential verificationism,
but the basic insight which animated that program remains valid on his
conception of language and language theory. Quine's approach to meaning
might well be described as the formula "meaning = the method of verification"
- gone holistic. His differences, but much more strikingly his fundamental
sympathy and agreement with the logical positivists' analysis of language and
meaning, are explicit in "Epistemology Naturalized": "the observation sentence
is the cornerstone of semantics. ... It is no shock to the preconceptions of
old Vienna to say that epistemology now becomes semantics. For epistemology

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remains centered as always on evidence, and meaning remains centered as always on verification; and evidence is verification " (p. 89). Quine and the positivists both acknowledge indebtedness to Pierce for the notion that the meaning of a statement consists in the difference its truth would make to possible experience (as Quine paraphrases it, op. cit., p. 78). For Quine the logical positivists went wrong in failing to take this dictum seriously enough, in not giving full scope to the "difference its truth would make". Duhem's holism is an expression of broad differences individual sentences make in the overall scheme, and conversely. Indeterminacy of translation, far from being a sceptical attack on the very notion of meaning, is to be construed as a positive feature of verificationism, setting out the limits and character of meaning. The program for radical translation, beginning with observation sentences and the stimulus meanings they serve to establish, and proceeding by indeterminate projection via analytical hypothesis into the higher reaches of theorecticity which make language a device of seemingly limitless expressive power— all this may be seen as working out by stages and degrees what positivism though could be done once for all, uniformly and determinately over the whole of language. For example, consider the problem I discussed in the final section of Chapter 2 of past tense statements, even restricted to those in the ostensibly simple form of observation reports, as 'he crossed the river'. The difficulty which positivist verificationism faced was to preserve connections of sense between the present tense observational statement e.g. 'he is crossing the river', and the immediate past tense report linked in truth value to it, as well as the distant past tense statement expressed in the same form of words. If all these statements are understood uniformly, then the attempt to account for that understanding in terms of conditions by which
they are established is put in jeopardy. It is only too evident that statements about the remote past are established in very different ways and far more indirectly than their homonymous counterparts dealing with more recent experiences. The positivists attempted a uniform account of sense through weakening the notion of observation by appeal to subjunctives and counterfactuals. Recall that this dilution of what is meant by 'possible' observation threatens to render vacuous the very notion of verification. The situation presents a dilemma, for taking the notion of verification more rigorously threatens impalement on another horn. If the sense of 'Caesar crossed the Rubicon' is really to be given in terms of what would for us who now utter the sentence establish it as true, it seems quite unconnected in sense with such a statement as 'Ryle crossed the Cherwell'. My notion of analogical extensions of language was an attempt to preserve anti-realist semantics from being gored by either horn. On that suggestion, the sense of a statement is given by the conditions which would establish it in its central and most immediate uses. The capacity to recognize these conditions as obtaining in such cases is taken to endow a sentence (form of words) with sense so that we comprehend statements expressed by that sentence and referring to remote situations in which that sort of evidence would no longer play a role in establishing its truth value. Quine's holistic conception of language, with the observation sentence as "cornerstone of semantics" might be construed as a quite different attempt to guide verificationism between unpallatable alternatives. On this view the sense of a given statement is indeed embodied in what would count as evidence for that very statement. But it is no longer required, because realized to be impossible, that any account of such evidence be a fixed listing of relevant observations, or even description
of indefinitely many such. The evidential relation is far too flexible and multifarious to allow always direct grounding in possible observation. Insofar as the subject of discourse is remote from immediate experience, truth determination is mediated by theory.

So it is that the vacuousness of "in principle" but humanly impossible observation is avoided. As to the other difficulty, of how the range of statements expressible in a single form of words may be endowed by a uniform account of meaning, Quine's solution accepts lack of uniformity. But the holism of his account saves lack of uniformity from degenerating into utter disconnection. What threatened verificationism with outright counter example was rendering the sense of "he crossed the river" in one case by reference to observation of the man and his movements relative to the river, and in another case by sifting through documents, assessing them by the canons of historical scholarship. If this is all that can be said by way of accounting meaning in these two cases it seems that the crucial feature of language, its flexibility in organizing diverse elements of experience near and remote has been entirely lost. Quine's formulation of verificationist semantics does not dispute that disparate sorts of experience may be brought to bear in attempts to assess statements whose meaning we feel is in some manner uniform. But his insistence on viewing the as embedded within an interconnected scheme of theory and observation does at least tie them together in a way which ought give at least some satisfaction to the intuition that it is no mere accident that these two statements are homophonously expressed. We may feel that the connection which Quine allows is too weak, doing justice neither to our intentions, nor to the demands in terms of human comprehensibility of a systematically
functioning apparatus of language. The reply must be that the amount of uniformity embodied in Quine's verificationist account is all that the facts dictate. Recall my earlier discussion of planetary theories and in that context of statements of the form 'this goes around that'. If my understanding of the lesson for semantics which Quine would have us draw from holism is correct, then pragmatic considerations of simplicity and the like will incline us to understand such a statement uniformly as regards a planet and the sun, and a dog and that tree. But facts (observations) do not force this on us, and giving due consideration to the whole scheme of language in which these statements are embedded, and so are endowed with meaning, we may find reason not to. At least some of the problems which the positivists took it in their espousal of verificationism as incumbent upon them to solve, are seen on a properly empirical and wide-scope view of language to dissolve.

When ... we take a verification theory of meaning seriously, the indeterminacy would appear to be inescapable. The Vienna Circle espoused a verification theory of meaning but did not take it seriously enough. If we recognize with Peirce that the meaning of a sentence turns purely on what would count as evidence for its truth, and if we recognize with Duhem that theoretical sentences have their evidence not as single sentences but only as larger blocks of theory, then the indeterminacy of translation of theoretical sentences is the natural conclusion. And most sentences, apart from observation sentences, are theoretical. ...Should the unwelcomness of the conclusion persuade us to abandon the verification theory of meaning? Certainly not. The sort of meaning that is basic to translation, and to the learning of one's own language, is necessarily empirical meaning and nothing more. ("Epistemology Naturalized", pp. 80-81)

Let me return for a moment to the theme that this analysis of meaning constitutes anti-realist semantics based upon an anti-realist conception of
truth. As I have argued, the concept of holistic dependence on theory renders truth immanent to human perceptions, and so anti-realist in character. But it is important to appreciate that the consequence of thus eschewing an absolute, transcendent notion of truth (perhaps of the kind it was thought the image of correspondence renders compelling) is not unalloyed coherence. The subjectivism of a coherence conception of truth is a different matter altogether from the anti-realism of conceiving a statement's being true in terms of possible human actions which reveal it to be such. Coherence, with its embarrassment over sorting out fairy tales from history or even setting out grounds for establishing a logic in terms of which the very notion of coherence may be proclaimed, must itself be regarded as incoherent. Holism is not coherence because observation statements provide a fixed, if not absolutely rigid constraint by fact. Appeal to fact at this level is not however, to jump from the fire of coherence into the frying pan of transcendent realism. For observation statements report fact directly accessible to human determination. The appropriateness of bivalence when considering this range of discourse is establishable on a fully anti-realist conception, just as mathematical intuitionism coincides with classical in the domain of e.g. quantifier-free arithmetic.

In the preceding discussion I have spoken of anti-realist semantics as based upon an anti-realist conception of truth. It is indeed a theme pervasive of this dissertation that the importance of truth as a philosophical notion lies in its role as foundation to an account of meaning. But while it is evident that truth and meaning are at once separate concepts and inextricably bound together, an attempt to raise issues of priority, either logical (based on conceptual analysis) or heuristic (in terms of how an understanding of
one may facilitate explanation of the other) or temporal (as perhaps in a
genetic account of how men came to utilize these concepts), any such
tempt seems to face equal plausibility in the opposite order of priority.
The issue of an ordering between the concepts of truth and meaning is
matched in sufficiently many facets to appear awkwardly like "which came
first, the chicken or the egg?" The issue of truth for a given form of
words cannot be raised until it is understood, its meaning grasped. We
wish to render an account of the meaning of a sentence in terms of its truth
conditions, i.e. what it means is what it says or asserts (for declarative
sentences), i.e. what must be the case in order for it to be true. The nature
of meaning then is functionally dependent upon what conception of truth may
prove tenable. And that is an issue between modesty as to human importance
in the world vs. modesty as to human conceptual capacities. But that
question seems to turn on what sort of meaning we may attach to the utterances
of our language. Is our linguistic capacity such that we may understand
sentences which describe situations utterly beyond our ken? If so, then
a transcendent notion of truth is indeed appropriate and unproblematic. If
not, if our understanding of statements consists ultimately in, or at least
derives from some kind of human experience of the situation described,
this constitutes grounds for maintaining truth a notion immanent to human
capacities. I have argued that Quine's conception of meaning constitutes
anti-realist semantics built upon an anti-realist conception of truth; clearly
such a claim is fraught with the seemingly awkward circularities just
adumbrated. But it may be that the Quinean scheme I have attempted to
expound and assess not only exemplifies the anti-realist scheme, but
carries with it as well a resolution of these difficulties. As befits a puzzle
of this character, a way out does not consist e.g. in opting for the egg as against the chicken, but rather in showing us a vantage point from which a question of priorities dissolves, and the mutual interaction, rather than constituting a vicious circle, serves to account for the functioning of the interconnected concepts. The picture to be exploited here is the holistic interaction of language in which, so it seems, truth and meaning share one and the same structure, constitute two ways of looking at the same natural phenomenon. Duhem saw his enterprise as a branch of epistemology, a particular corner of it, in which the issue at hand concerns the structure of theory and observation by which science comes to establish truths. Quine's claim is that this account serves not only science, but the general enterprise of how we come to know anything, to ascertain what is true. Thus widely applicable, this structure constitutes also the device by which language means. Truths cannot be expressed without language endowed by meaning; but the establishing of meaning for us in what we uncomprehendingly observe in an alien community as the interaction of verbal exchange and understanding is the enterprise of constructing their scheme of what is true. The underdetermination of theory by observation is the indeterminacy of translation. It is only after proceeding in this enterprise for quite some way with truth and meaning, the two sides of an unfamiliar coin, that we finally are able to say which is heads, which tails. We reach a point where we may attribute error to the native whose meaning/belief structure we have sought to uncover. And though we can come to do this, and may be increasingly confident in doing so, underdetermination of theory/indeterminacy of translation remains a phenomenon which denies us the comfort of being certain that we are right when we do so. There is no fact of the matter.
While I am somewhat persuaded by the foregoing account, a different one seems to me arguably tenable, and this more consonant with the scheme for interaction between truth, meaning and realism which is sketched and utilized through much of this dissertation. On this picture truth is indeed the substrate of meaning. Quine's program of radical translation is founded upon this very conception; for while the establishment of meaning is the goal of the enterprise, it is accomplished by determining, to the degree theoretical limitations allow, what is true (in that language). Later, and derivatively, we come to establish what is true for the native, i.e. what he believes; and as a late stage in the operation we reach a general account of what statements in that language mean. Here then we seem to have a clear temporal precedence of truth over meaning, and to the extent that Quine's armchair account of radical translation constitutes a general analysis of how meaning inheres in linguistic behavior, conceptual precedence as well. But here it may be protested that chicken and egg style puzzles earlier detailed are not by these considerations settled so much as ignored. How does what has just been said touch the point that issues of truth cannot even be raised except with respect to language endowed with meaning? This point is in its own way unexceptionable, but in that way not necessarily problematic for the preceding account. To raise the issue at all as to an assertive linguistic utterance being true, I need to know that it has meaning, but I do not need to know what meaning it has. Thus the first step in radical translation, perhaps rather slurred over in most accounts, is to establish that the sounds, signs, or whatever (to us unintelligible) which we observe in the social interactions of a given community constitute meaningful discourse between members of that
community. Without identifying something as verbal behavior of course the issue of translation does not arise. Nor is it an entirely trivial matter to do this. While on the whole we expect linguistic expression to proceed by use of vocal chords, what counts as language and what as singing or whistling while they work may require sorting out. Recognizing the assertive tone of voice is not a matter to be taken for granted, and Quine is explicit on difficulties which may attend identifying expression of assent and dissent. Nor can we be sure in advance that it isn't the gestures which convey meaning, and that the use of vocal chords merely provides emphasis and dramatic setting. Hoax remains a more than merely theoretical possibility; witness the state visit to H.M. fleet at Portsmouth by the Emperor of Abyssinia and his court, in the person of youthful members of Bloomsbury, who throughout the episode 'conversed' with each other in a tongue radically alien to the English officers who entertained them. But for all these difficulties, meaningful discourse may, by acute observation and sensible inductive technique, be identified in advance of meanings. And once that is accomplished, the enterprise of establishing meanings proceeds via determination of what, as expressed in the alien tongue, is true and held to be true.

A reply to these arguments for the precedence of truth over meaning in the Quinean scheme for semantics might turn on doubting that radical translation adequately encompasses the conceptual issues attendant on the concept of meaning. It is all very well as regards the problem of interpretation, for alien communities, and even closer to home, when I endeavour to understand my compatriots as we converse in our common native tongue. But what is left as a problem, it may be claimed, is an
accounting for me of how I endow my utterances with meaning, how it is I come to possess facility with the concepts of meaning and truth. Here there can be no issue of translation, of meaningfulness before meaning, of truth before understanding. The point must seem unattractive from the thoroughgoing empiricist vantage point from which Quine surveys the problem of meaning. For to take it that there is some feature of access to meaning particular to me but not applicable to how I interpret the speech of my fellows, this smacks of an appeal to introspection, a mentalism in which meanings are there to be grasped privately by minds. When I would understand what another means by the words he addresses to me, I must avail myself of the apparatus for radical translation. But to separate thus the problem of meaning for me from that of the others is to fail entirely at grasping the implications to be drawn from indeterminacy of translation. If it were the case that for myself I know what I mean because I grasp directly the meaning of sentences I utter (and then perhaps entertain such an issue as whether they are true or not), then short of some solipsistic view of others, I must suppose their speech as likewise backed by their grasp of objectual intended meanings. But then, I must allow that though it may be very difficult (or even in principle impossible) to be certain that I've got it right what they mean, by attaching to their statements when I hear them the same meaning they have in mind on uttering them, there is a fact of the matter whether I do that matching correctly or not. So it is that Quine's identification of the problem of meaning with radical translation does not allow for first person access to meaning to constitute a singular point in the account of meaning. And this despite the obvious point that on the face of it translation is not a
notion which finds application in soliloquy. But there is no mystery here, for learning constitutes a notion which does apply to individual linguistic performance and dispositions to perform. Native language learning is the behaviorally scrutable counterpart to radical translation. For purposes of these theoretical considerations, one account applies for both. The child's learning begins in stimulus meaning; the further devices and complexities of language follow along naturally, but once past the determinateness of stimulus synonymy there is no saying for sure because no saying at all whether the child has come to understand our language 'just as we who teach him do'. Indeterminacy of translation and the vagaries of reference which issue in ontological relativity apply as much to the child learning his first language as to the field linguist unravelling his nth. It is an important lesson, learned I take it from Wittgenstein, that we worry ourselves into a non-question if we should wonder whether someone evidently not color blind, in full command of the grammar and vocabulary of color concepts is nonetheless so constituted that from birth his experience which he describes as seeing green is actually what I experience when I see blue. It is a lesson in the same empiricist spirit, though not exactly the same lesson, to be learned from Quine should we be tempted to wonder whether the child is really referring to whole rabbits in his use of the term after we've taught it to him ostensively; for the appeal we might make to his command of the individuative apparatus of language by which he counts the pet rabbits, discourses on their duration and the like will have been learned by extensions of language use not directly susceptible to observational check.
So it is to be argued that the account of meaning in terms of radical translation is the whole story. Meaning is what may be learned or translated; it has no other existence, it is grasped in no other way. If we are thus licensed to take the account of radical translation as all that can and need be said by way of general analysis of meaning, then the preceding argument may well be accepted as settling the issue of priority between meaning and truth in favor of truth. Davidson's program for semantics may be construed, as I have argued in Chapter 3, at least in part as systematization of Quine's conception of meaning. This point as to priority of truth finds expression in Davidson's casting the problem of meaning for a particular language in the form of constructing a theory of truth for that language. I have attempted to establish in Chapter 3 that the concept of truth which Davidson's account would place at the foundation of semantic theory is an anti-realist one. And this point finds consonance with my claim as to the character of truth within a Duhemian holistic scheme of theory and observation.

§ 5. Quine and logical truth

Quine, like the positivists, while adhering to a form of anti-realist semantics, is imbued with an uncritical faith in what must be taken to constitute a realist conception of truth. And for him, as them, this faith is licensed by Tarski's analysis: "No sentence is true but reality makes it so. The sentence 'Snow is white' is true, as Tarski has taught us, if and only if real snow is really white" (Philosophy of Logic, p. 10). In Chapter 2 I set out grounds for doubting any claims as to the realist character of truth based either on convention T as a condition on any
analysis of truth, or on the actual analyses Tarski formulates in con­
formity with convention T. Carnap, as we saw, took the Tarskian con­
ception of truth as justifying adherence to classical logic, in particular
the principle of excluded middle. There is something of this same move
by Quine when he entertains the view that "what sentences of a language
to count as logically true is determined ... when we have settled two
things about the language: its grammar and its truth predicate." (p. 60)
(In this section references by page number alone are to Philosophy of Logic.)
Settling the grammar comes first, and that is a matter immanent to
each language considered, and indeed even to our particular approach to
that language; within the enterprise of radical translation settling a
grammar belongs to the stage of analytical hypothesizing. Once a work­
ing grammar is at hand, Tarski's techniques are applicable and a truth
predicate for that language is obtained. The principles of logic are thereby
determined, and it is Quine's unswerving view that they will turn out
classical logic, with all its "convenience and beauty" (p. 88). The grounds
Quine takes to support him in this expectation consist, I shall argue, in
a combination of claims as to the radical translation of logical particles, in
particular the sentential connectives, which are unjustified and indeed
wrong, and a dogmatic faith beyond argument.

Ultimately the positivists' attempt to erect the superstructure of
verificationism on a foundation of realist truth and attendant classical
logic could be seen directly to be flawed by incoherence. The account of
statement meaning in terms of what would serve to establish or refute it
runs up against the realist conception of situations described in language,
so or not in virtue of excluded middle, but quite beyond the scope of human
experience. We saw in particular that universal quantification over
moments of time (interpreted realistically) may create such inacces­
sibility. These difficulties do not find direct counterpart in Quine's
verificationist semantics. This point reflects two features of the holism
in Quine's account of statement meaning as grounded in experience.
The first is of course that we cannot in general expect to render a complete
account of how a given particular statement is decided by possible observa­
tional experience. Thus it is at least open on a Quinean formulation of
verificationism to deflect the counter-examples to sentential verifica­
tionism by attributing our inability to specify experiences which would
bear on their truth value to their position as highly theoretical within our
conceptual scheme. It is incumbent upon one who would take this line
to render an account of how such sentences do figure within the total
scheme of our holistic organization of experience. How convincingly
this might be done seems unclear in advance of a detailed attempt. But
there is at least the prima facie possibility of accounting in Quinean terms
for what proved a counter-example to logical positivist verificationism.
Nor is this result surprising in light of a second feature of holism: its
embodiment of an anti-realist conception of truth. The incoherence of
the positivist position was its espousal of anti-realist semantics against
a background realist conception of truth and meaningfulness. Quine's
verificationism is based on an appropriately constructivist notion of vérité.
Given the intimate connection between what are principles of logic and
realist/anti-realist conception of truth and meaning, one would expect
Quine's adherence to classical logic either to render the whole scheme in
some way directly incoherent, or to prove on inspection an excizable
prejudice. The latter seems to be the case.

Quine's view as to the character of logical principles within the holistic interplay of theory and observation has created a certain amount of puzzlement and drawn a good deal of critical fire. On the one hand he wants logic accorded no special status: "no statement is immune to revision. Revision even of the logical law of the excluded middle has been proposed as a means of simplifying quantum mechanics; and what difference is there in principle between such a shift and the shift whereby Kepler superseded Ptolemy, or Einstein Newton, or Darwin Aristotle?" ("Two Dogmas", p. 43). And yet such a claim seems disingenuous or at least puzzling, when set against the remark that "departure from the law of the excluded middle would count as evidence of revised usage of 'or' and 'not' " ("Carnap and Logical Truth", p. 396). Or in Word and Object, p. 59: "When someone espouses a logic whose laws are ostensibly contrary to our own, we are ready to speculate that he is just giving some familiar old vocables ('and', 'or', 'not', 'all', etc.) [Quine is here considering radical 'translation', from English into English] new meanings." And as Quine puts it later in Philosophy of Logic (p. 81): "here, evidently, is the deviant logician's predicament: when he tries to deny the doctrine he only changes the subject." So it seems that, on the other hand, principles of classical logic have a quite special status, namely that if we would deny them we must mean something different from what those who affirm them mean. This seems to contrast with e.g. "There's a porpoise in this pond" which might be denied while meaning by it just what one who maintains it does. Strawson and others interpret these passages as apparent affirmation of the notion of analyticity, which is meant to be eschewed on grounds of
holism. Strawson remarks of Quine's position in reviewing *Philosophy of Logic*: "controversion of standard logic is rejection of certain ideas, not dissent from generally accepted doctrine framed in terms of those ideas. No advocate of analyticity could ask, on this point, for more" (Journal of Philosophy, 68 (1971), p. 176). Quine is aware how close he skates to apparent concession; but it is done with an eye to strengthening his claim by the declaration that even this does not constitute analyticity. Thus he remarks on the passage cited above from "Carnap and Logical Truth": "This judgement was upheld in § III, though disqualified as evidence for the linguistic doctrine of logical truth." The disqualification is by appeal to a notion of 'obviousness', and the condition that what is taken as obvious in a given alien speech community ought to be given a translation so regarded in ours. Quine claims that logical principles possess obviousness, and that this point, rather than any linguistic doctrine of logical truth, is to account for the closely allied doctrines that "alternative logics are inseparable practically from mere change in usage of logical words" and "illogical cultures are indistinguishable from ill-translated ones" ("Carnap and Logical Truth", p. 389). Quine expresses the matter in the course of his *Word and Object* discussion of "Translating Logical Connectives" obversely to his principle of obviousness: "the maxim of translation underlying all this is that assertions startlingly false on the face of them are likely to turn on hidden differences of language" (p. 59). And in *Philosophy of Logic* p. 33 Quine declares "the canon 'save the obvious' bans any manual of translation that would represent the foreigners as contradicting our logic." What is obvious is all too obviously a matter of taste and
background and what is familiar. As Quine remarks in his discussion of "Deviant Logics" (p. 86): "and there is a loss, still more serious, on the score of familiarity." One begins to suspect that it is somewhere in this line of consideration that classical logic is being smuggled into the Quinean scheme of things. I am not inclined to take entirely at face value Quine's disclaimer "I have been using the vaguely psychological word 'obvious' non-technically, assigning it no explanatory value" ("Carnap and Logical Truth", p. 390). Strawson expresses qualms (in the penultimate paragraph of his review) at Quine's play on 'obvious'.

But there is more to Quine's account of radical translation as applied to sentential connectives than mere appeal to obviousness, or at least there is some structured argument in the course of that appeal. The first step in radical translation is to identify in the native population behavioral signs signifying sentential assent and dissent. What we settle on in this regard is already to some degree underdetermined, a matter of analytical hypothesizing, though Quine is confident a workable hypothesis will present itself readily enough. Somewhere further along in the project will come analytical hypotheses equating native morphemes to e.g. our English 'not', 'and', 'or' (it is usually just 'etc.' in Quine's account; I should have thought 'if ... then ....' presents particular difficulties, and while Quine notes the special problem of 'but', pointing to a problem does not solve it). That the translation into these English words belongs to analytical hypothesizing is integral to Quine's notion that grammar of a particular language is a notion relative to our analysis of it; as he remarks specifically (p. 87), "negation and alternation are immanent rather than transcendent." But once thought of, it is a simple matter
to check whether they constitute workable hypotheses:

By reference to assent and dissent we can state semantic criteria for truth functions; i.e. criteria for determining whether a given native idiom is to be construed as expressing the truth function in question. The semantic criterion of negation is that it turns any ... sentence to which one will assent into a sentence from which one will dissent, and vice versa. That of conjunction is that it produces compounds to which one is prepared to assent always and only when one is prepared to assent to each component. That of alternation is similar with assent charged twice to dissent. ... When we find that a native construction fulfills one or another of these three semantic criteria, we can ask no more toward an understanding of it.

(Word and Object, pp. 57-58)

What Quine supposes of course is that by recourse to 'behavioral criteria' we are able to 'pin down' certain native locutions as governed by the classical truth tables.

Before assessing how successful such an enterprise might be, let me insert two comments, the first on the character of sentential logic on this account, the second on what happens, in Quine’s view, in the further reaches of logic, with its quantifiers, predicates and variables. According to Quine the doctrine of analyticity in this context is the view that establishing certain native utterances as the translates of our logic principles is accomplished by discovering the semantic rules which govern their language, and in virtue of which then those utterances are true. Quine’s account of the status of logical truths in translation avoids attributing to them analyticity because the principles of translation have merely the character of empirical hypotheses. Still, Quine feels compelled to render some account as to why intuitively there seems to be something 'special' about logical truths. It is here he invokes the notion
of obviousness. This account ostensibly disallows claiming logical truths analytic by declaring analytic anything obvious, since obviousness comes in degrees. And as already noted, the condition of matching the obvious serves Quine as the basis for demanding that any putative logical particles of the native language go over into our truth functional ones. He thinks we can tell whether our translation does this or not, and such are his tastes in the obvious that he is confident that if it does not we will find the natives failing to assent to something we, on our translation of what they are saying, do take as obvious. And that, on Quine's general principles for translation, is prima facie grounds for bad translation. But of course it is not definitive of bad translation, and cannot be; that is indeterminacy. In the case of translating the apparatus of quantified logic the effect of indeterminacy is considerably more drastic. Not only is it a matter of indeterminate hypotheses what to equate to our quantified logic devices. There is no behavioral criterion for whether what we pick out as a likely candidate functions as quantification over individuals or rather something like our device for expressing "is a part of". The argument is from inscrutability of reference, combined with Quine's doctrine that the ontology of a language is fixed by its quantifications: to be is to be the value of a variable.

Returning to consider sentential connectives, we have Quine's claim that a semantics of assent and dissent fixes their sense, and identically to that embodied in the tables of classical truth functions. On the face of it this is surprising, since the classical notion of truth and falsity is not captured by assent and dissent. The former is dichotomous, the latter evidently not. Indeed, one way of describing the difference between classical and intuitionist conceptions of logic is that the former would fix the sense
of connectives in terms of truth and falsity, the latter in terms of assent
and dissent. On the other hand, with so much of Quine's analysis fitting
an anti-realist scheme of meaning and truth, a conception of logic integral
to it ought to be expected to share the characteristics of anti-realism, to
be close to intuitionist logical principles. I claim that this situation is
realized. And as I shall argue, were Quine to accept the point, his account
of logic would be rendered more convincing in the following ways: the
appeal to an unarticulated notion of obviousness could be eliminated in favor
of an articulated conception of the consequences of a thoroughly empiricist
viewpoint. And given the construction of intuitionist logic, it would occupy
the role within the total conceptual scheme which Quine has sought to establish for
logical truth. Quine's arguments have on this score proved tortuous and
often unconvincing (in at least the sense that many readers were unconvinced)
as he sought to dethrone logical truth from its honorific status as analytic
in virtue of rules of language, while at the same time preserving it as central
and pervasive in our conceptual scheme. This picture could, I shall argue,
be made out much more convincingly if it were intuitionist logic Quine were
championing.

We have already seen in a passage quoted from Word and Object how assent
and dissent conditions could, Quine supposes, characterize the (three main)
classical truth functions. Curiously, that formulation leaves quite out of
account the non-dichotomous situation in which the native neither assents to
nor dissents from the complex expression, or one or several of its sen-
tential components. Later Quine came to realize the omission: "These
conditions remain indeed less than definitive on one point: on the question of
a native's assenting to the compound but to neither component. ... In
Word and Object, p. 58, I gave only the condition on dissent and so over­
looked this limitation on the assent side. Conjunction suffered in equal
and opposite fashion "("Existence and Quantification", p. 104). Nonetheless,
he seems yet inclined to advance his previous claims for classical truth
functions, even if on amended and weakened grounds: "Still, the two
conditions do much toward identifying alternation" (loc. cit.) But the dif­
ficulties here are insufficiently appreciated, the lesson from them not drawn.
What is lacking in the professed accounts of sentential connectives is an
extremal clause telling us that the lines of the truth table are all the possibili­
ties that there are. But this is not the case with assent and dissent, rather
than classical truth and falsity as the elements of the table. And in the
absence of an extremal clause, the sense of the connectives is not fully
specified in Quine's attempt. Line by line the intuitionist is in full agreement
with the classical logician; any particular application of the classical tables
to generate assent or dissent on a complex statement from that of its sen­
tential components is intuitionistically valid. What the intuitionist is not
prepared to countenance is an argument based on surveying an entire table
and drawing conclusions on the supposition that the table lists all possible
cases. Quine's behaviorist account of construing logic can provide no
justification for doing so. But so far as the account is justified, it is
indistinguishable from the claims of the intuitionist logician. This argument
shows that the "deviant logician's predicament" (p. 81) is not quite as
Quine has made it out. The subject is sentential logic on Quine's behaviorist
construal of it; the doctrine successfully denied is Quine's claim that this
is the classical logic of truth functions.
The argument developed thus far may fairly be deemed inconclusive because I have in effect considered each sentential connective in isolation. The differences between classical and intuitionist logic emerge in terms of principles adhered to or denied only when connectives interact, e.g. disagreement over $A \lor \neg A$ and $\neg \neg A \rightarrow A$. But what I wish to argue is that the only behaviorally scrutable putative evidence which could bear on the issue of which, will either leave the matter indecisive (from an external viewpoint in which both classical and intuitionist allegiance are thought possible) or settle it in favor of intuitionism. In other words, the kind of evidence Quine would bring to bear on settling the translation of an alien's logic, will if successful, declare him anti-realist in logical allegiance.

The two principles, excluded middle and double negation, instanced above are in fact fully characteristic of the difference between classical and intuitionist outlook at the level of sentential logic. Neither is generally valid intuitionistically; either, when added as an axiom schema to intuitionistic sentential logic, yields the classical system. (They are not, however, intuitionistically equivalent instance by instance; thus a particular statement of the form $\neg \neg A \rightarrow A$ may be assertable while $A \lor \neg A$ fail to be. The converse implication is intuitionistically valid.) Now suppose radical translation to have proceeded to a point where native locutions have been hypothetically equated to 'or' and 'not' and 'if' ... then ....' and the correlation established in terms of assent and dissent conditions. Can we now work with assent and dissent to instances of $A \lor \neg A$ as a means of settling whether it is classical or intuitionistic logic which governs? I think not. Two tests suggest themselves. In one case we find a situation in which an instance $A \lor \neg A$ is assented to, but no assent (or dissent either,
of course) is forthcoming for either of \( A \) or \( \sim A \). We should be tempted to conclude that the alien community espouses classical logic. This would be unjustified. The intuitionist will affirm \( 10^{10} + 1 \) is prime \( \lor \sim (10^{10} + 1 \) is prime), because being prime is in principle decidable (indeed primitive recursive), but nonetheless our native has not carried out the deciding calculation as yet, and is not going to while we wait for his answer. Well, it may be rejoined, at least the situation is just as indecisive in the opposite direction. For suppose we find a situation in which assent is withheld from an instance of \( Av \sim A \). That will not show the native an intuitionist, for he might be a classical minded logician who believes that reference failure creates truth value gaps. But the situation is rather more what I have claimed when we consider tests by assent and dissent conducted with respect to the principle of double negation. Finding a statement \( A \) such that the native assents to \( \sim \sim A \) while not assenting to (though of course not dissenting from) \( A \) seems plausibly to mark the native as some manner of intuitionist. It is difficult to argue conclusively against the very possibility of a countervening explanation of this state of assent and non-assent, as in appeal to the truth value gap theorist to account for failure of excluded middle. The following argument may go some way in this direction: what could intervene between the perceived assertibility of \( \sim \sim A \) and the professed inability to assert \( A \)? Both statements are about precisely the same subject matter; the only difference must reside in the interpretation of iterated negation. And if it makes this difference, then ipso facto negation is being interpreted intuitionistically. Can there by an argument to opposite effect, upholding
a classical over intuitionist interpretation based on assent and dissent with respect to the principle of double negation. I claim not. For we should have to find an instance in which the native affirms \( \neg \neg A \to A \), but for which were he an intuitionist he would not, i.e. he would affirm \( \neg A \) but not \( A \). But it is not just that it might prove difficult to determine behaviorally whether such a situation obtained; it is sheer nonsense to suppose there could be such a situation. For suppose we knew that an intuitionist cannot affirm \( A \). By the intuitionist interpretation of negation it means he can affirm \( \neg A \). But this contradicts the supposition that as an intuitionist he would affirm \( \neg A \). The point here is that an intuitionist failure to affirm \( \neg A \to A \) is not due to its being false, but because he has not yet seen how to convert the construction which establishes \( \neg A \) into one which establishes \( A \). The native who affirms \( \neg A \to A \) cannot thereby be characterized as an adherent of classical logic, since he is behaviorally (in terms of affirmations) indistinguishable from the intuitionist who has been brought by lesson or his own ingenuity to affirm that very instance. And it is forever open to each intuitionist to reach this affirmational state; the intuitionist validity of \( \neg (\neg \neg A \to A) \) is another expression of this same point.

These arguments are decisive against Quine's claim to have found within his scheme a secure niche for classical, truth-functional logic. What is affirmed, and on the basis of Quine's own account of logic under radical translation, are intuitionist principles of logical truth. We had earlier adduced general grounds to expect anti-realist logic as that integral to Quine's overall conception of truth and meaning, and we may construe these last arguments as at least in part realizing that expectation. Nothing
in these arguments counts against Quine's construction of logic. The point comes to accusing Quine of not following out the consequence of his own insights. On occasion, he seems to express and defend on pragmatic grounds forthright prejudice for classical logic, which might account for the lack of consonance here belabored. Classical logic ought be adhered to because there is such a lot of indeterminacy around that we need to make some arbitrary decisions to get on with translation and interpretation. In *Words and Objections* (p. 318) Quine remarks "'Save logical truth' is conventional in character because of the indeterminacy of translation. It is a rule which, compatibly with all stimulus meanings and other verbal dispositions, could be obeyed or flouted. But it is not capricious. The very want of determinacy puts a premium on adhering to this strong and simple rule as a partial determinant." This text occurs in the course of a passage in which Quine sets himself to ponder "the connection between this rigidity of [sentential] logic in translation and the question of the immunity of logic to revision." The situation is that "a phoneme sequence which is a logically true English sentence today could sometime cease to be logically true. We would call this change a change not in logic but in English."

Quine's explanation for this situation is the convention 'save logical truth'. But what he is at pains to distinguish is that this is not a convention inherent in language, but a principle we choose to adhere to. We do so to cut down on indeterminacy of translation, itself indicative of the fact that language structure is not an abstract operative in virtue of its discoverable conventions. The impact of my preceding arguments is that if by 'save logical truth' Quine intends classical sentential logic, this is not a principle we have the option to choose. Or rather, choosing it would be perhaps on a par with 'save
truths of quantified logic'. We might *suppose* that is what we were doing, but by no behaviorally scrutable tests could we establish this. All determinations to this effect will make essential use of analytical hypotheses where the degree of indeterminacy is high (cf. *Words and Objections*, p. 312, for some discussion of the notion that "indeterminacy of translation comes in degrees"). On the other hand if the maxim be 'save intuitionist sentential logic', then we do have a convention to which we determinately can adhere, even if Quine is right that indeterminacy of translation leaves us (empirically) free not to.

The advocacy of mathematical intuitionistism, or even dispassionate consideration of it seems not to be an undertaking Quine finds congenial. From his vantage point, after years of work within classical systems and not a few textbooks out on the market inculcating that viewpoint in the theory, such advocacy would violate his 'maxim of minimum mutilation' (p. 85), or as I would be inclined to put it, 'hang on to what you've got'. Quine waxes poetic on the attractions of classical logic, and paints intuitionism in most unbecoming colors: "The classical logic of truth functions and quantifiers is free of paradox, and incidentally is a paragon of clarity, elegance, and efficiency." (p. 55) Two pages later he rhapsodizes on "the familiarity, the convenience, the simplicity, and the beauty of our logic," and he excoriates intuitionism for lacking these qualities. Familiarity is an accident of experience, convenience depends on what the project is at hand, simplicity hardly seems to favor one as over the other of these logical conceptions, and beauty is in the eye of the beholder. The only point with *any* content which Quine raises against intuitionist logic is that it "lacks the transparency even of a many-valued truth-functional
logic" (p. 57). The standard of transparency here invoked is the purely formal truth table. But for such objects, to fix the sense of the connective which labels the last column requires some account of what those T's and F's stand for. Such an explanation is beyond anything explicit or even determinately implicit in the formal array itself. Quine's overlooking this point constitutes, as we have seen, a serious lacuna in his discussion of radical translation of the logical connectives (one in fact which cannot be filled in to give his intended conclusion). Quine's objection to the non-truth table explanations of sense for the intuitionist connectives (as he would have it, "explained with the help of words and phrases like 'refute' and 'follow from' ") is that they "go dim when one tries to respect the distinction between saying a sentence and talking about it " (loc. cit.). That is a distinction which can, as noted in my discussion of Ramsey on truth in Chapter 2, cause difficulties in ostensibly classical contexts. But Quine is only too enthusiastic to allow that Tarski successfully negotiates the treacherous rapids of use/invention. ("The truth predicate serves the crucial purpose ... of disquotation" (p. 97), and Tarski's account, as we have noted, can be understood intuitionistically. Besides lack of 'transparency' on this account, for Quine "there is a serious loss of simplicity, especially when the new logic is not even a many-valued truth-functional logic," but he tops that with the complaint "and there is a loss, still more serious, on the score of familiarity" (p. 86). Lack of familiarity does not in this context seem something to thus admit so lightly, indeed boast of and ostensibly make capital on. Some familiarity might have boosted the credibility of his assessment in these matters. A little more even, and he might not have come to this assessment. There is a certain
plaintiveness in his remark "this only begins to illustrate the handicap of having to think within a deviant logic ;" but one shudders to envisage the impact on the advance of science of such an attitude widespread. Still, there is a grudgingly open-minded remark in Quine's discussion, one indeed potentially concessive: "The price is perhaps not quite prohibitive, but the returns had better be good." What I am suggesting is that from Quine's point of view the return is indeed excellent, namely a logic consistent with his thorough-going empiricism, and given the other components of his conception of language, the price is a bargain.

Now it is sometimes urged that Quine ought to welcome intuitionism, quantum logic and any other alternative logic as instancing his holistic thesis that logical truths are of no fundamentally unique epistemic character, and all statements including them are, in light of various considerations, revisable (subject to degrees of how much else need be revised in the process). On this view, facing a choice between competing logics is thought perhaps to constitute a realization of the indeterminacy of translation. In fact however, such suggestions stem from a failure to appreciate fully Quine's claim that logical truths are not true in some way sharply different from non-logical truths. The point is not that logic is up for grabs, but that there is no radically separable domain to be construed one way or the other, with the vagaries of indeterminacy demanding we make this choice. That conception would constitute relativized analyticity as discussed in §3.

Quine is correct in his claim that we can decide, in setting out on the project of radical translation, not to rest content with our choice of analytical
hypotheses until we have isolated devices of the native language which share the behavioral characteristics of our sentential connectives, notably 'not', 'or', 'and', 'if ... then .....' (It might be quite complicated; perhaps they use the Sheffer stroke.) Should we manage the identification we will generate whole ranges of statements sharing the sought after property that our assessment as to the truth of a putative translate matches the native's assessment of his utterance; and these will for good measure have the further happy feature of being alike for them and us obviousness (commanding ready and widespread assent). But what I claim is that to the extent we can be confident on the basis of behavioral observation that we have properly identified some of their locutions with our logical particles, we must suppose them and ourselves adherents to intuitionist logic if the translations involving those identifications are to guarantee matching ready assent with ready assent. Nor ought this to surprise us. Adherence to classical logic is founded on a realist, transcendent conception of truth. In its very nature such conception exceeds the bounds of any characterization in terms of human capacities for determining it to obtain or not. But the devices by which Quine would pin down sentential logic are entirely in terms of what can be established as true. Any set of statements of general form which can be settled at this stage by these devices as 'obviously' true will constitute anti-realistically valid principles of logic.

There is a further feature of Brouwer's conception of logic congenial to Quine's holistic scheme and the place of logic in it. The point is of course an analogical one, as Brouwer's concern was exclusively with mathematics. But in that context he wished to de-throne logic from the
position it held in classical thinking as prior to mathematical investigation. It is the device by which we discover (immutable) mathematical truths. On Brouwer's picture all mathematical truths are created by constructive acts of intuition. Logic consists then in "reflection on general principles about constructions and constructive proofs" (Troelstra, Principles of Intuitionism, p. 5). Thus logic arises out of mathematical activity rather than preceding it. As Heyting puts it (Intuitionism: an Introduction, p. 6), every logical theorem "is but a mathematical theorem of extreme generality; that is to say, logic is a part of mathematics, and can by no means serve as a foundation for it." So in its very conception intuitionistic logic is by analogy characterized with a property central to Quine's doctrine of logical truth. Quine declares in "Two Dogmas" (p. 42) that logical laws have no peculiar status within the "man-made fabric" of "total science", "being in turn simply certain further elements of the field." In particular, logical truth is not marked out from other truth by possessing any "distinctive epistemological basis" (Words and Objections, p. 318). This conception is characteristic of and integral to his views on analyticity and the nature of language. Brouwer's construction of mathematical intuitionism appears to exemplify the Quinean doctrine of logical truth. In light of previous considerations, this situation is not surprising.

It seems to me that Quine, by deep insights which extend the confused empiricism of the logical positivists, formulates a truly empiricist conception of language. What I am claiming, in opposition to Quine, is that the logic of thorough-going empiricism is anti-realist, the model for that being intuitionism. Now it may be, as Quine would
have it, that we have no choice but be empiricists. There are ways this observation may be construed as to command wide assent on the part of philosophers, scientists and men on the street in the grip of what are in fact widely differing persuasions. But I am inclined to think the claim tenable in respect of the full scheme of Quine's conception (excepting of course his aberrant advocacy of classical logic). Given that scheme the most general truths which it seems incumbent upon me to affirm are those of intuitionist logic. But I might never (indeed earlier did not, and may well still not) have grasped Quine's conception; I might not have realized (as he seems not to) its affinity to intuitionist principles. None of this would count against my claim to membership in the community of English language users. There is then this evident sense in which logic is not dictated or validated by any set of principles or rules constitutive of a language.

For Quine, "a man's theory on a given subject may be conceived, nearly enough, as the class of all those sentences, within some limited vocabulary appropriate to the desired subject matter, that he believes to be true" (Words and Objections, "Reply to Chomsky", p. 309). Logical theory concerns indeed a limited vocabulary, but a vocabulary which itself turns up in discourse on almost any subject matter. Principles of logic are for me sentences of my overall theory, i.e. which I take to be true, distinguished (to echo Heyting) merely by their 'extreme generality.' The generality is manifest in the point that, however limited a subject matter I choose to consider, instances of my logical theory turn up amidst my theory of that subject. How do I establish as true the statements of (my) logical theory? It is, as befits the character of that subject matter,
by very general considerations - echoing the remark quoted from
Troelstra, by reflection on general principles about theory and observa-
tion, how statements come to be established. My enterprise is not
peculiarly linguistic, a search (perhaps even by introspection) after the
rules which govern my capacity for speech (the linguistic doctrine of
logical truth and analyticity). Rather these reflections belong to what
might be called speculative metaphysics, in a sense where that means
nothing more than very general considerations about the world and human
perceptions of it. It is with this sense, I take it, that Quine speaks of
"a blurring of the supposed boundary between speculative metaphysics and
natural science" ("Two Dogmas", p. 20).

What now of a dispute over which principles of logic are to be accepted?
First off, I claim that there are such disputes, and would instance that
between Quine and intuitionists. A purely linguistic doctrine of logical
truth would have it that at least one of the disputants is confused or deficient
in the use of language, not a plausible (or even intelligible) claim to
advance about anyone. On the picture which I am here urging, following
Quine and Brouwer, this disagreement is not essentially different in
character from any dispute e.g. in physics or gardening over what is true,
saving only the degree of generality in what is claimed, and attendantly
in the arguments. As in any such difference of opinion, it is open to each
party to maintain the rightness of his position, the wrongness of the con-
trasting one, each to suppose that the opponent would be brought to accept
his position if the opponent would only look properly at the evidence and
arguments; each may have an account of how the opponent has managed to
mislead himself. Let me add here to my previous discussion of some of these
features of this particular dispute about logic.

It seems to me Quine has supposed his argument rendered convincing, even compelling with appeal to an extraordinarily exiguous basis: "every logical truth [and he here of course means classical logic] is obvious, actually or potentially. Each, that is to say, is either obvious as it stands, or can be reached from obvious truths by a sequence of individually obvious steps" (pp. 52-83). But just how obvious is it that the truths of classical logic are obvious, or even potentially so? In this passage Quine goes on to support his claim by appeal to the point that "logic of quantification and identity admits of complete proof procedures, and some of these are procedures that generate sentences purely from visibly true sentences by steps that visibly preserve truth." This is at least in part another instance of the viewpoint which I faulted Tarski on in my Chapter 2 discussion of the philosophical neutrality of convention T and formal systems constructed in conformity with it. Philosophical insight, still less normative positions, cannot be extracted from purely formal results without the input of a philosophical analysis. At least we must establish the non-formal problem to be solved. You cannot get something for nothing, even in philosophy. The result by which Quine would justify his appeal to a 'neutral' sense of obvious, namely the 'completeness' of proof procedures for classical predicate logic, provides striking illustration of this limitation. The result is due to Gödel (1930); Herbrand independently and Skolem already in 1922 had the procedures, but nonetheless 'failed' to achieve the completeness. What Herbrand and Skolem 'lacked' was an analysis of the formal philosophical notion of logical validity in terms of non-finitist notions, particularly 'truth'. Gödel, commenting particularly with regard to Skolem 1922, finds the situation
"surprising", but thinks "the explanation is not hard to find. It lies in a widespread lack, at that time, of the required epistemological attitude toward metamathematics and toward non-finitary reasoning" (letter to Hao Wang, 7 December 1967, quoted in Wang, From Mathematics to Philosophy, p. 8). Quine is cognizant of this point, as indicated in his quite fair and accurate statement of it:

Any such proposal, to define validity or logical truth in terms of a proof procedure, tends to call forth a clamor of protest. It is protested that the property of being provable by the chosen proof procedure is intrinsically uninteresting; it derives its interest solely from the completeness theorem, which equates it to logical truth in a prior and intrinsically interesting sense. It is protested also that in so defining logical truth we would pull the rug from under the important completeness theorem itself, depriving it of content. (p. 57)

And from a certain point of view he has an answer which deals with these protests. He appeals to the fact, alike contended by the protestor, that the completeness theorem, e.g. in the formulation "If a schema is satisfied by every model it can be proved", "is independent of how we define logical truth, since it does not mention logical truth by name" (loc.cit.). He then claims, presumably on the basis of earlier arguments (pp. 53-55), that "[completeness] shows that we can define logical truth by mere description of a proof procedure, without loss of any of the traits that may have made logical truth interesting to us in the first place" (loc.cit.). Now there is a sense in which this claim is correct: to the extent that a given proof procedure can be seen to render an account of logical truth at all, in the sense that the sentences generated by it are logically true, then the completeness theorem can be invoked to establish that provability is the whole story. What is first required is to know that sentences formally derivable by the given proof procedure possess "the traits
that may have made logical truth interesting to us in the first place." This is the property of formal derivability generally referred to as **soundness**. Completeness and soundness are converse properties of a system of formal deduction. They are the two directions of a biconditional, which establish the equivalence of formal derivability to some other notion (assuming completeness and soundness both formulated with respect to the same notion, e.g. 'satisfied by every model'). Completeness is a theorem of significant mathematical content; by contrast, its converse, soundness, is taken as immediate, obvious, even trivial. From a mathematical point of view this assessment is appropriate. But from the vantage point of certain philosophical issues the relative balance of significance and triviality is exactly reversed. It seems to me that Quine is operating with the mathematical perspective in his application of these results and arguments to the **essentially** philosophical issue: what is logical truth? What for Quine gives logical truth its particular character among the totality of truths in our web of belief is obviousness. And what guarantees invariance of logic in translation is the maxim 'preserve the obvious.' And what guarantees the logic so preserved in classical sentential logic is that it in particular, as over other conceptions is possessed of a maximal degree of obviousness. Now Quine has an argument of some complexity to establish the obviousness which will render classical logic unique and favored - the favored choice in translation. I take it to have (roughly) the following structure (not always explicit):

We take it that a statement S is logically true just in case it can be established by the most general considerations possible (a conception I take to be shared by Quine and Brouwer). It is then argued that such considerations must, if they are to be maximally general, abstract from the particular
subject matter of $S$; this condition seems adequately captured by the requirement that such considerations be adequate to establish the truth of all statements of the same form as $S$. The notion of form is intended to abstract from content, subject matter, but there need be no mystery in that. Form of $S$ is a syntactic, grammatical structure which it exhibits, or at any rate into which it may be analyzed. Recall that, following Quine, the grammar of a language is immanent to it, constituting part of what is to be fixed by analytical hypotheses. Consequently, "our proposed abstract notion of logical truth depends not only on the language but on how we grammaticize it" (p. 59). The next step is to render the notion of logical truth in more mathematically tractable terms. This is accomplished by appeal to the notion of arbitrary set theoretic interpretation of $S$; in place of all statements of the same form as $S$, we take the elements (lexicon) of $S$ not constitutive of the form with respect to which it is logically true and consider varying their extension over arbitrary domains of individuals. Such a structure (interpretation) constitutes a model of $S$. The notion 'S is true in a given model' is as analyzed by Tarski. 'S is true in all models' is then taken to capture the previous analysis 'all statements of the same form as $S$ are true'. (I ignore here Quine's scruples over whether there is enough match between possible interpretations of $S$ and statements formally congruent to $S$ to merit this step; he overcomes them.) The next stage is reached via appeal to the Gödel completeness theorem: "If $S$ is true in all interpretations, then it can be formally derived by some given proof procedure." We might wonder at the legitimacy of appealing to a mathematical result itself making essential use of full classical logic in an argument aiming to settle the question 'what is logic?'; conceivably it might be
adequate for his purposes were Quine to utilize here the Gentzen Haupstatz in a form applicable to either classical or intuitionist systems. But granted all this, I wish to focus attention on the final step. Having ostensibly shown that all logical truths are demonstrable by various complete proof procedures, Quine takes it that "some of these are procedures that generate sentences purely from visibly true sentences by steps that visibly preserve truth" (p. 83). This is meant to justify the claim that logic is different from all other domains of knowledge (theories) insofar as "every logical truth is obvious, actually or potentially" and thereby "logical truth is guaranteed under translation. The canon 'save the obvious' bans any manual of translation that would represent the foreigners as contradicting our logic," with the grudging exception: "apart perhaps from corrigible confusions in complex sentences." One may be inclined to urge against Quine a complaint voiced by Wittgenstein in the Tractatus (6.1271) against Frege: "It is remarkable that a thinker as rigorous as Frege appealed to the degree of self-evidence as the criterion of a logical proposition". Is Quine doing anything so very different from what Frege is here accused of when he would have us believe that some proof procedures are endowed with "visibly true" axioms and visibly truth preserving rules of inference? And how is this any advance on the original unvarnished notion of obviousness? I am disinclined to accept that statements of the form \((\neg p \rightarrow \neg q) \rightarrow ((\neg p \rightarrow q) \rightarrow p)\) are visibly, evidently, obviously true: this on two grounds, one 'deep', the other verging on trivial, but both counting significantly against Quine's conception of logic. One of these is the difficulty one encounters, as indeed Quine must have in teaching his course Phil. 140, of inculcating a facility for recognizing classical tautologies even with
reasonably bright and moderately motivated students. And even if the
difficulties on this score prove minimal, the very fact that there is call
for such courses counts rather decisively against Quine's claim that
"we learn logic in learning language" (p. 100). That is, unless that claim
is to be construed in terms of idealized potentialities ('could be brought
to see ....'?), which would so weaken the claim as to render it quite
irrelevant to any general account of logic in radical translation. What
this suggests is taking more seriously than Quine seems to the similarities
of logic to other theories, e.g. physics, geology, gardening. They are
all, logic included, areas of specialized study; gaining expertise in each
requires effort, and each is subject to debate and disagreement as to what
ture expertise consists in. In a quite ordinary sense, physics is a more
specialized study than gardening, even though what it talks about has much
 wider application. By the same token, I suspect that logic is a subject of
study more specialized than geology, less specialized than physics. These
considerations are , as I announced them, nearly trivial, but they bear on
Quine's claims about the character of logic in terms of obviousness and
attendant invariance under radical translation. For Quine's claims to
obviousness require that logic actually be a maximally unspecialized subject.
If it is not, if settling what to count as logically true is a matter of study and
reflection (the assessment of arguments and counter-arguments), logic
can hardly be accorded the behaviorally central and pervasive role given
it in Quine's account of radical translation. I think it is a fallacy to suppose,
as Quine does, that "logical truths, being tied to the grammar and not to the
lexicon, will be among the truths on which all speakers are like best to
agree" (pp. 101-102). Not a bit of it - they are far more likely to agree
that e.g., 'Many human bodies other than mine have before now lived on the earth.' Still, there is no call to allow this particular statement a principle of logic; it is far too specialized, being specifically about human bodies and the earth. The confusion comes in supposing that maximal generality of subject matter and concomitant generality of considerations to establish it will lead to widespread agreement, if not spontaneously, then by minimal prompting. Indeed, the very generality will militate against widespread consideration or even capacity for consideration of the matter; such is my trivial point.

My less trivial one aims to show that there is no reason to suppose maximally general considerations endow their conclusions with a transparency rendering them beyond dispute. The dispute will simply be on very general, fundamental grounds, and the remoteness and abstractness occasioned by extreme generality will if anything militate against obviousness (in Quine's simple behavioral sense of commanding ready assent), though not necessarily against some reasonable, or even very high degree of certainty.

One sense in which it is sometimes maintained that logically true statements are obvious, visibly true, or self-evident is that all it requires to establish them is reflection on what they mean; once understood, they must be seen as true. This formulation might be taken as characteristic of the linguistic doctrine of logical truth; to confuse matters, Quine seems often to trade on this explanation of obviousness while maintaining it cannot give comfort to those who see logical truth as that justified by rules of language. The difference here may be a dispute not over the formula 'true in virtue of language' but over what constitutes language. I wish to dispute this doctrine.
on either understanding. Of the three notions mentioned in the opening sentence of this paragraph it seems to me that only the third, 'self-evident', has any chance of being accounted applicable to logical truths by analysis in terms of understanding, or reflection on meaning. Previous considerations for my first point militate against so accounting logic as actually (in terms of how people respond to it) 'obvious' or 'visibly true'. Thomas Jefferson held 'All men are created equal' to be a self-evident truth. Its lack of obviousness (as commanding ready assent) was evident in the need to declare it in that tendentious context, and in the opposition its declaration engendered. Nonetheless, it was deemed self-evident for containing in its very statement access to the evidence which would establish it, namely reflection on the nature of men and the notion of equality. In like manner it may be deemed that any statement of the form \((\neg p \rightarrow \neg q) \rightarrow ((\neg p \rightarrow q) \rightarrow p)\) is self-evidently true. What are the reflections, on the basis of which all such statements may be seen as true, which the very statements render accessible? Evidently, the only candidates are the concept of truth itself, and the particular syntactic form at hand. How might these elements be combined together to render these statements evidently true? One approach might be in terms of truth tables and the notion of tautology. The way these two elements, form and truth, interact is embodied in the truth functions. Calculating with them, we see that however the \(p\)'s and \(q\)'s are assigned \(T\)'s and \(F\)'s, the function set out in the form \((\neg p \rightarrow \neg q) \rightarrow ((\neg p \rightarrow q) \rightarrow p)\) takes the value \(T\). Does this tell us that statements of this form are logically true? It could of course by fiat, but we are in this discussion starting from a conception of logical truth which does not allow this (uninteresting) possibility. To suppose that such consideration might
settle the philosophical problem of logical truth is to take the formalist conception of language congenial to Carnap and vigorously eschewed by Quine. Still, Quine would count them as relevant, and rightly so, with the need then to establish an appropriate connection between the formal calculus of truth functions and our use of language to say things which are true. He takes it that his behavioral account of sentential connectives in terms of assent and dissent in the context of radical translation effects such a connection. That in this way certain general patterns are established of holding true (assenting to) complex statements involving certain syntactic elements seems undeniable and important. In particular, it serves to isolate in language certain locutions with claim to be considered logical in their capacity to effect syntactic combinations of sentences into a further complex sentence such that knowing whether or not the component sentences are assented to, and nothing more, allows us to determine whether the complex one is or not. This is certainly logic in Quine's sense, constituting evaluation of truth "tied to the grammar and not to the lexicon" (pp. 101-102). But, as has already been argued, the devices so identified can only be shown to be intuitionistic rather than classical truth functions. What considerations settle such a dispute? The necessary move, at least to conduct it, is what Quine has termed semantic ascent. This is to shift from using a sentence to mentioning it in the attribution of truth. It allows us to express general principles of logic: e.g. "Every sentence of the form 'p or not p' is true" (p. 12). If we could actually assert this particular statement, that would settle the dispute in Quine's favor, but before considering what grounds could count for it, a few words on the character of semantic ascent. Semantic ascent allows us to make peculiarly
logical (general) statements, as opposed merely to establishing a particular statement by logical considerations. But while this move might appear to be effected by a shift from using language to talking about it, Quine is at pains to disallow any such considerations to count in favor of a linguistic doctrine of logical truth, in particular the claim that logical truths are about language. The point is evident with respect to individual statements, as the relevant instance of convention T makes clear: "by calling the sentence true, we call snow white." But such cases do not call for semantic assent anyway. Still, in the cases where we do need it "ascent to a linguistic plane of reference is only a momentary retreat from the world, for the utility of the truth predicate is precisely the cancellation of linguistic reference." In effect the condition that a truth predicate satisfy convention T shows it to constitute a "device of disquotation" so that "despite a technical ascent to talk of sentences, our eye is on the world" (loc. cit.). Much in all these claims seems to me unexceptionable, at least insofar as they combat a construal of logical truth as peculiarly linguistic. But I begin to grow uneasy that this will end up providing cover on a grab at 'something for nothing,' or for very little when we get to the issue of justifying certain statements which make essential use of semantic ascent, in particular principles of logic. "Every sentence of the form 'p or not p' is true" will turn essentially on considerations as to the notion of truth, and supposing truth merely a device for "disquotation" in the use of which we "keep our eye on the world" cannot do duty for more substantial consideration of the notion of truth itself. Now one might construe Quine as acceding to this viewpoint with his remark; "whatever sentences of a language to count
as logically true is determined, on this theory, when we have settled two things about the language, its grammar and its truth predicate" (p. 60 PL).

But fixing the truth predicate for a given language is in Quine's view precisely what Tarski has taught us how to do. And with Tarski, Quine supposes any such predicate to constitute an analysis of the classical notion of truth as it operates in that particular language. This is where Quine's notion of truth as disquotation and the return to reality would be invoked in the justification of logical principles. His discussion on p. 11 strongly suggests this viewpoint: "the truth predicate serves, as it were, to point through the sentence to the reality; it serves as a reminder that though sentences are mentioned, reality is still the whole point." But here we must remind ourselves of the arguments in Chapter 2 for the neutrality of the Tarskian analysis of truth as between realist and anti-realist conceptions of truth. The point there in short was that convention T, and also Tarski's specific constructions, are all intelligible and acceptable to the intuitionist - when interpreted by his lights. But the issue of whether every sentence of the form 'p or not p' is true (where behavioral criteria have picked out the words functioning as 'or' and 'not') is precisely a matter of which conception of truth proves operative. If we can frame a conception in which truth of a given sentence is a matter of how it is with the world solely, then evidently excluded middle holds: every statement of the form 'p or not p' is true. On the other hand, if the concept of truth is in some way relative to human actions, capacities, theorizings, perceptions or whatever, then we have no general grounds to suppose on the basis of that conception that every statement of the form 'p or not p' is true. Which is not of course to say that they are not true, but just they are not logically true. To establish a
particular statement of the form 'p or not p,' consideration will need be brought to bear taking account of particular features of p, or perhaps some more inclusive class of statements to which p belongs.

Now what sorts of consideration could count as establishing one conception of truth as against others? Whatever it is, will constitute evidence for the attendant logical theory. I have previously argued that Quine's thoroughly empiricist conception of language and his holistic picture of how observation, theory and meaning interact provides grounds to favor an anti-realist conception of truth. The point is that statements at a remove from the empirical fixity of observation statements are to be assessed as to truth or falsity only insofar as they figure within encompassing theory. Their truth or falsity is tied by both logical connection and pragmatic consideration to the correctness of the inclusive theory. And theories are underdetermined by fact. It need only be remembered then that theory is at least to the degree of empirical underdetermination man-made artifice, and Quinean holism is seen to embody anti-realist truth. This constitutes a particularly attractive account of such a conception through accommodating fully the objectivity of facts with relativity of truth to human determinability. What allows this welcome and not prima facie probable alliance is the grading of theoreticity. Further, as previously noted, the logic attendant upon this anti-realist conception of truth promises to be some form of intuitionism. And this fits with what can be demanded of the radical translation of sentential connectives using behavioral scrutability criteria.

Logic is to be taken as that body of truths endowed with maximal generality. Our theory of it is couched in terms of "some limited vocabulary appropriate
to the desired subject matter", in this case words like 'and', 'or', 'not', 'if ... than ....' - and crucially, 'is true'. The point that these words (or at least the former) will turn up in the formulation of any other theory I might wish to espouse is indicative of logic's generality. In keeping with the broadness of the subject matter, the considerations by which I may deem my theory on this subject established are likewise highly general: reflection on the character of theory, truth, language, in short, speculative metaphysics in Quine's sense. Nor is it to be claimed that with such maximal generality I have reached bedrock, on which I might firmly anchor the foundation of my entire conceptual edifice. For this appeal to philosophy is merely the appeal to another theory at an extreme grade of theoreticity, i.e. remoteness from the constraints of observation. It is Quine's theory, a theory which would persuade us of a commitment to thorough-going empiricism and draw the consequences of that for such very general issues as the character of language. Being persuaded of Quine's conception, this is the theory I appeal to in my claims as to what is logically true. The grounds for my logical theory then are endowed with self-evidence, but hardly, I think, obviousness - ready assent is not to be expected.

It appears I disagree with Quine as to logical theory; he affirms e.g. 'every statement of the form 'p or not p' is true' and I do not (though I do not deny it. I just lack any general reason for affirming 'p or not p' in advance of knowing at least something about p beyond merely that it is a statement). Our disagreement is evidently at a very high level of theoreticity, involving as it does essential appeal to the notion 'truth'. That this notion is essential to the disagreement is evidenced in the impossibility
already noted of drawing the contrast between classical and intuitionist logic purely in terms of observable assent and dissent with respect to particular statements. Semantic ascent is crucial to the discussion, and it is at the same time to draw inward from the observationally determinate boundary of total theory. Quine's notion of truth with respect to which he affirms excluded middle must be a highly theoretical one since no amount of querying the native on assent to particular statements will render a sufficient basis for affirming classical logic. The meaning of truth for Quine outruns its stimulus meaning. Consonant with Quine's thorough-going empiricism, I think we are called upon to operate with a notion of truth essentially anti-realist in character. To try getting beyond that notion constitutes an attempt to transcend the bounds of empiricism.

On the general account of interconnection between theory, observation and meaning, disagreement as to theory may optionally be smoothed out in favor of difference in meaning. Such is indeterminacy of translation, and it applies to this disagreement over theory as well as any. Indeed, perhaps it applies a little better, in the sense that thanks to some early work of Glivenko and Gödel I can actually go some way to specify the non-homophonic reinterpretation which in this case would support agreement as to theory. By Glivenko's result, if the intuitionist prefaces each principle of logic which the classical theorist upholds by a double negation, the resulting principle will be intuitionistically correct. This operation comes to a reconstrual of classical assertion. Consonance is achieved, though from a classical point of view what has been done to achieve it will seem not so much exceptionable as empty, since classically
of course double negation collapses into simple assertion, as a general principle of logic. Gödel's translation of classical logic into intuitionistically acceptable principles seems to provide more insight for the intuitionist as to what the classical logician means by his principles, and on the basis of which insight the intuitionist sees that he agrees with the classical logician in all he asserts. Conversely, the proposed translation is one that the classical logician is often disposed actually to adopt for himself, though the motive then is of course entirely different, principally economy as to primitive concepts. The theorem is this: if one translates the classical notions 'not p', 'if p then q', 'p or q', 'p and q' respectively by the intuitionist ones '¬p', '¬(p & ¬q)', '¬((¬p & ¬q)', 'p & q' then every principle of classical sentential logic is valid intuitionistically. As previous discussion makes clear, behavioral tests may well prove inconclusive as to whether a given alien community (e.g. Quine and his successful students in Phil. 140) adhere to a classical theory of logic and so disagree with the intuitionist on what the principles of logic are, or whether these connectives are to be translated non-homophonically, with the resulting principles affirmed by them consonant with construing them as intuitionists. It might even be relatively easy to reinterpret Quine's arguments via semantic ascent which ostensibly establish classical principles of logic, as favoring intuitionist ones - this in virtue of the neutrality of Tarski's truth predicate between realist/anti-realist conceptions of truth. Simplicity is probably still balanced on the side of interpreting Quine homophonically (e.g. when he says 'or' he means 'or') and attributing to him and his followers an explicable degree of illogicality - i.e. as unjustifiably affirming certain statements as logically true. My position then is that Quine, in the context of his general holistic conception
of theory and language, is wrong to affirm classical logic, correct in taking it that there is a logical theory to be affirmed, and insufficiently imbued with his own notion that logical truths are not true in some manner uniquely different from any other truths. Unless he is ready to allow that "the geology of two cultures will be incommensurable at worst and never in conflict, since conflict would simply discredit our translation" (cf. p. 96), he ought not to make that claim with respect to logic. The point is that in much weakened form, Quine affirms something like this principle for geology - but only to the extent that we have to trade off between discrediting a working translation and attributing false beliefs. What has led Quine to allow for logic such a difference in degree as to constitute what must appear a difference in kind is his supposition that the truths of logic are obvious, to an extreme degree unlike any truth of geology. That view is a mistake, compounded of confusing self-evidence with obviousness. With logic construed properly as a theory 'first among equals' (in a situation generally where some are more equal than others), the disagreement with linguistic accounts is sharply focused, and the character of logic revealed.

I want, finally, to conclude with two concessive comments. First, I have spoken through this discussion as if the non-classical system of logic to which Quine's conception commits him is intuitionist. But the arguments I have given are not equal to a conclusive point of such sharpness. One might even object that at most what has been shown is that the two valued classical logic won't fit, but all that leads to might be some finite multivalued logic. Such a system would be essentially different from intuitionist logic, and greatly preferred by Quine, it
appeared. Not to argue here the point in any detail, I think a reply to this doubt could be fashioned from the observation that finite multi-valued logics admit always of the distinction between designated and non-designated truth values, and every statement has one or the other. But this will still leave considerable indecision as to what might be the principles of a logic consonant with a thoroughly empiricist approach to language and attendant conception of truth. This leads to my second comment, for which I borrow (quite out of context) a phrase from Wittgenstein in a letter to Russell (22 June 1912): "Logic is still in the melting pot." Despite the extraordinary richness of work in mathematical logic over the past one hundred years, the philosophical question 'what is logic' seems to me, on the basis of preceding discussion, very much an open one. It is an issue intimately bound up with truth under its two aspects, realism vs. anti-realism, and the interconnection of meaning and truth.
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